[redacted]

PROCEEDINGS

OF THE

ROYAL SOCIETY OF LONDON.

From June 17, 1869, *to June* 16, 1870.

VOL. XVIII.

LONDON:
PRINTED BY TAYLOR AND FRANCIS,
RED LION COURT, FLEET STREET.
MDCCCLXX.

[redacted]

CONTENTS.

VOL. XVIII.

No. 114.—*June* 17, 1869.

[redacted]

January 6, 1870.

ERRATA.

Vol. xvi. page 346, line 8 from bottom, *for* instead of a depth *read* in seas of a depth.
„ xvii. „ 345, line 8 from bottom, *for* $\delta N = -157''\cdot156$ *read* $\delta N = +157''\cdot156$.
„ xviii. „ 207, line 10 from bottom, *before and after the words* "Stream-lines generated by a Sphere," *dele* the marks of quotation.

NOTICE TO BINDER.

Plate II. to face p. 352. Plate III. to face p. 359.

PROCEEDINGS

OF

THE ROYAL SOCIETY.

June 17, 1869 (continued).

X. "On a Group of Varieties of the Muscles of the Human Neck, Shoulder, and Chest, with their transitional Forms and Homologies in the Mammalia." By JOHN WOOD, F.R.C.S., Examiner in Anatomy at the University of London. Communicated by Dr. SHARPEY, Sec. R.S. Received June 17, 1869.

(Abstract.)

THE muscular varieties described by the author in his paper comprise the *occipito-scapular*, the *levator claviculæ*, and the *cleido-occipital*, among the muscles which elevate the scapulo-clavicular bone-arch; the *sterno-scapular*, the *sterno-clavicular*, and the *scapulo-clavicular*, of those which depress it; and the *supracostal*, placed upon the upper part of the thorax.

The human *occipito-scapular* was first observed and described by him in the Proceedings of the Royal Society in 1867. Since that time various developments of muscular slips connected with the *splenii*, *levator anguli scapulæ*, and *serrati* have been observed, and are described and figured as a series of varieties transitional from the *occipito-scapular* behind to the *levator claviculæ* in front of the neck. The homology of the *occipito-scapular* with the *levator scapulæ minor vel posterior* of Douglass, the *rhomboideus capitis*, *rhomboïde antérieur* of Meckel, and the *rhomboïde de la tête* of Cuvier, is traced in the different orders of the Mammalia, from direct observation, in the following animals, viz. the Bonnet-Monkey, the Hedgehog, Mole, Dog, Cat, Badger, Weasel, Rabbit, Guineapig, Norway Rat, and Squirrel, of which drawings from dissections accompanied the paper; and also from various authorities in reference to a considerable number of other animals.

The *levator claviculæ* he described in reference to its animal homologies

in his paper read before the Royal Society in 1864; he has found it in 6 out of 202 subjects. In the present paper the author gives an abstract of the observations of the older and modern anatomists referring to this muscle in the human subject under various names, and enters at length into its homologies in the Mammalia, as described by writers under its synonyms,—the *levator scapulæ major vel anterior* (Douglass), *omo- ou acromio-trachélien* (Cuvier and Meckel), *acromio-basilar* (Vicq d'Azyr), *basio-humeralis* (Krause), *Kopf-Arm-Muskel* (Peyer), *clavio-trachélien* (Church), *transverso-scapulaire* (Strauss-Dürckheim), *omo-atlanticus* (Haughton), and *cervico-humeral* (Humphry),—illustrating them by drawings from his own dissections. He enters more fully into the discussion of the apparently anomalous composition of the muscle in the Rabbit, gives reasons and comparative illustrations from the Fallow-deer and Ass for considering the seeming doubling of the muscle to result from a peculiar development of the *cleido-mastoid* in apparent conjunction with it, and considers that the muscle which has gone under the last name in the Rabbit to be really a development of the *cleido-occipital.*

The *cleido-occipital* he described in his paper published in the Proceedings of the Royal Society in June 1866; and he has found it since that time in 37 out of 102 subjects. In the present paper he quotes briefly the various anatomists who have described it as part of the *sterno-cleido-mastoid* or *trapezius,* and connects it homologically with the muscles which have been described in the claviculate mammalia as a second *cleido-mastoid,* and in the semiclaviculate as the *trapezius clavicularis* ("*portion cervicale*") of that muscle, giving illustrations of its gradual or transitional forms of development from specimens that have come under his own observation, or which have been gathered from the writings of others, as far as to the formation of the compound *cephalo-humeral* or *levator humeri* muscle of the Rodents and Carnivora.

The *sterno-scapular* muscle was first described as a variety in the human subject by the author in his paper published in the 'Proceedings' in 1865; it had been previously described by various anatomists and by himself as a double *subclavius,* with an insertion into the scapula. In the present paper he briefly quotes these authorities, and shows the various developments of the muscle in animals. In connexion with it he describes a *scapulo-clavicular* variety (first observed by him as a human variety in 1865), and compares it with the human abnormalities described by authors as varieties of the *omo-hyoid.* It is described by Cuvier as the "*scapulo-clavien*" in the Rat-mole of the Cape and in the *Didelphis marsupialis,* and has been found by the author in the Rabbit, Guineapig, Squirrel, and Norway Rat.

He also describes the specimens he has found of the *sterno-clavicular* muscle, mentions the observers who have before seen it and recognized its homologies, and gives illustrations of its formation in the Rabbit, Guinea-pig, and other animals.

The *supra-costal* muscle was first discovered and described and figured

by the author in his paper published in the 'Proceedings' in 1865, and was again noted and recorded by him in 1867; it has also been observed in the human subject by Professor Turner and others, and is considered by the former to be the representative of the *rectus thoracicus* of animals The author, however, is of opinion that the muscle figured by Cuvier as the *sterno-costal* in animals is a better fitting homology, and gives in this paper illustrations from his own dissections in animals in support of this view.

XI. "Results of the first year's performance of the Photographically Self-recording Meteorological Instruments at the Central Observatory of the British System of Meteorological Observations." By Lieut.-General EDWARD SABINE, R.A., President. Received June 17, 1869.

Before the Fellows of the Society disperse for the long vacation, I am desirous to bring under their notice the results of the first year's performance (January 1 to December 31, 1868) of the photographically self-recording meteorological instruments established at Kew, the Central Observatory of the British Meteorological System instituted by the Board of Trade and superintended by a Committee of Fellows of the Royal Society.

The photograms, with tabulations carefully prepared from them, are transmitted monthly by Mr. Stewart, the Superintendent of the Kew Observatory, to Mr. Scott, the Director of the Meteorological Office in London, where the results are computed and embodied in Tables, of the nature of those which are now presented.

The first of these Tables shows the *Diurnal Variation*, or the values of the phenomena at each of the 24 hours, on the mean of the year. It exhibits

1st. The Temperature.

2nd. The Elasticity of the Aqueous Vapour.

3rd. The Barometric Pressure.

4th. The Pressure of the Dry Air.

5th. The Humidity.

In meteorology and climatology much instruction may often be derived from tracing the modifying influences of diversities of situation; and I have thought that these Tables might be made more acceptable and interesting to the Society, and the subject be advantageously illustrated, by the addition of corresponding results for two other stations, which are very nearly in the same geographical latitude as Kew, but are very differently situated in other respects, being in the interior of the European and Asiatic continent—thoroughly continental therefore, and as such contrasted with our insular British stations. Nertchinsk and Barnaoul, both in Siberia, are two of the stations of the great Russian system of observatories, established by our

late Foreign Member, Mr. A. T. Kupffer, and ably superintended by him for several years until his decease. I had been assured by M. Kupffer that I might thoroughly rely on the observations made at these two stations; and I have since acquired experimentally the fullest confirmation of this assurance in the case of Nertchinsk (as regards the *magnetical*, and inferentially therefore also as regards the *meteorological* observations), by the very delicate and sufficient test adverted to in page 238 of Art. VI. in the Phil. Trans. for 1864. Barnaoul is in lat. 53° 20′, corresponding with the rough average of the latitudes of our British stations generally, and is 400 feet above the sea. Nertchinsk differs only 10′ from the latitude of Kew, but has otherwise a marked feature of diversity in being at an elevation of 2230 feet, whilst Kew is only 34 feet above the sea-level. At Kew we have only as yet available the records of a single year, necessarily influenced by the natural irregularities which cause one year to differ from another. These irregularities are lessened, in the case of the Siberian stations, by combining in the present paper the results of two years of observation.

I may now proceed to the Table of the Diurnal Variations, and to a brief notice of the most salient features presented by the comparative view of the phenomena of the three stations as shown in that Table.

In discussing the diurnal variations of the meteorological elements, it is customary to commence with the *temperature*, regarding it as in a great degree the governing agent in regulating the phenomena of those other elements which are the subjects of the photographical registration. In the middle latitudes, with which alone we have at present to deal, the diurnal variation of the temperature is recognized as a single progression, having one ascending and one descending branch, the turning-points being a maximum at an early hour in the afternoon, and a minimum at a little before sunrise. We find this to be the order of the phenomena at the three stations under review, viz. a maximum between 2 and 3 hours, and a minimum between 16 and 17 hours (4 and 5 A.M.), the *range* between the extremes presenting, however, very marked differences, being 10°·7 (Fahr.) at Kew, 14°·0 at Barnaoul, and 17°·0 at Nertchinsk.

It has been the practice for the last thirty years, at the principal European observatories, to regard the elastic force of the aqueous vapour as an important meteorological element, and to employ it in the separation of the barometric pressure into its two constituents, viz. the pressure of the dry air, and the elasticity of the aqueous vapour mingled therein*. In conformity with this practice, we may take the *vapour tension* next in the order of succession. It was remarked by Bessel, in the Astron. Nach. for 1838 (No. 356), that "since the invention of Daniell's hygrometer and August's psychrometer, we possess the means of ascertaining at all times with ease and sufficient exactness the quantity of aqueous vapour contained in the

* In the publications of the British Colonial Observatories (1840-1847) this method was adopted in the meteorological reductions, being one of its earliest applications.

TABLE I. Diurnal Variation of the Meteorological Elements, at Kew in England, and at Nertchinsk and Barnaoul in Siberia.

Hours of Mean Time.	KEW. Lat. 51° 29′ N., long. 349° 42′ E.; height 34 feet. Year 1868.					NERTCHINSK. Lat. 51° 19′ N., long. 119° 36′ E.; height 2230 feet. Years 1858 and 1859.					BARNAOUL. Lat. 53° 20′ N., long. 83° 57′ E.; height 400 feet. Years 1858 and 1859.					Hours of Mean Time.
	Thermometer, Fahr.	Vapour.	Barometer.	Dry air.	Humidity.	Thermometer, Fahr.	Vapour.	Barometer.	Dry air.	Humidity.	Thermometer, Fahr.	Vapour.	Barometer.	Dry air.	Humidity.	
	°	in.	29 ins. +	29 ins. +		°	in.	27 ins. +	27 ins. +		°	in.	29 ins. +	29 ins. +		
0.	56·2	·304	1·019	·715	68	33·5	·188	·833	·645	68	41·6	·221	·585	·364	71	0.
1.	57·2	·304	1·010	·706	66	34·9	·190	·823	·633	67	42·6	·221	·582	·361	70	1.
2.	57·4	·300	1·005	·705	65	35·6	·190	·817	·627	65	42·8	·220	·579	·359	68	2.
3.	57·7	·302	0·998	·696	64	35·5	·185	·812	·627	64	42·6	·218	·579	·361	67	3.
4.	57·2	·300	0·999	·699	65	34·4	·184	·811	·627	63	41·9	·217	·580	·363	69	4.
5.	56·6	·294	1·000	·706	67	32·7	·179	·815	·636	63	40·8	·214	·580	·366	71	5.
6.	55·2	·294	1·005	·711	71	30·4	·177	·818	·641	66	39·3	·211	·581	·370	72	6.
7.	54·0	·294	1·014	·720	73	28·0	·175	·828	·653	69	37·6	·210	·581	·371	75	7.
8.	52·3	·303	1·018	·715	77	26·1	·169	·836	·667	72	35·9	·209	·582	·373	78	8.
9.	51·1	·294	1·022	·728	80	24·6	·163	·840	·677	74	34·5	·205	·583	·378	80	9.
10.	50·1	·290	1·025	·735	82	23·5	·157	·842	·685	75	33·4	·302	·583	·381	82	10.
11.	49·4	·290	1·024	·734	83	22·3	·155	·842	·687	76	32·4	·199	·582	·383	83	11.
12.	48·6	·292	1·025	·733	85	21·7	·151	·841	·690	76	31·0	·197	·580	·383	85	12.
13.	48·1	·292	1·021	·729	86	20·9	·149	·840	·691	77	30·7	·193	·579	·386	85	13.
14.	47·6	·290	1·019	·729	86	20·2	·146	·837	·691	78	30·1	·190	·577	·387	87	14.
15.	47·3	·288	1·015	·727	87	19·5	·144	·838	·694	79	29·3	·187	·577	·390	87	15.
16.	47·0	·286	1·015	·729	87	18·8	·142	·838	·696	79	28·8	·186	·576	·390	89	16.
17.	47·1	·288	1·011	·723	88	18·6	·144	·839	·695	80	29·0	·188	·577	·389	88	17.
18.	47·5	·294	1·012	·718	88	19·1	·150	·842	·692	80	30·0	·195	·580	·385	87	18.
19.	48·5	·298	1·020	·722	87	20·5	·158	·847	·689	79	31·3	·202	·583	·381	85	19.
20.	50·0	·302	1·025	·723	84	23·2	·170	·849	·679	77	32·9	·209	·586	·377	83	20.
21.	51·8	·316	1·026	·710	80	26·3	·178	·851	·673	75	35·6	·216	·589	·373	80	21.
22.	53·5	·314	1·023	·709	75	29·4	·184	·849	·665	73	38·2	·220	·591	·371	76	22.
23.	55·1	·314	1·021	·707	73	31·7	·188	·843	·655	70	40·0	·221	·590	·369	73	23.
Means	51·9	·298	30·016	29·718	78	26·3	·168	27·835	27·667	73	35·5	·206	29·582	29·376	78·7	Means

atmosphere." The most convenient mode of *photographic* investigation and record which presented itself, and was adopted at Kew, was by the employment of wet and dry thermometers; the difference between the two thermometers admits of exact measurement, and supplies the element which is desired, the accuracy of the record being occasionally tested by comparison with the results obtained by Regnault's "hygromètre à condensation"*. The gain of even two years of observation over a single year may be here at once seen by the greater regularity of the two years' record at the Siberian stations. Taking these therefore in the first instance, we find that at both stations the elasticity of the vapour presents a single progression, having maxima about noon, and minima at 16 hours (4 A.M.). The difference in the amount of vapour at the two stations is due, of course, to the greater altitude of Nertchinsk. At Kew the progression is not quite so regular as where two years are combined; the values at 21, 22, and 23 hours are high in comparison with the other hours, possibly owing to peculiarities in the weather of the particular year; in other respects the progression is similar to that at Nertchinsk and Barnaoul, and the time of minimum is identical at the three stations, viz. at 16 hours. The higher elasticity of the vapour at Kew, in comparison with the two Siberian stations, is, of course, due to the higher temperature at Kew†.

In the case of the *Barometer* there are slight indications at each of the three stations of the existence of a double progression; but in the middle latitudes a longer series of observation is clearly required to determine regular periods (if such there are) in a satisfactory manner. One conclusion is obvious, that in the latitudes of 51° and 53° the striking regularity and magnitude of the double period which prevail in the tropics do not subsist.

The minimum of the *dry air* coincides at the three stations, as nearly as may be, with the warmest hour of the day (2 or 3 hours). There is also, at each of the three stations, an approximate maximum at or near the coldest hour. At Barnaoul and Nertchinsk the progression between the hours of minimum and maximum is uninterrupted; at Kew it is obvious that a single year is not sufficient to justify conclusions in this respect.

Regarding the *Humidity*, the minimum, or dryest hour of the 24, is in all cases coincident with, or closely following upon, the warmest hour; and the hour of greatest humidity that of the lowest temperature. Kew

* There have been some few occasions in this, the first year at Kew, when the continuity of the trace from the wet thermometer failed, in consequence of the freezing of the water by which its ball was wetted, or owing to other causes. Arrangements have now been made to meet these difficulties in continuous registration.

† The Tables employed in the calculation of the values inserted in the columns of "Elastic Force of Vapour" and "Humidity" have been the well-known Russian Tables, 'Tables Psychrométriques et Barométriques à l'usage des Observatoires Météorologiques de l'Empire de Russie.' Very convenient Tables have also been published by the Smithsonian Institution, computed by Dr. Guyot. Two of the three stations of the present paper being Russian, it was deemed advisable to employ the Russian 'Tables Psychrométriques, &c.' for the reduction of the results in the present paper.

and Barnaoul have, on the mean, almost exactly the same degree of humidity, the greater amount of vapour at Kew being balanced, in its influence on the humidity, by the higher temperature. Nertchinsk is both the coldest and the driest.

So far as the purposes of the Meteorological Committee can yet be considered as settled, it is their intention to combine the results of every five years of observation into a Table of Diurnal Variations, similar to that which is now presented for Kew for a single year. A second period of five years will yield a second Table; and two such combined will form a ten-year Table, more satisfactory than either of its two component parts, but still open to correction by incorporation with subsequent periods of equal duration.

The other six observatories of the system established by the British Government, viz. Aberdeen, Armagh, Falmouth, Glasgow, Stonyhurst, and Valencia*, have received their instruments, which had been prepared and verified at the Central Observatory (Kew), where also those who were to work with them had received personal instruction in their use; and on the completion of these and all other needful arrangements, the six observatories commenced on July 1, 1868, a continuous record corresponding in all respects to that at Kew. The photograms and the tabulations prepared from them at the several observatories are transmitted monthly to Kew, where they undergo careful examination, and revision if required; and at the expiration of a second month they are sent, with the records prepared at Kew itself, to the Meteorological Office, where, under the direction of Mr. Scott, they are formed into Tables, and used for all meteorological purposes for which they may be available. The mode and extent in which the information thus obtained may be most suitably communicated to the public are not yet fully determined, but are receiving careful consideration.

Table II. (which occupies the next 5 or 6 pages) exhibits the *annual* variations at the three stations, analogous to the *diurnal* variations shown in Table I. It is obvious that such Tables cannot but assist greatly in studying the climatological phenomena in different localities; but a discussion of them would be premature until a wider observational basis is provided.

* It was the purpose of the Committee, approved by the Board of Trade, that there should have been an eighth meteorological station, viz. one in the north of Scotland. In the first estimate sent to the Treasury by the Board of Trade, the necessary cost of such a station was included; but on the receipt of a letter from the Treasury to the Board of Trade, June 5, 1867, stating that "in the estimates for the current year My Lords are aware that they have proposed a less sum than had been estimated for, and intend that the arrangements to be made by the Committee should be curtailed accordingly," the meteorological station in the north of Scotland was in consequence curtailed.

	KEW.—Temperature, Fahrenheit.												NERTCHINSK.—					
Hours of mean time.	January.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	October.	Nov.	Dec.	January.	Feb.	March.	April.	May.	June.
	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°
0.	39·6	47·0	48·6	53·6	63·7	68·0	72·4	68·9	65·6	53·0	45·1	48·4	−11·5	− 2·1	17·1	41·1	56·8	66·0
1.	40·0	47·8	49·4	54·6	65·1	69·1	74·4	69·8	66·9	54·3	45·6	49·0	−10·2	− 0·3	19·2	42·2	57·7	66·9
2.	40·1	48·0	49·8	54·8	65·6	69·6	75·0	69·6	67·3	54·5	45·9	49·1	−10·2	+ 0·5	20·8	42·5	58·5	67·6
3.	40·0	48·3	49·8	54·9	66·0	70·6	75·9	70·0	67·8	54·3	45·6	49·0	−11·2	+ 0·7	21·2	42·9	58·7	67·2
4.	39·6	47·5	49·3	54·7	65·7	70·4	76·5	69·7	66·9	53·4	44·7	48·0	−13·5	− 1·0	20·7	42·9	58·6	66·7
5.	39·2	46·4	48·8	53·5	65·8	70·6	76·1	68·9	65·9	52·2	43·9	47·7	−16·5	− 4·5	19·0	41·8	58·1	65·7
6.	39·0	44·9	47·3	52·4	63·8	70·0	74·6	67·2	63·3	50·6	43·1	46·8	−17·7	− 7·3	15·1	40·0	56·2	64·4
7.	38·9	44·0	45·7	50·7	61·7	68·5	72·5	65·3	60·8	50·3	42·6	46·6	−18·4	− 8·9	11·7	36·8	53·1	62·3
8.	38·7	43·3	45·0	48·9	58·3	64·8	69·2	63·3	59·3	48·2	42·1	46·4	−18·6	− 9·6	10·3	34·3	49·7	59·3
9.	38·4	42·8	44·3	47·6	56·3	62·2	66·7	62·0	58·1	47·3	41·8	46·2	−18·7	−10·1	9·2	32·7	46·9	56·2
10.	38·2	42·3	43·5	46·6	54·6	60·0	64·8	60·9	56·9	46·3	41·1	46·0	−19·3	−10·6	8·6	31·4	45·9	54·5
11.	38·2	42·2	43·1	45·6	53·4	58·5	63·4	60·3	55·9	45·6	40·7	45·7	−19·8	−11·3	7·8	30·2	42·3	53·1
12.	37·9	41·9	42·2	44·7	51·8	57·0	62·0	59·4	55·0	45·0	40·6	45·3	−20·1	−11·8	6·8	29·2	42·0	51·9
13.	37·8	42·0	41·8	44·4	50·9	55·8	60·5	59·0	54·3	44·6	40·5	45·4	−20·4	−12·6	6·0	28·6	40·8	51·0
14.	37·8	41·9	41·0	44·1	50·1	54·6	59·3	58·5	53·8	44·3	40·5	45·3	−20·8	−13·6	5·0	28·0	39·8	50·1
15.	37·7	41·8	40·9	44·2	49·6	53·7	58·5	58·1	53·4	44·2	39·8	45·3	−21·4	−14·3	3·8	27·2	38·9	49·3
16.	37·6	41·8	40·6	43·7	49·2	52·9	57·9	57·5	53·1	44·2	40·0	45·2	−21·9	−14·9	2·8	26·5	38·0	48·9
17.	37·5	41·5	40·6	43·5	49·6	53·7	58·2	57·5	52·9	44·3	40·2	45·2	−22·2	−15·5	1·9	26·2	38·4	49·7
18.	37·3	41·2	40·0	43·7	51·5	55·8	59·8	58·3	53·1	44·2	40·1	45·2	−22·6	−16·0	1·3	27·1	40·6	52·2
19.	37·5	41·1	40·3	44·7	53·5	58·8	62·4	60·2	53·9	44·4	40·1	44·9	−22·7	−16·4	3·1	29·8	43·7	54·6
20.	37·9	41·3	41·8	46·9	56·2	60·9	64·8	62·5	56·4	45·8	40·6	45·1	−22·6	−14·0	7·2	33·2	48·1	58·6
21.	38·3	42·2	44·1	49·3	59·0	63·1	67·4	64·2	58·9	47·7	41·4	45·5	−20·3	− 9·4	10·9	36·0	51·3	60·7
22.	38·9	43·9	46·1	50·7	61·4	64·8	69·9	65·8	61·5	49·7	42·6	46·5	−16·5	− 5·6	13·6	38·3	53·8	63·4
23.	39·7	45·5	47·7	52·6	63·1	67·1	71·3	67·7	63·7	51·8	43·9	47·4	−13·8	− 4·4	16·4	40·0	55·8	64·6
Means	38·6	43·8	44·7	48·8	57·7	62·5	67·2	63·5	59·4	48·3	42·2	46·5	−18·0	− 8·9	10·8	34·5	48·9	58·5

	KEW.—Tension of Vapour.												NERTCHINSK.—					
	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.
0.	·204	·228	·224	·254	·358	·354	·404	·430	·382	·286	·238	·282	·027	·041	·078	·141	·197	·354
1.	·206	·226	·226	·256	·360	·360	·408	·428	·388	·284	·231	·280	·029	·043	·081	·139	·194	·353
2.	·204	·222	·226	·252	·350	·366	·390	·426	·386	·280	·226	·280	·029	·044	·084	·141	·193	·355
3.	·204	·218	·228	·250	·350	·360	·400	·426	·384	·284	·233	·286	·027	·039	·087	·136	·194	·348
4.	·214	·218	·226	·252	·346	·362	·394	·410	·394	·296	·224	·276	·023	·039	·083	·137	·187	·364
5.	·210	·214	·230	·248	·354	·366	·404	·420	·376	·300	·224	·274	·019	·035	·077	·135	·185	·348
6.	·212	·216	·234	·254	·340	·366	·414	·426	·392	·294	·224	·274	·018	·029	·070	·136	·183	·342
7.	·216	·218	·230	·250	·342	·376	·422	·420	·384	·276	·224	·274	·018	·028	·062	·133	·185	·352
8.	·206	·214	·232	·256	·338	·370	·416	·424	·389	·290	·222	·272	·018	·028	·060	·125	·185	·345
9.	·208	·220	·234	·258	·342	·374	·414	·420	·384	·280	·220	·272	·017	·027	·060	·125	·178	·326
10.	·202	·220	·226	·248	·338	·372	·408	·420	·382	·274	·218	·266	·017	·027	·058	·122	·174	·312
11.	·200	·220	·228	·248	·332	·370	·408	·422	·374	·272	·214	·264	·017	·025	·057	·120	·172	·307
12.	·198	·220	·226	·248	·328	·362	·396	·418	·370	·270	·212	·260	·016	·025	·055	·116	·171	·302
13.	·198	·218	·222	·254	·322	·374	·394	·430	·360	·264	·212	·260	·017	·024	·056	·117	·167	·296
14.	·198	·220	·222	·250	·320	·356	·389	·422	·360	·262	·212	·256	·017	·023	·053	·115	·163	·292
15.	·194	·218	·224	·246	·312	·348	·387	·422	·356	·262	·216	·256	·016	·022	·052	·113	·163	·287
16.	·192	·216	·220	·252	·308	·342	·390	·418	·356	·262	·218	·256	·016	·022	·050	·111	·161	·288
17.	·196	·220	·224	·252	·316	·352	·386	·418	·354	·266	·214	·256	·015	·020	·048	·112	·167	·296
18.	·200	·218	·228	·248	·330	·360	·402	·424	·354	·278	·214	·256	·015	·021	·048	·116	·176	·318
19.	·200	·218	·230	·258	·352	·362	·416	·434	·372	·278	·222	·262	·015	·020	·052	·125	·186	·332
20.	·200	·232	·242	·260	·354	·362	·428	·434	·382	·289	·212	·260	·016	·024	·062	·130	·191	·348
21.	·206	·238	·226	·264	·358	·356	·436	·428	·418	·296	·220	·262	·017	·029	·069	·135	·199	·357
22.	·204	·230	·228	·256	·354	·350	·432	·430	·396	·292	·230	·270	·022	·038	·073	·138	·200	·361
23.	·214	·260	·228	·258	·366	·364	·418	·438	·402	·294	·244	·276	·024	·039	·077	·139	·201	·357
Means	·204	·223	·228	·253	·341	·362	·407	·424	·379	·280	·222	·268	·019	·030	·065	·127	·182	·331

[redacted]

Temperature, Fahrenheit. | BARNAOUL.—Temperature, Fahrenheit.

July.	August.	Sept.	October.	Nov.	Dec.	January.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	October.	Nov.	Dec.	Hours of mean time.
°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	
72·3	70·2	56·7	37·0	12·9	−14·3	8·1	8·8	22·5	48·1	60·6	71·1	75·0	70·7	55·1	43·5	28·8	6·9	0.
73·4	71·6	58·2	38·9	14·2	−13·0	8·8	10·3	24·2	48·9	61·8	72·0	75·8	71·2	55·9	44·6	29·4	7·5	1.
73·9	72·1	59·2	39·5	14·6	−12·4	9·0	10·5	25·2	49·5	62·1	72·1	76·1	71·1	56·2	44·8	29·4	7·5	2.
74·0	71·8	60·6	39·5	14·0	−13·4	8·4	10·4	25·6	49·8	62·1	71·6	74·8	71·2	56·1	44·9	29·2	6·9	3.
73·5	71·6	59·2	38·1	12·0	−15·9	6·9	9·5	25·1	49·6	61·8	71·5	75·5	71·0	55·3	44·0	27·8	6·0	4.
72·0	70·2	57·7	35·7	9·7	−18·2	5·9	7·9	23·5	48·5	61·1	70·8	73·8	69·8	53·9	41·8	26·8	5·5	5.
70·6	68·0	54·2	32·2	8·0	−19·0	5·1	6·7	21·0	46·3	59·4	69·6	72·2	67·8	51·9	40·8	26·0	5·1	6.
67·9	64·4	49·6	29·9	7·2	−19·7	4·4	5·6	19·2	43·6	57·0	67·9	70·1	65·0	49·7	39·2	25·4	4·6	7.
64·7	60·8	47·4	28·2	6·8	−20·5	3·8	4·5	17·8	41·7	53·7	64·8	67·5	62·2	47·8	38·1	25·0	4·1	8.
61·8	58·2	45·4	27·3	6·5	−20·8	2·9	3·5	16·3	40·2	51·6	61·9	65·1	60·1	46·6	37·0	24·7	3·5	9.
59·4	56·8	44·6	26·5	6·0	−21·2	2·7	3·0	15·4	38·8	49·8	59·8	63·2	58·5	45·6	36·0	24·5	3·2	10.
57·8	55·6	43·7	25·6	5·2	−21·6	2·2	2·3	14·2	37·9	48·2	58·1	61·6	57·2	44·8	35·3	24·2	3·1	11.
57·0	54·1	42·9	25·2	4·8	−21·9	1·9	1·6	13·2	37·0	46·8	56·8	60·2	56·1	44·0	34·6	24·1	2·9	12.
56·2	53·1	41·7	24·4	4·2	−22·1	1·7	1·0	12·1	35·7	45·3	55·4	59·0	55·2	43·1	33·9	23·9	2·6	13.
55·4	52·1	41·1	23·8	3·6	−22·2	1·7	0·5	11·1	34·9	44·2	54·3	58·3	54·3	42·6	33·5	23·5	2·3	14.
54·6	51·4	40·1	22·8	3·3	−22·4	1·7	−0·1	10·0	34·1	43·2	53·1	57·1	53·2	41·9	32·7	23·0	2·3	15.
54·0	50·6	39·4	22·0	3·1	−22·6	1·6	−0·7	9·1	33·6	42·2	52·5	56·5	52·5	41·2	32·3	22·8	2·2	16.
55·0	50·3	38·8	21·1	2·5	−22·9	1·6	−0·9	8·3	33·4	44·2	54·4	57·8	52·4	40·6	31·8	22·4	2·2	17.
56·9	51·4	38·8	20·7	2·0	−23·4	1·7	−1·1	8·0	35·4	47·5	57·5	60·7	53·7	40·7	31·8	22·1	2·2	18.
59·5	53·9	40·4	20·9	1·9	−23·2	1·7	−1·3	9·1	37·3	50·0	60·5	63·5	56·1	42·7	32·1	21·8	1·5	19.
63·5	58·4	44·7	23·8	2·2	−23·2	1·4	−0·9	11·2	39·8	53·0	63·1	66·1	58·8	45·0	33·9	22·2	1·4	20.
66·5	62·3	48·2	27·6	4·2	−22·2	2·5	1·4	15·7	43·0	56·1	66·3	69·4	62·8	48·2	36·2	23·5	1·7	21.
68·8	65·3	52·4	31·2	7·5	−18·9	4·4	4·4	19·2	45·6	58·3	68·7	72·1	66·2	51·0	39·1	26·2	3·1	22.
71·1	68·2	53·9	34·1	10·0	−15·9	6·5	7·2	21·4	47·3	59·6	70·0	72·8	68·9	53·6	41·1	26·9	4·6	23.
64·2	60·9	48·3	29·0	6·9	−19·6	4·0	3·9	16·6	41·7	53·3	63·5	66·8	61·9	48·1	37·6	25·2	3·9	Means

Tension of Vapour. | BARNAOUL.—Tension of Vapour.

in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	
·481	·436	·275	·132	·073	·024	·070	·074	·113	·181	·244	·389	·477	·425	·272	·191	·140	·071	0.
·474	·448	·275	·141	·074	·027	·073	·076	·118	·182	·243	·387	·482	·419	·275	·192	·137	·072	1.
·471	·441	·277	·142	·074	·025	·073	·076	·120	·182	·240	·386	·479	·412	·273	·188	·134	·072	2.
·464	·430	·266	·141	·072	·023	·070	·076	·122	·178	·243	·390	·476	·407	·272	·186	·133	·071	3.
·461	·424	·266	·137	·065	·019	·065	·073	·120	·178	·238	·390	·474	·406	·269	·185	·131	·068	4.
·455	·424	·260	·132	·059	·018	·064	·068	·114	·178	·240	·384	·473	·402	·264	·180	·128	·067	5.
·459	·427	·261	·125	·054	·017	·062	·066	·105	·175	·236	·381	·472	·410	·263	·176	·126	·066	6.
·461	·421	·252	·118	·052	·016	·061	·063	·099	·175	·242	·389	·471	·407	·259	·169	·124	·065	7.
·449	·399	·243	·114	·051	·015	·060	·062	·095	·171	·238	·390	·475	·403	·253	·169	·124	·064	8.
·422	·379	·238	·112	·051	·016	·059	·060	·091	·170	·235	·382	·470	·397	·248	·166	·125	·062	9.
·406	·367	·233	·108	·051	·016	·058	·059	·090	·166	·234	·378	·461	·388	·243	·164	·124	·062	10.
·393	·359	·228	·110	·050	·014	·057	·058	·086	·165	·231	·369	·453	·380	·242	·163	·124	·062	11.
·385	·347	·222	·107	·049	·015	·057	·058	·084	·164	·230	·362	·443	·375	·239	·160	·124	·062	12.
·385	·340	·222	·106	·048	·015	·057	·057	·083	·161	·228	·353	·431	·368	·238	·158	·123	·062	13.
·380	·335	·216	·102	·046	·014	·058	·055	·080	·159	·224	·344	·425	·363	·235	·156	·122	·062	14.
·372	·331	·209	·100	·045	·014	·058	·053	·078	·158	·219	·336	·418	·356	·231	·154	·122	·061	15.
·369	·327	·205	·100	·046	·015	·058	·054	·077	·157	·220	·334	·416	·352	·226	·153	·122	·061	16.
·384	·327	·203	·096	·043	·015	·059	·054	·075	·160	·230	·347	·424	·353	·225	·152	·120	·061	17.
·403	·340	·205	·093	·044	·014	·059	·054	·075	·164	·246	·371	·451	·366	·225	·150	·119	·061	18.
·433	·367	·216	·098	·044	·014	·059	·054	·077	·172	·256	·385	·475	·380	·235	·152	·119	·060	19.
·459	·405	·240	·107	·046	·013	·060	·054	·086	·180	·262	·399	·486	·400	·245	·158	·120	·060	20.
·467	·430	·249	·114	·051	·016	·061	·059	·095	·182	·262	·401	·492	·417	·263	·168	·124	·061	21.
·477	·445	·259	·125	·059	·019	·064	·065	·103	·183	·259	·407	·490	·431	·267	·178	·130	·063	22.
·482	·449	·268	·129	·065	·022	·067	·071	·109	·182	·250	·400	·488	·424	·271	·186	·133	·066	23.
·433	·392	·241	·116	·055	·017	·062	·062	·096	·172	·240	·377	·463	·393	·251	·169	·126	·064	Means

[redacted]

KEW.—Atmospheric Pressure at 62° Fahr. | NERTCHINSK.—

Hours of mean time.	January.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	October.	Nov.	Dec.	January.	Feb.	March.	April.	May.
	ins. 29+	ins. 29+	ins. 29+	ins. 29+	ins. 29+	ins. 29+	ins. 29+	ins. 29+	ins. 29+	ins. 29+	ins. 29+	ins. 29+	ins. 27+	ins. 27+	ins. 27+	ins. 27+	ins. 27+
0.	·986	1·212	1·061	1·018	1·067	1·207	1·098	·965	·917	1·028	1·070	·605	·977	·992	1·021	·760	·654
1.	·967	1·202	1·056	1·013	1·061	1·198	1·086	·961	·908	1·021	1·062	·586	·969	·980	1·014	·751	·644
2.	·976	1·196	1·051	1·009	1·053	1·194	1·079	·948	·900	1·012	1·058	·583	·965	·972	1·004	·740	·633
3.	·956	1·194	1·046	1·001	1·047	1·188	1·067	·944	·891	1·005	1·057	·583	·973	·972	·997	·734	·626
4.	·963	1·195	1·046	·997	1·043	1·180	1·075	·941	·887	1·009	1·059	·591	·981	·975	·995	·728	·619
5.	·958	1·193	1·045	·998	1·042	1·178	1·081	·939	·889	1·015	1·069	·597	·990	·983	·997	·730	·617
6.	·960	1·191	1·052	·999	1·045	1·180	1·088	·942	·897	1·024	1·080	·603	·993	·992	1·004	·734	·623
7.	·964	1·208	1·061	1·032	1·051	1·184	1·092	·948	·907	1·028	1·084	·605	·997	·998	1·013	·746	·631
8.	·971	1·209	1·066	1·016	1·059	1·192	1·104	·961	·919	1·035	1·081	·605	·999	1·002	1·023	·758	·646
9.	·975	1·209	1·066	1·021	1·071	1·205	1·121	·968	·913	1·038	1·075	·606	·999	1·002	1·026	·766	·661
10.	·975	1·207	1·068	1·022	1·076	1·211	1·124	·974	·915	1·040	1·082	·609	·998	1·000	1·027	·764	·669
11.	·976	1·212	1·061	1·020	1·079	1·214	1·128	·972	·901	1·037	1·071	·621	·993	·998	1·024	·764	·672
12.	·972	1·208	1·065	1·031	1·079	1·216	1·129	·976	·898	1·037	1·069	·623	·987	·997	1·026	·768	·672
13.	·969	1·205	1·061	1·009	1·079	1·214	1·132	·971	·896	1·037	1·061	·619	·983	·992	1·027	·763	·672
14.	·970	1·206	1·057	1·003	1·074	1·213	1·128	·968	·892	1·032	1·061	·622	·985	·991	1·024	·760	·671
15.	·969	1·199	1·048	·998	1·069	1·212	1·125	·961	·886	1·026	1·052	·619	·988	·988	1·024	·758	·671
16.	·967	1·201	1·046	·993	1·069	1·213	1·126	·959	·882	1·027	1·049	·617	·984	·987	1·026	·758	·671
17.	·959	1·202	1·048	·991	1·072	1·216	1·128	·958	·871	1·031	1·046	·612	·979	·984	1·029	·763	·675
18.	·955	1·205	1·059	·994	1·077	1·222	1·132	·963	·839	1·034	1·048	·613	·979	·985	1·033	·765	·681
19.	·955	1·209	1·066	1·004	1·082	1·227	1·137	·967	·882	1·044	1·052	·612	·985	·990	1·044	·769	·685
20.	·960	1·222	1·075	1·010	1·083	1·231	1·137	·965	·885	1·054	1·061	·617	·990	·997	1·049	·769	·687
21.	·970	1·229	1·078	1·012	1·077	1·229	1·130	·964	·887	1·057	1·063	·622	·996	·997	1·049	·765	·681
22.	·970	1·236	1·082	1·009	1·036	1·225	1·104	·966	·889	1·055	1·073	·626	·997	·996	1·048	·762	·686
23.	·946	1·239	1·084	1·012	1·066	1·210	1·104	·970	·884	1·053	1·063	·617	·992	·991	1·041	·756	·670
Means	·966	1·208	1·061	1·008	1·065	1·207	1·111	·960	·895	1·032	1·064	·609	·987	·990	1·024	·755	·659

KEW.—Pressure of Dry Air at 62° Fahr. | NERTCHINSK.—

Hours of mean time.	January.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	October.	Nov.	Dec.	January.	Feb.	March.	April.	May.
	ins. 29+	ins. 29+	ins. 29+	ins. 29+	ins. 29+	ins. 29+	ins. 29+	ins. 29+	ins. 29+	ins. 29+	ins. 29+	ins. 29+	ins. 27+	ins. 27+	ins. 27+	ins. 27+	ins. 27+
0.	·782	·984	·837	·764	·709	·853	·694	·535	·535	·742	·832	·323	·949	·950	·946	·619	·457
1.	·761	·976	·830	·757	·701	·838	·678	·533	·520	·737	·832	·306	·940	·937	·933	·611	·449
2.	·772	·974	·825	·757	·703	·828	·689	·522	·514	·732	·832	·303	·936	·929	·919	·599	·440
3.	·752	·976	·818	·751	·689	·828	·667	·514	·507	·721	·825	·297	·946	·933	·910	·597	·431
4.	·759	·977	·820	·745	·697	·818	·681	·531	·493	·713	·835	·315	·958	·935	·911	·592	·433
5.	·748	·979	·815	·750	·688	·812	·677	·519	·513	·715	·845	·321	·970	·949	·920	·595	·433
6.	·748	·975	·828	·745	·705	·814	·674	·516	·505	·730	·856	·329	·974	·961	·934	·597	·440
7.	·748	·990	·831	·782	·709	·808	·670	·528	·523	·752	·860	·331	·977	·971	·951	·613	·444
8.	·765	·995	·834	·760	·731	·822	·688	·537	·521	·745	·859	·333	·982	·974	·963	·633	·461
9.	·767	·989	·832	·763	·729	·831	·707	·548	·529	·758	·855	·334	·982	·974	·965	·640	·483
10.	·773	·987	·842	·774	·738	·839	·716	·554	·533	·766	·864	·343	·980	·974	·969	·642	·494
11.	·776	·992	·833	·772	·747	·844	·720	·550	·527	·765	·857	·347	·975	·972	·968	·645	·500
12.	·774	·988	·839	·783	·751	·854	·733	·558	·528	·767	·857	·363	·971	·971	·970	·651	·500
13.	·771	·987	·839	·755	·755	·840	·738	·541	·536	·773	·849	·359	·966	·969	·971	·645	·505
14.	·772	·986	·835	·753	·754	·857	·730	·546	·522	·770	·849	·366	·968	·967	·971	·645	·508
15.	·775	·981	·824	·752	·767	·864	·729	·539	·530	·764	·836	·363	·971	·966	·972	·645	·507
16.	·775	·985	·826	·741	·761	·871	·736	·541	·531	·765	·831	·361	·968	·965	·976	·646	·510
17.	·763	·982	·824	·739	·756	·864	·742	·540	·517	·765	·832	·356	·964	·964	·980	·652	·508
18.	·745	·987	·831	·746	·747	·862	·730	·539	·485	·756	·834	·357	·964	·965	·986	·648	·506
19.	·755	·991	·836	·746	·730	·865	·721	·533	·510	·766	·830	·350	·970	·968	1·017	·644	·499
20.	·760	·990	·833	·750	·729	·869	·709	·531	·503	·765	·849	·357	·974	·974	·986	·639	·496
21.	·764	·991	·852	·748	·719	·873	·694	·536	·469	·761	·843	·360	·979	·968	·981	·630	·482
22.	·766	1·006	·854	·753	·682	·875	·672	·536	·493	·763	·843	·356	·975	·957	·975	·624	·476
[illegible]	·732	·979	·856	·754	·700	·846	·686	·532	·482	·759	·819	·341	·967	·952	·963	·616	·469
[illegible]	·762	·986	·833	·756	·725	·845	·703	·536	·516	·752	·842	·341	·967	·960	·960	·628	·476

Atmospheric Pressure.							BARNAOUL.—Atmospheric Pressure.												
June.	July.	August.	Sept.	October.	Nov.	Dec.	January.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	October.	Nov.	Dec.	Hours o mean time.
ins. 27+	ins. 27+	ins. 27+	ins. 27+	ins. 27+	ins. 27+	ins. 27+	ins. 29+	ins. 29+	ins. 29+	ins. 29+	ins. 29+	ins. 29+	ins. 29+	ins. 29+	ins. 29+	ins. 29+	ins. 29+	ins. 29+	
·635	·680	·748	·825	·890	·885	·925	·802	·862	·820	·677	·441	·306	·198	·266	·453	·640	·665	·899	0.
·624	·671	·737	·827	·878	·877	·910	·803	·859	·816	·671	·432	·302	·194	·264	·450	·633	·663	·894	1.
·615	·662	·728	·829	·871	·874	·916	·805	·857	·811	·666	·426	·294	·191	·261	·450	·630	·663	·894	2.
·611	·654	·719	·798	·867	·875	·920	·810	·859	·807	·663	·422	·292	·189	·261	·450	·628	·666	·897	3.
·609	·651	·716	·793	·869	·871	·927	·814	·859	·807	·661	·418	·289	·185	·262	·449	·632	·677	·899	4.
·608	·652	·717	·795	·867	·886	·934	·818	·860	·806	·659	·413	·291	·185	·262	·449	·635	·680	·901	5.
·614	·657	·721	·801	·873	·881	·937	·816	·860	·805	·655	·413	·291	·187	·263	·450	·642	·687	·903	6.
·620	·673	·727	·813	·883	·894	·943	·817	·858	·805	·653	·414	·290	·192	·264	·451	·647	·683	·904	7.
·631	·675	·740	·823	·888	·899	·944	·817	·858	·806	·654	·416	·291	·193	·262	·451	·650	·681	·902	8.
·642	·686	·748	·830	·890	·900	·942	·815	·857	·806	·657	·419	·291	·198	·265	·453	·655	·675	·900	9.
·644	·693	·751	·831	·893	·897	·942	·813	·853	·803	·657	·420	·293	·201	·267	·452	·657	·673	·900	10.
·648	·695	·752	·834	·891	·896	·941	·811	·851	·803	·657	·422	·295	·203	·266	·452	·657	·669	·899	11.
·647	·692	·753	·833	·891	·892	·936	·806	·845	·802	·657	·419	·293	·204	·265	·451	·655	·663	·898	12.
·644	·693	·754	·836	·889	·887	·931	·804	·842	·803	·660	·418	·293	·205	·264	·451	·653	·658	·897	13.
·643	·694	·754	·837	·889	·871	·929	·801	·841	·797	·659	·418	·293	·203	·262	·448	·650	·655	·895	14.
·643	·696	·757	·838	·885	·887	·929	·800	·840	·796	·661	·418	·293	·203	·263	·447	·649	·654	·895	15.
·644	·697	·759	·843	·884	·885	·924	·796	·839	·797	·662	·420	·296	·203	·263	·447	·650	·651	·894	16.
·645	·702	·756	·847	·886	·884	·919	·793	·838	·802	·662	·424	·297	·206	·265	·447	·650	·649	·894	17.
·651	·707	·766	·852	·880	·885	·919	·790	·841	·805	·668	·426	·302	·211	·269	·450	·651	·651	·896	18.
·655	·708	·773	·858	·884	·890	·922	·791	·842	·810	·670	·431	·307	·215	·274	·453	·652	·652	·900	19.
·655	·708	·771	·860	·888	·894	·928	·795	·848	·813	·671	·436	·306	·215	·277	·462	·655	·657	·905	20.
·650	·705	·769	·859	·901	·901	·931	·800	·853	·814	·674	·437	·304	·215	·277	·468	·657	·659	·910	21.
·644	·700	·766	·858	·898	·905	·935	·806	·857	·819	·670	·437	·303	·212	·277	·467	·658	·662	·914	22.
·639	·693	·757	·849	·895	·902	·932	·807	·852	·819	·678	·438	·299	·212	·276	·464	·652	·663	·914	23.
·636	·685	·747	·832	·885	·888	·930	·805	·851	·807	·663	·424	·296	·201	·266	·453	·648	·665	·900	Means

Pressure of Dry Air.							BARNAOUL.—Pressure of Dry Air.												
ins. 27+	ins. 27+	ins. 27+	ins. 27+	ins. 27+	ins. 27+	ins. 27+	ins. 29+	ins. 29+	ins. 29+	ins. 29+	ins. 29+	ins. 28+	ins. 28+	ins. 28+	ins. 29+	ins. 29+	ins. 29+	ins. 29+	
·280	·199	·312	·550	·758	·813	·900	·732	·788	·707	·495	·197	·917	·721	·841	·180	·449	·526	·828	0.
·270	·197	·289	·551	·736	·807	·883	·730	·783	·698	·489	·189	·915	·711	·845	·175	·442	·526	·821	1.
·260	·191	·287	·554	·729	·800	·890	·732	·781	·691	·485	·186	·908	·712	·849	·178	·441	·528	·822	2.
·263	·191	·289	·531	·727	·803	·898	·740	·783	·685	·484	·179	·902	·713	·854	·178	·443	·533	·827	3.
·244	·189	·292	·527	·730	·806	·908	·749	·786	·687	·483	·180	·898	·711	·856	·180	·447	·547	·830	4.
·261	·198	·293	·535	·739	·826	·915	·754	·792	·692	·480	·173	·907	·713	·860	·183	·456	·552	·834	5.
·272	·197	·294	·540	·748	·826	·920	·754	·794	·700	·481	·177	·910	·715	·853	·187	·465	·561	·837	6.
·268	·212	·307	·561	·764	·842	·927	·756	·795	·705	·478	·172	·901	·721	·857	·192	·478	·559	·839	7.
·285	·225	·340	·580	·774	·848	·928	·757	·796	·712	·483	·178	·901	·718	·859	·198	·482	·557	·838	8.
·316	·265	·368	·592	·778	·848	·927	·756	·797	·714	·488	·183	·909	·728	·869	·205	·489	·550	·837	9.
·332	·287	·384	·598	·784	·847	·926	·755	·794	·714	·490	·187	·915	·745	·878	·209	·493	·549	·838	10.
·341	·301	·393	·605	·782	·845	·925	·754	·793	·717	·492	·191	·926	·750	·886	·210	·495	·546	·836	11.
·345	·307	·406	·612	·784	·842	·921	·749	·787	·720	·494	·189	·931	·761	·891	·211	·496	·539	·836	12.
·348	·309	·413	·613	·785	·841	·916	·747	·785	·721	·499	·190	·940	·774	·896	·214	·495	·536	·835	13.
·351	·314	·419	·621	·785	·845	·915	·743	·786	·716	·499	·194	·949	·779	·899	·213	·494	·533	·833	14.
·356	·322	·426	·629	·786	·842	·914	·742	·787	·718	·503	·198	·957	·784	·907	·216	·494	·533	·833	15.
·356	·329	·433	·638	·784	·840	·909	·738	·785	·721	·504	·200	·962	·788	·911	·220	·497	·529	·833	16.
·349	·318	·428	·643	·790	·840	·904	·734	·784	·727	·503	·193	·950	·782	·912	·222	·498	·529	·833	17.
·333	·304	·426	·647	·786	·841	·905	·732	·787	·730	·504	·181	·931	·759	·903	·225	·492	·532	·835	18.
·323	·274	·406	·642	·786	·845	·908	·731	·789	·732	·498	·175	·922	·741	·893	·218	·499	·532	·840	19.
·312	·250	·366	·620	·781	·849	·914	·736	·794	·728	·490	·174	·907	·728	·878	·217	·497	·537	·845	20
·293	·237	·338	·611	·786	·849	·915	·739	·794	·718	·492	·175	·902	·724	·859	·205	·488	·535	·849	21.
·287	·224	·322	·598	·774	·846	·916	·742	·792	·717	·497	·178	·897	·722	·847	·199	·480	·531	·851	22.
·282	·210	·308	·581	·764	·837	·910	·738	·781	·710	·496	·187	·888	·724	·852	·193	·466	·530	·848	23.
·305	·252	·356	·591	·768	·834	·912	·743	·789	·712	·492	·184	·919	·738	·873	·201	·478	·539	·836	Means

[redacted]

Hours of mean time.	KEW.—Humidity of the Air.												NERTCHINSK.—					
	January.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	October.	Nov.	Dec.	January.	Feb.	March.	April.	May.	June.
0.	85	72	66	63	63	53	52	62	62	72	80	84	91	89	73	56	39	57
1.	85	69	65	61	60	52	49	61	60	69	77	81	91	87	71	53	41	56
2.	83	67	64	60	57	52	47	61	59	68	74	81	89	86	70	53	41	55
3.	83	66	64	59	57	50	46	60	58	69	74	83	88	85	69	51	40	54
4.	89	67	65	60	56	50	45	58	61	74	76	84	84	83	68	51	39	56
5.	89	68	68	61	57	50	47	62	60	77	80	84	82	82	68	48	36	58
6.	91	73	75	66	59	51	50	65	69	81	81	87	82	81	68	56	42	59
7.	93	76	75	69	63	55	54	69	75	78	82	87	82	81	69	61	48	64
8.	89	78	79	75	69	62	60	75	79	88	84	87	82	82	70	64	52	70
9.	91	81	81	80	77	68	65	77	81	87	84	88	82	82	72	67	55	73
10.	88	82	81	79	81	74	69	80	84	88	85	87	84	82	72	68	59	74
11.	88	82	83	82	83	77	72	82	85	90	85	87	84	81	72	69	61	75
12.	88	84	85	85	86	79	74	84	88	91	85	87	84	81	73	71	64	79
13.	88	82	85	88	88	83	77	88	87	91	85	87	86	81	73	71	65	80
14.	88	84	87	88	89	85	78	88	88	91	85	87	85	82	75	73	66	81
15.	86	84	87	86	89	86	81	89	88	91	89	87	86	82	75	74	68	82
16.	86	82	87	90	89	87	83	91	90	91	89	87	86	81	76	74	66	83
17.	88	85	89	90	91	87	82	91	90	93	87	87	84	82	76	76	71	83
18.	90	85	93	88	88	82	80	89	88	96	87	87	84	82	76	75	70	81
19.	90	85	93	88	87	75	75	85	91	96	91	90	86	84	78	74	64	78
20.	88	91	97	82	80	69	72	78	85	94	85	88	88	88	81	69	58	73
21.	90	90	80	76	74	64	67	74	86	90	85	87	87	89	81	63	54	68
22	87	81	74	71	66	59	61	70	75	84	85	87	89	90	78	59	50	64
23.	89	87	70	67	65	57	56	67	70	77	86	85	89	90	74	57	47	60
Means	88	79	79	76	74	67	64	75	78	84	83	86	86	84	73	64	54	69

XII. "On the Connexion between oppositely disposed Currents of Air and the Weather subsequently experienced in the British Islands." By Robert H. Scott, M.A., Director of the Meteorological Office. Communicated by the President. Received June 17, 1869.

In the number of the 'Proceedings of the Meteorological Society' for February 1869, there is a paper by Mr. Charles Meldrum, of the Mauritius, on the connexion between the rotation of the wind in the Southern Indian Ocean and the positions of oppositely directed air-currents. In this paper the author expresses his opinion that the tropical hurricanes of the Southern Indian Ocean *invariably* originate between two opposite streams of air.

More than a year previous to the appearance of Mr. Meldrum's paper my own attention had been drawn to the occurrence in these islands of some remarkable storms, which appeared to be connected with the previous existence at the earth's surface of the two wind-currents, polar and equatorial, in close proximity to each other.

The first occasion on which this was noticed by me was on January 22,

Humidity of the Air.						BARNAOUL.—Humidity of the Air.												
July.	August.	Sep.	October.	Nov.	Dec.	January.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	October.	Nov.	Dec.	Hours of mean time.
63	63	59	60	81	86	93	94	85	56	47	51	56	59	64	66	82	94	0.
60	60	56	58	79	86	93	94	84	55	45	50	55	62	63	65	79	95	1.
59	58	55	58	78	85	93	94	83	53	45	50	54	57	62	62	79	95	2.
58	57	50	57	76	79	94	93	83	52	45	51	55	56	62	63	79	95	3.
58	57	52	60	75	76	93	94	83	53	45	52	56	56	63	64	80	96	4.
61	59	55	62	75	74	93	93	83	55	46	52	57	58	65	67	81	95	5.
64	65	60	66	74	73	94	93	85	57	48	53	61	63	69	69	82	94	6.
70	71	71	68	73	74	94	94	85	62	52	58	65	68	73	69	84	95	7.
75	75	72	70	73	74	94	94	86	65	57	64	72	73	76	72	85	94	8.
79	79	75	71	73	75	94	94	87	68	62	68	77	77	78	74	86	94	9.
82	80	78	71	73	76	94	95	88	70	65	74	80	80	80	76	86	94	10.
82	82	78	72	74	76	94	95	88	72	68	76	84	82	82	77	86	95	11.
84	84	77	72	74	75	94	96	89	73	72	78	85	84	83	79	86	95	12.
86	85	79	73	74	74	94	96	90	75	74	80	87	86	84	80	86	96	13.
87	86	80	74	73	73	94	96	91	77	76	81	88	87	86	80	87	96	14.
88	88	81	74	74	76	94	96	92	79	78	83	90	89	86	81	87	96	15.
89	88	82	76	74	78	95	96	92	80	80	84	91	90	87	81	88	95	16.
89	89	82	76	72	78	95	96	92	81	78	81	89	90	88	82	88	95	17.
88	90	83	75	74	77	95	96	92	79	74	78	86	89	88	82	89	95	18.
86	89	82	78	75	77	95	97	92	76	71	72	82	86	85	81	89	96	19.
79	84	77	78	76	77	95	97	92	74	64	69	77	82	82	80	89	96	20.
74	79	70	72	81	79	95	97	91	66	58	62	70	76	78	76	88	96	21.
70	73	67	68	82	82	95	96	89	61	53	58	63	69	73	73	86	95	22.
66	67	63	64	81	84	94	96	86	58	51	55	60	62	67	71	84	95	23.
75	75	70	69	76	78	94	95	88	67	61	66	72	74	76	74	85	95	Means

1868, when the atmospherical conditions over these islands were very remarkable; easterly winds were prevalent over the central and northern portions of the area, while in France there were strong westerly gales. The channels of the currents were so close to each other that, while at Yarmouth there was a strong easterly wind, there was a westerly gale at Portsmouth. The contrast exhibited by the two currents as regards temperature was very marked, and a dense fog was experienced in London. Barometrical readings were very low over the region which separated the districts of the respective currents. Next day pressure rose very rapidly; but this was only the precursor of an equally sudden diminution of its amount, and of the advent of the equatorial current which swept with great violence over these islands, producing a very serious southerly gale on the 24th of January.

On the 8th of December last, conditions similar to those of January 22 were observed. Strong easterly winds were reported from Scarborough, while westerly winds of great force prevailed in the Channel and in France. This state of things was succeeded, after an interval of two days, by a southerly storm, the whole sequence of phenomena resembling very closely what had been noticed eleven months before.

In order to trace out this remarkable succession of occurrences, it was resolved to examine all the cases in which the polar and equatorial currents made their appearance at the surface of the ground within the area of the British Islands, and to record the phases of weather which ensued. As these currents flow in opposite directions, it is evident that they must move in channels approximatively parallel to each other, so that there are only two cases to be examined.

I. The polar current flows in a latitude higher than the equatorial current.

II. The polar current flows in a latitude lower than the equatorial current.

The daily weather reports for a period of 27 months have been examined, and the result has been that 27 instances of case I. and 30 of case II. have been discovered.

These instances are all enumerated in two tables, which are herewith submitted*.

In accordance with the relation between the motion of the wind and the distribution of atmospherical pressure which has been laid down by Prof. Buys Ballot, viz. that barometrical readings are lower on the left-hand side of a current of air than on the right, we should expect to find that in case I. there would be a relative barometrical minimum, and in case II. a relative barometrical maximum between the currents. This supposition is found to be abundantly confirmed by the observations.

As regards the weather subsequently experienced, Mr. Meldrum states that when the two currents, the N.W. monsoon and S.E. trade, are noticed simultaneously over the Indian Ocean, the channel of the latter lying on the southern edge of that of the former, *i.e.* in a latitude higher than it, the barometer between them is low and falling. Ultimately the reduction of pressure becomes greater at one point than at the others, and a centre of barometrical depression is formed, resulting eventually in a cyclone.

In the instances which form the subject of the present paper, we have not been able to trace the actual genesis of a storm within the limits of our area of observation. Most of our storms come on us from the Atlantic, and are apparently not formed in the immediate district from which our reports are derived. The result of the investigation appears, however, to show that whereas the conditions of case I. are indicative of considerable atmospherical disturbance, those of case II. seem to show that winds will probably be light for some days.

Case I. The polar current flows in a latitude higher than the equatorial current.

In other words, easterly winds prevail in the north, westerly in the south. Northerly and southerly winds are nearly entirely absent.

Twenty-seven instances have been noticed of these conditions, and they

* The Tables are necessarily so condensed that they would hardly be intelligible if printed with the paper.

are very generally followed after a brief interval by a serious barometrical depression, frequently resulting in a southerly gale.

In 12 instances a southerly gale followed in 2 days.
„ 4 „ „ „ „ 3 days.
„ 6 „ fresh southerly winds, not a gale, followed.
„ 2 „ a north-east gale followed.
„ 2 „ a southerly gale set in at once.
„ 1 „ no change of weather ensued.
—
27

These facts appear to show that the conditions of case I. indicate a deep-seated disturbance of the atmosphere. In almost every case they seem to point to the existence, or at least the formation, of a barometrical minimum over the Atlantic Ocean, which will probably advance to our coasts and result in a southerly storm. On only two occasions did the centre of the disturbance pass to the southward of these islands, viz. the two instances in which the north-easterly gale followed.

Case II. The polar current flows in a latitude lower than the equatorial current.

In other words, easterly winds prevail in the south, westerly in the north.

Thirty instances have been noticed.

In 11 instances no change of weather ensued.
7 „ The polar current completely displaced the equatorial current over these islands, and easterly winds set in.
7 „ This displacement was only partial, and north-westerly winds set in.
5 „ Southerly gales or fresh southerly winds followed.
—
30

It would appear from the foregoing that case II. is not, as a rule, indicative of the approach of a serious atmospherical disturbance, although such did occur in five instances (one-sixth of the total number under consideration). In the great predominance of instances the weather either remained unchanged and calm, or else the polar current succeeded in displacing the equatorial more or less completely, and the winds which prevailed over these islands generally were from points between N.W. and E.

It is obvious that, from the very limited area from which our observations are derived, we are at present unable to examine into the mutual action of the currents on each other (unaffected by any influence exerted on the wind by inequalities in the earth's surface, such as those produced by an irregular coast-line stretching out into the open sea), as has been done by Mr. Meldrum for the Indian Ocean; but it is hoped that this commencement of an attempt to trace a connexion between successive conditions of weather may be deemed worthy of the notice of the Royal Society.

XIII. "On the presence of Sulphocyanides in the Blood and Urine." By Arthur Leared, M.D., M.R.I.A., &c. Communicated by C. Handfield Jones, M.B. Cantab. Received June 17, 1869.

In the course of some investigations into the composition and uses of saliva, I was led to study one of its components, sulphocyanide of potassium, in relation to its presence and action in the system, in a way that has not hitherto been done.

Treviranus, in 1814, discovered that saliva became reddened by a persalt of iron in solution; and the reaction was afterwards stated by Tiedemann and Gmelin to be due to the presence of sulphocyanide of potassium*. A strange difference of opinion has nevertheless prevailed on the subject. Thus the reaction has been supposed to be caused by a taint of the saliva from carious teeth; whilst Bernard states that he found it took place only in the saliva of tobacco-smokers.

It is unnecessary to produce here the arguments on both sides of the question; the weight of authority is altogether in favour of the existence of the salt in saliva. By some of those, however, who have admitted that it is an ingredient of the secretion it has been regarded as a curiosity rather than as playing any part in the economy.

I have made numerous experiments for the purpose of satisfying myself as to the constancy with which a sulphocyanide exists in human saliva. For this end a solution containing twenty grains of perchloride of iron in an ounce of distilled water was employed. Such a solution is of a light-yellow colour, but it acts better than the paler solution of the persulphate of iron. The mode of procedure was very simple. The saliva to be tested was ejected on a surface of white porcelain, or upon a piece of white paper, and a drop of the test-solution added. The colour which the saliva assumed was compared with a scale of four shades of red on paper, resembling those produced by the sulphocyanide of iron. These shades were labelled "very marked," "marked," "faint," "a trace," and corresponded approximately with the colours struck by iron in solutions of sulphocyanide of potassium of the relative strength of $\frac{1}{16}$, $\frac{1}{32}$, $\frac{1}{64}$, $\frac{1}{128}$ of a grain to the ounce of distilled water.

Such a scale is appended herewith. An examination of the saliva of fifty persons taken consecutively, half being males and half females, and of ages ranging from under one year to 65 years, gave the following results:—

	Very marked.	Marked.	Faint.	A trace.	None.
25 Males	0	17	4	3	1
25 Females....	1	10	8	2	4

As regards the influence of age and other practical points, the numbers

* Recherches expérimentales, Physiologiques et Chimiques sur la Digestion. Paris, 1827.

are insufficient for the deduction of trustworthy results. Some points, however, which it is unnecessary to prove by tabulation, were well borne out, and these are sufficient for the present purpose.

It was established that in the saliva of the great majority of persons a red colour is struck with perchloride of iron.

It was ascertained that the existence of carious teeth is not requisite for the production of this reaction, because it occurred in many instances in which all the teeth were sound.

It was further ascertained that tobacco-smoking was not indispensable, because the colour was produced in many cases in which the individual never used tobacco.

It was also remarked that in all the cases in which the absence of the sulpho-cyanide was noted, although no definite disease was apparent, the health was feeble, and that, on the other hand, a marked reaction with iron usually corresponded with the ordinary indications of sound health. To this subject I shall afterwards have occasion to return. It is probable that, by means of evaporation, a sulphocyanide would in every instance have been detected in the saliva. But for practical purposes it is assumed that when not discovered by the means described, it is not present.

The particular base combined with the sulphocyanic acid in human saliva is a matter of little importance. It has lately been stated that it is not potassium, but sodium, which was long ago mentioned by Tiedemann and Gmelin as taking the place of potassium in the saliva of sheep.

The soluble sulphocyanide which exists in the saliva cannot be regarded as an excretion, because it passes with the saliva into the stomach. Whatever its use or its ultimate destination, it seemed probable that a salt of so stable a nature was not decomposed in its passage through the system. This suggested that I should look for it in the urine.

Iron, as is well known, yields a very characteristic test of the presence of sulphocyanides. One compound only which it forms, namely, that with meconic acid, is at all likely to be confounded with the sulpho-cyanide of iron. The great sensitiveness of this test also makes it peculiarly adapted for quantitative analysis, by means of colour.

I found in my first experiments that when the urine of a person in whose saliva a sulphocyanide was abundant, was concentrated by evaporation, a reddish-brown colour was caused by the addition of perchloride of iron. But owing to the dark colour assumed by the concentrated urine, and the mode in which precipitation occurred, no reliance could be placed on this as a proof of the presence of a sulphocyanide in the urine. The step which then suggested itself was to decolorize the urine by means of animal charcoal. But it turned out, when this was effected, that the colourless liquid gave no reaction with the persalt of iron. The following experiment was then tried:—

A solution of one grain of sulphocyanide of potassium in an ounce of

distilled water was filtered through animal charcoal. The filtrate was tested with the iron solution. There was no reaction whatever.

It was plain from this that animal charcoal possesses the power either of separating sulphocyanides from their solutions or of decomposing them.

Various other methods for separating the colouring-matter were now tried with more or less success. The most perfect of these, as regards the removal of colour, was the addition of a solution of sub-acetate of lead. But the use of this solution is open to the objection that acetate of iron, which is formed in testing for sulphocyanic acid, is itself red. It is true that the colour is not so intense as that which was actually formed in most cases; and it was possible in estimating the amount of the essential colouring-agent present by an easy application of the colour-test to deduct the amount of colour due to the acetate of iron.

A modification of the method employed by Professor Harley for separating the colouring-matter of the urine, for the purpose of obtaining urohæmatin, proved on the whole the best. It consists in evaporating the urine in a water-bath to the consistence of thick syrup, treating with alcohol, and adding gradually milk of lime. The filtrate from this mixture was found to be of a light-yellow colour, closely resembling that of the iron solution. The ferric solution was added to this filtrate so long as precipitation of oxide of iron occurred. The liquid now assumed a reddish colour, varying in depth according to circumstances. The mixture was then filtered; but it generally happened that, after standing some hours, a second filtration was necessary.

The coloured fluid obtained by either of these methods from evaporated urine is of a bright red colour, exactly resembling that formed by an aqueous solution of sulphocyanide of potassium with perchloride of iron.

In some respects the two solutions did not exactly agree.

The colour of an aqueous solution of sulphocyanide of iron is only affected by mineral acids when in considerable excess. But the colour formed with iron in evaporated urine is easily destroyed by these acids.

The colour of the pure solution is removed by perchloride of mercury, while that of the organic solution is not affected by the mercurial solution.

It is well known that in certain cases the presence of organic matter in solution greatly modifies chemical action. The action of acids in the present instance was a question of degree. The colour was removed from the urinary solution by a small quantity of a mineral acid, and it was removed or impaired in case of a pure solution by a greater quantity of acid.

The following observation throws light upon the action of perchloride of mercury in the respective solutions.

Perchloride of mercury at once destroys the colour of an aqueous solution of sulphocyanide of iron. But, as I have ascertained, if the solution has been previously boiled (and boiling was employed in the case of the

urinary solutions), the red colour is no longer destroyed by the mercurial solution.

Since, then, these difficulties are capable of removal, the argument by the method of exclusion in favour of the red colour being due to sulphocyanide of iron appears conclusive. There is in fact no other source from which the red colour could proceed in the process by which the urine was decolorized by milk of lime.

Some salt of sulphocyanic acid must, then, be admitted to be a component of the urine.

For the detection of the salt it is only necessary to evaporate eight ounces of urine in a water-bath. If milk of lime be employed as the decolorizing agent which, for reasons already stated, is to be preferred, the urine should be concentrated to a thick syrup.

In the present stage of my inquiries many details are purposely omitted, particularly those which refer to the quantitative determination of the sulphocyanide in many different samples of urine. I may mention, however, that I found the average quantity present in healthy urine to amount to about $\frac{1}{8}$ of a grain in sixteen ounces.

Since, then, a sylphocyanide was found in the urine, and was previously known to exist in saliva, it was natural to look for it in other secretions. It was therefore sought for in a large quantity of cow's milk, and in two ounces of human sweat, but with negative results.

Two ounces of pure pus from a cyst on a man's back were also examined, but no sulphocyanide was found.

But as sulphocyanic acid was proved to exist in a secretion from which it may be presumed to enter the blood, and also in an excretion derived from the blood, it was to be expected that it would be found in the blood itself.

The blood operated on was in every instance diluted with an equal part of distilled water. The mixture was then evaporated in a water-bath until the red colour was altogether lost, and brown coagula, with apparently little fluid, remained. The mass was strained through muslin by pressure of the fingers. The filtrate was then decolorized by one of the processes already described. Briefly stated, then, it was found that a sulpho-cyanide exists in the blood of man, and in that of the pig, fowl, turbot, salmon, and toad.

I also found that when the serum of pig's blood, procured as free from colour as possible and diluted with an equal portion of water, to prevent complete coagulation, was treated with a solution of perchloride of iron, it became red in a marked degree. This result has a special interest, because it was obtained without any previous chemical manipulation, and the presence of a sulphocyanide was thereby proved. And this curious circumstance was also ascertained. If a few drops of a weak solution of sulphocyanide of potassium be mixed with this reddened and diluted serum, and the iron solution is again added, no increase of colour is produced. This

shows that while the sulphocyanide naturally present in the serum is capable of combining with added iron, the serum possesses the power of preventing the formation of sulphocyanide of iron when both compounds are added and intermixed with it.

An analogous masking of chemical action is described by Bernard. He found that when a solution of lactate of iron is mixed with serum, and a solution of cyanide of potassium is then added, prussian blue is not formed, as would be the case if the solutions were mixed in water instead of serum.

I have not been able to decide positively whether the sulphocyanide is or is not confined to the serum. Analysis, after combustion, is unsuitable, because sulphocyanides are formed in the combustion of organic matter. But so far as I have been able to determine from the maceration of the clot in water, the sulphocyanides exist only in the serum.

The foregoing facts point either to the presence of free sulphocyanic acid, or of sulphocyanide of potassium, or sodium, or of both, in the serum of the blood. This leads to the consideration of that much-vexed question, the cause of the red colour of the blood. So far as concerns exact colour, nothing more closely resembles blood than a solution of sulphocyanide of iron. This is *primâ facie* evidence that red blood owes its colour to the iron compound. The iron is known to be localized in the globules. These are surrounded by a fluid containing sulphocyanic acid in a combination which easily yields the acid when required. Such is the theory at present suggested.

I am not unaware of the difficulties in the way of its acceptance. The colour of hæmatin cannot, it is said, depend upon the iron it contains, because nearly the whole of the iron may be removed without affecting the colour of the hæmatin*. But it is not stated that all the iron is ever removed, and it may be that a very small proportion suffices for the formation of the colour, while the larger proportion of the metal is held in reserve in the globules in the same manner as sulphocyanic acid appears to be in the serum.

Having found a sulphocyanide in the blood, it next occurred to me to look for it in the eggs of birds. It is natural to suppose that, since in the process of incubation red blood is formed, its assumed elements of colour would be found in the egg before incubation. This supposition proved correct. Fortunately the albumen of the hen's egg affords a ready means of research. It is only necessary to mix it with an equal quantity of distilled water, by which complete coagulation by the iron solution is prevented. The albumen of a hen's egg weighs about 300 grains, and this quantity was found to contain $\frac{1}{20}$ of a grain of sulphocyanic acid. The yolk was intimately mixed with water, then evaporated to dryness in the water-bath, and extracted with alcohol; but no trace of the salt could be detected. It is probable, therefore, that the sulphocyanide exists exclusively in the

* Elements of Chemistry. By W. Miller, M.D., 3rd edit. p. 872.

albumen, which, as the process of vivification proceeds, enters into combination with iron, which originally exists in the yolk.

The presence of a sulphocyanide in saliva must be referred to one of two sources. It is either an exclusive product of the secretion itself, or it previously exists in the blood and is extracted from it as a component of the saliva. The amount of sulphocyanide found in different analyses varies greatly; my own results show only about half a grain in twenty ounces of saliva from a healthy subject. This nearly agrees with the observations of Bidder and Schmidt. Wright makes the quantity very much greater. If we take the estimate at only half a grain in twenty ounces of saliva, and reckon this to be the quantity of the secretion swallowed in twenty-four hours, the salt might be probably found in the blood and in the urine.

If, however, my experiments have been rightly interpreted, it is certain that sulphocyanide of potassium or sodium is not a mere product of the salivary glands, We have seen that it is found in the blood of all orders of vertebrate animals, and we know that fish do not possess salivary organs. Assuming that it is extracted out of the blood, what is its use in the saliva?

Considering its composition, it seemed possible that it acted either as an antiseptic or else as an agent which prevented fermentation in the alimentary canal, and thus indirectly aided digestion.

The conditions which favour the fermentation of saccharine matter, namely, acidity and the proper temperature, are constantly present in the stomach. Is sulphocyanide of potassium in saliva destined to check this fermentation, which, under favourable circumstances, may occur in less than an hour?

Carefully conducted experiments proved that it neither possesses the power of preventing ordinary fermentation nor that of checking it when already in action.

We shall now see what is its action in preventing putrefaction. Two equal portions of roast mutton were placed, the one in water, and the other in the same quantity of a solution of sulphocyanide of potassium of the strength of 1 grain of the salt to 1 ounce of water. After some weeks the meat which had lain in water was found to be broken up into shreds, and was quite putrid; that in the sulphocyanide solution was merely softened, and had a sour smell, but was not putrid.

Sulphocyanide of potassium, therefore, possesses an antiseptic power; but whether or not this property comes into operation in the alimentary canal is a question I cannot now decide.

I have made many quantitative analyses to determine the amount of sulphocyanide eliminated with the urine in various diseases, including typhus, typhoid, and scarlet fever.

Not to enter into details at present, it will be sufficient to state what the results showed with much uniformity. In all diseases in which wasting of the body was marked, the excretion in the urine of a sulpho-

cyanide, in common with some other substances, was unusually great. This increase of it in the urine was found to correspond with its decrease in, or more frequently its disappearance from, the saliva. This circumstance goes to prove that the salt is not formed by the saliva, but is an ingredient of the blood itself.

XIV. "On some Elementary Principles in Animal Mechanics."—No. II. By the Rev. Samuel Haughton, M.D. Dublin, D.C.L. Oxon., Fellow of Trinity College, Dublin. Received June 14, 1869.

In a former communication to the Royal Society on this subject (Proceedings, 20th June 1867), I endeavoured to establish the two following principles:—

I. *That the force of a muscle is proportional to the area of its cross section.*

II. *That the force of a muscle is proportional to the cross section of the tendon that conveys its influence to a distant point.*

The first of these principles is true under all circumstances, but the second requires to be modified somewhat in its statement. If the conditions as to friction of the tendons that convey the action of the muscles to a distant point be the same, then the force of the muscles will be proportional to the cross sections of the tendons; but if the tendons be subjected to different amounts of friction, then the areas of their cross sections will cease to be proportional to the forces of the muscles, as represented by the areas of their cross sections.

In my former paper (No. I.), I selected, in illustration of principle II., the long flexor tendons of the toes of the Rhea and other struthious birds, and showed that the cross sections of the muscles and tendons bore, approximately, a constant ratio to each other. Now, in the *Struthionidæ*, the conditions as to friction of the long flexor tendons of the toes are similar although different in each species, and hence it was easy to prove that the ratios of the cross sections of the muscles and tendons were nearly constant.

When, however, muscles and tendons, variously conditioned as to friction, are compared together, the constancy of the ratio of their cross sections disappears, and undergoes a variation depending on the friction to which both muscles and tendons are exposed.

In order to ascertain the proportion of the cross section (or force) of a muscle to the cross section (or strength) of its tendon in the human subject, I made the following observations on the right arm and hand of a well-developed male subject in the Royal College of Surgeons in Ireland, in March 1868.

I first ascertained the specific gravities of the muscles and tendons, with the following results:—

Muscles.

	Sp. gr.
Biceps humeri (*long head*)	1·050
Biceps humeri	1·054
Brachialis	1·053
Mean............	1·0523

Tendons.

Scapular tendon of biceps	1·112
Radial tendon of biceps	1·119
Mean............	1·1155

From these specific gravities it was easy to determine the cross section of either muscles or tendons, by weighing a known length of either one or other. In this manner the following Table was constructed:—

Cross sections of Muscles and Tendons in an Adult Human Male Subject, and Ratios of same.

Name of muscle.	Cross section of muscle, in square inches.	Cross section of tendon, in square inches.	Ratio of cross section of muscle to that of tendon.
1. Biceps humeri	0·895	0·0317	28·2
2. Palmaris longus	0·148	0·0050	26·4
3. Ext. carp. rad. longr.	0·584	0·0223	26·2
4. Ext. carp. rad. brevr.	0·405	0·0220	18·4
5. Biceps humeri (long head)	0·379	0·0212	18·0
6. Fl. poll. longus	0·228	0·0145	15·7
7. Fl. carp. rad.	0·234	0·0155	15·1
8. Ext. carp. uln.	0·212	0·0199	10·7
9. Fl. dig. subl.	0·618	0·0665	9·3
10. Fl. dig. prof.	0·768	0·0928	8·3
11. Ext. oss. met. poll.	0·223	0·0289	7·7
12. Fl. carp. uln.	0·182	0·0254	7·2

From the preceding Table, it appears that the ratio of the cross section of the muscles to that of the tendons may range from 7 to 28, or be four times greater in one case than another. We may also see in general, that the tendons exposed to the greatest amount of friction have the smallest coefficients of cross section. Thus the radial tendon of the *biceps* has a coefficient of 28·2, while the scapular tendon, which undergoes the friction of passing over the head of the humerus, has a coefficient of 18·0. Again, the *Ext. oss. met. poll.*, whose tendon winds round the radius, and has the duty imposed upon it of binding down the tendons of the radial extensors of the wrist, has the coefficient of 7·7, as compared with 26·2 and 18·4, the coefficients of the comparatively free tendons of these extensors.

As it might be objected that the relative cross sections of muscle and tendon, in a human subject that died a natural death, might be exceptional

in character, from wasting during the last illness, I determined to test the question by experiment, and accordingly selected a fine Pyrenean Mastiff for the purpose, which I killed by strychnia, and dissected immediately after death, with the following results, which were obtained, as before, by noting the specific gravities of the muscles and tendons, and by weighing a measured length of each:—

Cross sections of Muscles and Tendons in a Pyrenean Mastiff, and ratios of same.

Name of muscle.	Cross section of muscle, in square inches.	Cross section of tendon, in square inches.	Ratio of cross section of muscle to that of tendon.
1. Gastrocnemius	2·631	0·0520	50·6
2. Fl. carp. rad.	0·283	0·0059	48·0
3. Fl. dig. long.	0·195	0·0045	43·3
4. Ext. carp. rad................	0·632	0·0160	39·5
5. Fl. carp. uln.	0·176	0·0056	31·4
6. Fl. hall. long................	0·680	0·0228	29·8
7. Biceps humeri...............	0·909	0·0449	20·2
8. Fl. dig. subl.	0·319	0·0251	12·7
9. Fl. dig. prof.	0·902	0·0830	10·9
10. Ext. carp. uln.	0·181	0·0197	9·2

These results, obtained from measurements made upon a freshly killed animal, confirm those found from observation of the human subject, and prove that the ratio of the cross section of the muscle to that of its tendon depends upon the amount of friction experienced by the latter, the coefficient being greater in proportion as the friction is less.

The following observations, made upon a Wallaby Kangaroo, confirm in a general way the preceding results:—

Cross sections of Muscles and Tendons in a Wallaby Kangaroo, and ratios of same.

Name of muscle.	Cross section of muscle, in square inches.	Cross section of tendon, in square inches.	Ratio of cross section of muscle to that of tendon.
1. Gastrocnemius...............	1·313	0.0356	36·9
2. Fl. long. dig.	0·354	0·0246	14·4

It appears from the preceding investigation that the cross section of a muscle does not bear a constant ratio to the cross section of its tendon, unless the friction experienced by the muscle and tendon be also constant, and that there may even be a *surplusage* of strength in the tendon beyond what is absolutely necessary to resist the combined force of the muscle and friction. This surplusage, however, cannot be supposed to be large, if the principle of *economy of material* in nature be admitted.

XV. "Researches on the Hydrocarbons of the series $C_n H_{2n+2}$. —No. V. On Octyl compounds." By C. SCHORLEMMER. Communicated by Prof. STOKES, Sec. R.S. Received June 17, 1869.

After I had found that the octylalcohol obtained by distilling castor oil with caustic soda, is methyl-hexyl carbinol, or a secondary alcohol*, it appeared to me of great interest to study the chemical structure of those alcohols which can be obtained from the different hydrocarbons of the formula $C_8 H_{18}$, the more so as Cahours and Pelouze assert that the derivatives of the octane contained in petroleum are identical with those derived from the castor-oil alcohol†, a statement which was afterwards confirmed by Chapman ‡.

The hydrocarbons which I used for my experiments were hydride of octyl, or octane from petroleum, and the hydrocarbon of the same composition, which I obtained by acting upon iso-octyl iodide with zinc and hydrochloric acid. The two hydrocarbons, as well as their derivatives, resemble each other in their physical properties so much, that one would be inclined to consider them as identical; their chemical properties, however, prove that they are only isomeric.

(1) *Derivatives of Octane from Petroleum.*

The boiling-point of this hydrocarbon is given differently by different observers between 116° and 120°; according to my latest researches, it appears to boil a few degrees higher. After fractionating it for a very long time, the greater portion was found to boil between 120° and 125°; it was now heated with nitric acid and again fractionated over sodium. A considerable portion distilled now at 119°–122°, but by far the largest quantity at 122°–125°. From this latter portion I prepared the octyl-chloride, a colourless liquid, which smells like oranges, and boils at 173°–176°. This chloride was heated up to 200° for several hours with concentrated acetic acid and potassium acetate. It was thus completely decomposed; the chief product of the reaction consisted of octylene, besides that a much smaller quantity of octyl-acetate had been formed. This ether is a colourless liquid, boiling at 200°–205°, and having a pleasant pear-like odour. It was converted into octyl-alcohol by heating it with an alcoholic solution of caustic potash. This alcohol, after being purified by washing it several times with water, and drying over fused potassium carbonate, was obtained as a colourless oily liquid, boiling at 180°–182°, and possessing exactly the odour of methyl-hexyl carbinol.

The alcohol was oxidized by mixing it slowly with a cold solution of 2 parts of potassium dichromate and 3 parts of sulphuric acid in 10 parts of water, care being taken to avoid as much as possible any elevation of

* Proc. Roy. Soc. vol. xvi. p. 376.

† Ann. Chem. Phys. 4 ser. vol. i. p. 53.

‡ Journ. Chem. Soc. N. S. vol. iii. p. 296.

temperature during the reaction. As soon as a permanent brownish tinge showed that a slight excess of chromic acid was present, no more of the oxidising mixture was added. The liquid was shaken from time to time, and distilled after a few hours. The acid distillate was neutralized with sodium carbonate; only a small quantity of an acid was formed; the chief oxidation-product consisted of a neutral oil, having the odour of methyl-œnanthol, and, like this compound, it formed with hydrogen-sodium sulphite a crystalline compound. The liquid having no constant boiling-point, and the quantity being only small, I did not analyze it, but oxidised it further by heating it with the chromic-acid solution. The acid distillate was neutralized with sodium carbonate, the solution of the sodium salts was evaporated and distilled with diluted sulphuric acid. An oily acid, distilled over the residue in the retort, contained a large quantity of acetic acid, which was obtained pure by several distillations; from it silver acetate was prepared and analyzed.

0·3355 of this salt contained 0·2160 silver.

Calculated for $C_2 H_3 Ag O_2$.	Found.
64·67 per cent. Ag.	64·38 per cent.

The distillate, which contained the oily acid, was neutralized with an excess of barium carbonate, boiled and filtered. On evaporation, the barium salt separated in form of an amorphous skin; it could not be obtained in the crystalline state; I therefore dissolved it again in more water, and precipitated it fractionally with silver nitrate.

1st Precipitate ..	0·1535	gave	0·0754 silver.
2nd „ ..	0·3718	„	0·1798 „

Calculated for silver caproate, $C_6 H_{11} Ag O_2$.	Found. (1)	(2)
48·43 per cent. Ag.	49·12	48·36.

In the liquid, from which the second precipitate had been filtered off, a further addition of silver nitrate did not give any more precipitate. On evaporating it, small granular crystals separated, the analysis of which showed that they consisted of impure silver acetate; 0·5540 contained 0·3440 silver, or 63·23 per cent.

Besides caproic and acetic acids, a small quantity of an acid having the composition of caprylic acid was formed. This acid was precipitated probably as a basic salt, together with the excess of barium-carbonate, used in neutralizing the oily acid; it could not be extracted by boiling water. On dissolving the mixture of barium-salt in diluted nitric acid, oily drops separated. The liquid was distilled, the distillate neutralized with ammonia and precipitated with silver nitrate in two fractions,

1st Fraction ..	0·1344	contained	0·0566 silver.
2nd „ ..	0·0465	„	0·0203 „

Calculated for $C_8H_{15}AgO_2$	Found. (1)	(2)
43·02 per cent. Ag.	42·11 per cent.	43·87 per cent.

(2) Derivatives of Octane from Methyl-hexyl Carbinol.

The alcohol obtained from castor oil is easily converted into its corresponding hydrocarbon by treating it first with iodine and phosphorus, and acting upon the iodide thus obtained with zinc and hydrochloric acid.

I have already described this hydrocarbon in a former communication; it boils constantly at 124° C. The octyl-chloride obtained from it has only a faint orange-like smell; it boils at 174°–176°. Heated with concentrated acetic acid and potassium-acetate to 200°, it is decomposed after a few hours, octyl-acetate and octylene being formed in about equal quantities, whilst the chloride obtained from petroleum gave about three times more octylene than acetate.

This acetate boils at 198°–202°, and has the same pear-like odour as that described above. The alcohol prepared from it by heating it with an alcoholic solution of caustic potash, had no constant boiling-point; it distilled between 180° and 190°; the greatest portion between 182° and 186°; its odour is very much like that of methyl-hexyl carbinol. As I had only about 4 grammes, I could not subject it to fractional distillation. On oxidizing it with the chromic-acid solution, the greatest care was taken to avoid rise of temperature, the solution being added very slowly until a permanent brownish colour showed that a slight excess of chromic acid was present, the vessel being all the time surrounded by cold water. In order to have as decisive results as possible, I oxidised at the same time, and under exactly the same circumstances, 4 grammes of methyl-hexyl carbinol. The liquids were allowed to remain together for a day, and were frequently shaken, then distilled, and the distillate neutralized with sodium carbonate.

The two results differed very widely; methyl-hexyl carbinol was, as in my former experiments, converted into methyl-œnanthol, a small portion of which was oxidised further to acetic and caproic acids. The caproic acid was separated by repeated distillation from the acetic acid, and neutralized with ammonia. From this solution I purposed to prepare silver salts by fractional precipitation, but only one precipitate was obtained.

0·1113 of this salt contained 0·0536 silver.

Calculated for $C_6H_{11}AgO_2$.	Found.
48·43 per cent. Ag.	48·16 per cent. Ag.

The octylalcohol from the pure hydrocarbon yielded a large quantity of an oily acid, and a smaller portion of a neutral oil, but not a trace of acetic acid. The oily acid has the composition of caprylic acid; it was analyzed as the silver salt, which was obtained by fractional precipitations.

1st Precipitate ..	0·3500	contained	0·1700 silver.
2nd „ ..	0·3090	„	0·1335 „

Calculated for $C_8 H_{15} Ag O_2$.	Found. I.	Found. II.
43·02 per cent.	42·86 per cent.	43·20 per cent.

The neutral oil had quite the properties of an acetone; it gave a crystalline compound with hydrogen-sodium sulphite, and was not changed any further by the oxidising mixture in the cold; but on heating them together, oxidation took place and fatty acids were formed, which appeared to be a mixture of propionic and valerianic acids; of acetic acid not a trace could be detected.

1st Fraction of the silver salt	.. 0·1325	gave	0·0675 silver.
2nd „ „ „	.. 0·2732	„	0·1385 „
3rd „ „ „	.. 0·1562	„	0·0780 „

Calculated for Silver valerate.	Calculated for Silver caproate.	Found. I.	Found. II.	Found. III.
51·67 per cent. Ag.	48·43 per cent. Ag.	50·94	50·69	50·0

The percentage amount of silver contained in these salts corresponds better with that of valerate than that of caproate; most probably a little caprylic acid was still present, which caused the amount of silver to be a little too small.

The solution from which these silver salts had been precipitated gave on evaporation small granular crystals, having the composition of silver propionate.

0·1738 gave 0·1033 silver.

Calculated for $C_3 H_5 Ag O_2$.	Found.
59·67 per cent. Ag.	59·43. per cent.

From the results of my experiments I draw the following conclusions:—

(1) The octyl alcohol, obtained from the hydrocarbon $C_8 H_{18}$, occurring in American petroleum, consists chiefly of methyl-hexyl carbinol, $\left.\begin{matrix} CH_3 \\ C_6 H_{13} \end{matrix}\right\} CH\,OH$, and is therefore identical with the alcohol obtained from castor oil. Not only the physical properties of the two and their derivatives agree*, but also their oxidation products are the same; they both give methyl œnanthol or methyl-hexyl acetone, $\left.\begin{matrix} CH_3 \\ C_6 H_{13} \end{matrix}\right\} CO$, which, by further oxidation, splits up into acetic and caproic acids.

Besides this secondary alcohol, there is also formed a smaller quantity of a primary alcohol, as amongst the products of oxidation an acid containing eight atoms of carbon was found.

(2) The hydrocarbon, $C_8 H_{18}$, which is formed by replacing in methyl-hexyl carbinol the group hydroxyl HO by hydrogen, is different from that

* The only exception is the boiling-point of the acetate, which I found to be 200°–205°, whilst that from castor oil boils, according to Bouis, at 193°.

[redacted]

found in petroleum. It gives, by the proper reactions, a considerable quantity of a primary alcohol, and a smaller quantity of a secondary one; the latter is not identical with methyl-hexyl carbinol, but consists most probably of ethyl-amyl carbinol, $\left.\begin{matrix}C_2H_5\\ C_5H_{11}\end{matrix}\right\}CH\,OH$, as, on oxidation, it yields valerianic and propionic acids.

The primary alcohol appears to differ from the primary octyl alcohol, which has been found lately by Zincke in the seeds of *Heracleum spondylium**. The essential oil of these seeds consists chiefly of an octyl acetate, boiling at 206°–208°, and possessing an orange-like smell, whilst that which I obtained smells strongly of pears, and boils at 198°–202°. By oxydising his alcohol, Zincke obtained a caprylic acid, which solidified at 12°, whilst the acid which I got remained liquid at 0°. Zincke's alcohol is most likely the normal alcohol, and that which I obtained an alcohol containing the group isopropyl†.

(3) On acting upon the hydrocarbons of the series C_nH_{2n+2} with chlorine, a mixture of primary and secondary chlorides is formed. This is proved by the fact that the alcohols derived from these chlorides yield, on oxidation, besides an acid containing the same number of atoms of carbon as the alcohol, also acetones, or the characteristic oxidation products of secondary alcohols. Not only the above researches show this, but also my former experiments on the oxidation of amyl-alcohol prepared from the hydride, which gave, besides valerianic acid, also acetic acid and the acetone, $C_5H_{10}O$‡.

It is certainly very remarkable that the hydrocarbon from petroleum yields *methyl-hexyl carbinol,* whilst the hydrocarbon which is obtained from methyl-hexyl carbinol is not reconverted into this alcohol, but gives *ethyl-amyl carbinol,* and besides a primary alcohol.

The further investigation of this subject is certainly of the highest theoretical interest; but there is great difficulty in pursuing this research, as I have already observed, in consequence of the small yield of pure alcohol from large quantities of the hydrocarbons.

XVI. "On the Derivatives of Propane." By C. Schorlemmer. Communicated by Prof. Stokes, Sec. R.S. Received June 17, 1869.

The chief product obtained by the action of chlorine upon propane consists, as I have already stated in my last communication§, of propylene dichloride; besides this compound, we find in smaller quantities the normal propyl chloride and products richer in chlorine, which boil between

* Zeitschrift für Chemie, N. F. vol. v. p. 55.

† Proc. Roy. Soc. vol. xvi. p. 379.

‡ Ibid. p. 374.

§ Ibid. No. 111, 1869.

100° and 200° C. To obtain the latter in larger quantities, I took those portions of the substitution-products which boiled above 80°, and passed chlorine into them for several days, having them exposed to direct sunlight, as in diffused light hardly any action took place. By this means a liquid was obtained which boiled between 120° and 200°. Subjected to fractional distillation, the greater portion boiled between 150° and 160°, but it was found impossbile to isolate a compound having a constant boiling-point. The reaction of this liquid, as well as the boiling-point and the analysis, show that it consists of *trichlorhydrine*, $C_3H_5Cl_3$, mixed with higher chlorinated products.

0·275 gave 0·8155 silver chloride.

Calculated for $C_3H_5Cl_3$.	Found.
72·20 per cent. Cl.	73·34 per cent.

The reaction most characteristic of trichlorhydrine is that, on heating it with caustic potash, it decomposes into hydrochloric acid and epidichlorhydrine, $C_3H_4Cl_2$, a liquid which boils at 100°, and which combines directly with bromine, forming the compound $C_3H_4Cl_2Br_2$, the boiling-point of which is 220°.

On heating the liquid, boiling between 150° and 160°, with powdered caustic potash, a violent reaction set in, and, besides water, a heavy oil distilled over, which possessed the somewhat garlic-like odour of epidichlorhydrine, and which boiled between 95° and 105°. The higher chlorinated products contained in the original liquid were destroyed by this reaction, carbonaceous matter being left with the potassium chloride in the retort. To the impure epidichlorydrine thus obtained bromine was added; this combined with it with a hissing noise and evolution of heat. On distillation, the greater portion of the compound boiled at 200°–220°; the part boiling between 215° and 220° was analyzed.

0·1835 of this compound gave 0·4455 of a mixture of silver chloride and silver bromide.

0·2955 of this mixture left, on heating it in a current of hydrogen, 0·1928 silver.

	Calculated.		Found.
C_3	36	13·28	——
H_4	4	1·48	——
Cl_2	71	26·20	26·3
Br_2	160	59·04	58·2
	271		

These experiments prove sufficiently that the liquid boiling between 150° and 160° contained a large proportion of trichlorhydrine. It is noteworthy to remark that the substitution-products of the primary propyl chloride are identical with those of the secondary chloride, as, according to Linnemann, on passing chlorine into isopropyl iodide, the products

which are formed are (1) the secondary chloride, (2) probably propylene dichloride, and (3) trichlorhydrine*.

In my last communication I have already called attention to the different behaviour of ethane and propane under the action of chlorine. A further instance is the formation of trichlorhydrine, the chemical structure of which is most probably expressed by the formula $CH_2Cl-CHCl-C_2HCl$, whilst by substituting 3 atoms of hydrogen by chlorine in ethane, the compound CH_3-Cl_3 is formed.

The liquid from which I had separated the trichlorhydrine was again treated with chlorine in the direct sunlight for several days. On distilling it afterwards, it came over between 200° and 250°. The portion boiling between 200° and 205° solidified in the receiver as a white, crystalline mass. In order to remove from it an oily liquid which it contained, it was pressed between filter-paper and recrystallized repeatedly from alcohol. The analysis conducted to the formula $C_3H_4Cl_4$.

(1) 0·1462 gave 0·5460 silver chloride and 0·0035 silver.

(2) 0·1282 gave 0·4051 silver chloride.

	Found.	
Calculated for $C_3H_4Cl_4$.	I.	II.
78·01 per cent.	78·02 per cent.	78·24 per cent.

Tetrachlorpropane, as this compound may be called, crystallizes from a hot alcoholic solution in small needles, four or eight of which are generally grouped together, forming a regular star. Its smell strongly resembles that of camphor. Exposed to the air, it volatilizes pretty quickly; heated in a test-tube, it fuses, subliming rapidly at the same time. In a sealed capillary tube it melted at 177°–178°, and solidified again at 176°–175°.

The liquid boiling between 205° and 250° was very little acted upon by chlorine even in the brightest sunshine and presence of iodine; also treatment with potassium chlorate and fuming hydrochloric acid produced little effect, as, after acting upon it for several days, the liquid boiled again between 220° and 250°. The portion boiling between 243° and 250° was analyzed:—

0·1926 gave 0·6580 silver chloride and 0·0034 silver.

Calculated for $C_3H_2Cl_6$.	Found.
84·86 per cent. Cl.	85·04 per cent.

This compound is therefore hexachlorpropane, $C_3H_2Cl_6$, a colourless, heavy liquid, which smells somewhat like camphor, and boils without decomposition at about 250°.

From these experiments it would appear that in propane we cannot replace by direct substitution more than six atoms of hydrogen by chlorine. This observation gains in interest by the fact that in sextane (hexylhydride) C_6H_{14}, from petroleum also not more than six atoms of hydrogen

* Annal. Chem. Pharm. vol. cxxxvi. p. 48, and vol. cxxxix. p. 17.

are replaceable by chlorine. Pelouze and Cahours* state that the last substitution-product of this hydrocarbon is the compound $C_6H_8Cl_6$. I repeated this experiment, and passed chlorine into pure sextane, first in the diffused and afterwards in the direct sunlight, as long as any action could be observed. Thus I obtained a heavy colourless liquid, which did not distil without decomposition, the analysis of which showed that it had the above composition.

0·1612 gave 0·4654 silver chloride and 0·0076 silver.

Calculated for $C_6H_8Cl_6$.	Found.
72·7 per cent. Cl.	72·8 per cent.

XVII. "On *Holtenia*, a Genus of Vitreous Sponges." By Wyville Thomson, LL.D., F.R.S., Professor of Natural Science in Queen's College, Belfast.

(Abstract.)

During the deep-sea dredging cruise of H.M.S. 'Lightning' in the autumn of the year 1868, the dredge brought up, on the 6th of September, from a depth of 530 fathoms, in lat. 59° 36′ N., and long. 7° 20′ W., about 20 miles beyond the 100-fathom line of the Coast Survey of Scotland, fine, grey, oozy mud, with forty or fifty entire examples of several species of siliceous sponges. The minimum temperature indicated by several registering thermometers was 47°·3 F., the surface temperature for the several localities being 52°·5 F.

The mud brought up consisted chiefly of minute amorphous particles of carbonate of lime, with a considerable proportion of living *Globigerinæ* and other Foraminifera, and of the "coccoliths" and "coccospheres," so characteristic of the chalk-mud of the warmer area of the Atlantic. The sponges belonged to four genera; one of these was the genus *Hyalonema*, previously represented by the singular glass-rope sponges of Japan and the coast of Portugal, and the other three genera were new to science. One of these latter was the subject of the paper.

Associated with the sponges were representatives, usually of a small size, of the Mollusca, the Crustacea and Annelides, the Echinodermata, and the Cœlenterata, with numerous large and remarkable rhizopods. Many of the higher invertebrates were brightly coloured and had eyes.

Four nearly perfect specimens of the sponge described in the memoir were procured.

Holtenia, n. g.†

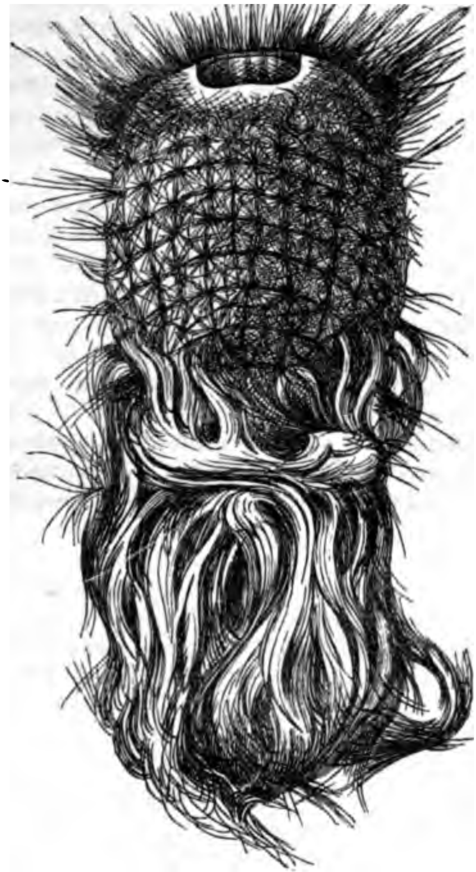
H. Carpenteri, n. sp.

The body of the sponge is nearly globular or oval. Normal, and

* Comptes Rendus, vol. liv. p. 1241.

† The genus is named in compliment to M. Holten, Governor of the Faroe Islands, and the species is dedicated to Dr. W. B. Carpenter, V.P.R.S., with whom the author was associated in the conduct of the expedition.

apparently full-grown specimens are from 9″ to 1′ 1″ in length, and from 7″ to 9″ wide. The outer wall consists of an open, somewhat irregular, but



very elegant network, whose skeleton is made up of large separate siliceous spicules. These spicules are formed on the hexradiate stellate type; but usually only five rays are developed, the sixth ray being represented by a tubercle. To form the framework of the external wall, the four secondary branches of the spicule spread on one plane, the surface of the sponge, while the fifth or azygous branch dips down into the sponge-substance. This arrangement of the spicules gives the outer surface of the sponge a distinctly stellate appearance, the centres of the stars being the point of radiation of the secondary branches of the spicules. These quinque-radiate spicules measure about 1″ 5‴ from point to point of the cross-like secondary branches, and the length of the azygous arm is from 7·5‴ to 1″.

Smaller stars, formed by the radiation of smaller spicules of the same class, occupy the spaces between the rays of the larger stars.

The rays of each star bend irregularly, and meet the rays of the spicules forming the neighbouring stars. The rays of the different spicules thus run along for some distance parallel to one another, and are held together by a layer of elastic sarcode, which invests all the spicules and all their branches. Between the rays of the spicules, over the whole surface, the sarcode forms an ultimate and very delicate network, its meshes defining minute inhalent pores.

At the top of the sponge there is a large osculum, about 3″ in diameter, which terminates a cylindrical cavity, which passes down vertically into the substance of the sponge to a depth of 5″ 5‴. The walls of this oscular cavity are formed upon the same plan as the external wall of the sponge; and the stars, which are even more conspicuous than those of the outer wall, are due to the same arrangement of spicules of the same form. The ultimate sarcode network is absent between the rays of the stars of the oscular surface.

The sponge-substance, which is about 2″ in thickness between the oscular and the outer walls, is formed of a loose vacuolated arrangement of bands and rods of greyish consistent sarcode, containing minute disseminated granules and groups of granules of horny matter, and minute endoplasts.

Towards the outer wall of the sponge the sarcode trabeculæ are arranged more symmetrically, and at length they resolve themselves into distinct columns, which abut against and support the centres of the stars, leaving wide, open, anastomosing channels between them. The sarcode of the outer wall, and that of the wall of the oscular cavity, is loaded with minute spicules of two principal forms, quinqueradiate spicules with one ray prolonged and feathered, and minute amphidisci.

Over the lower third of the body of the sponge, fascicles of enormously long delicate siliceous spicules pass out from the sarcode columns of the sponge-body in which they originate, through the outer wall, to be diffused to a distance of not less than half a metre in the mud in which the sponge lives buried; and round the osculum and over the upper third of the sponge, sheaves of shorter more rigid spicules project, forming a kind of fringe.

The author referred all the sponges which were found inhabiting the chalk-mud to the Order Porifera Vitrea, which he had defined in the 'Annals and Magazine of Natural History' for February 1868. This order is mainly characterized by the great variety and complexity of form of the spicules, which may apparently, with scarcely an exception, be referred to the hexradiate stellate type, a form of spicule which does not appear to occur in any other order of sponges. The genus *Holtenia* is nearly allied to *Hyalonema*, and seems to resemble it in its mode of occurrence. Both genera live imbedded in the soft upper layer of the chalk-

mud, in which they are supported,—*Holtenia* by a delicate maze of siliceous fibres, which spread round it in all directions, increasing its surface without materially increasing its weight,—*Hyalonema* by a more consistent coil of spicules, which penetrates the mud vertically and anchors itself in a firmer layer.

It appeared to the author and to Dr. Carpenter, who had had their attention specially directed to this point as bearing upon the continuity and identity of some portions of the present calcareous deposits of the Atlantic with the cretaceous formation, that the vitreous sponges are more nearly allied to the *Ventriculites* of the chalk than to any recent order of Porifera. They are inclined to ascribe the absence of silica in many ventriculites, and the absence of disseminated silica in the chalk generally, to some process, probably dialytic, subsequent to the deposit of the chalk, by which the silica has been removed and aggregated in amorphous masses, the chalk flints.

The Vitreous Sponges along with the living Rhizopods and other Protozoa which enter largely into the composition of the upper layer of the chalk-mud, appear to be nourished by the absorption through the external surface of their bodies of the assimilable organic matter which exists in appreciable quantity in all sea-water, and which is derived from the life and death of marine animals and plants, and in large quantity, from the water of tropical rivers. One principal function of this vast sheet of the lowest type of animal life, which probably extends over the whole of the warmer regions of the sea, may probably be to diminish the loss of organic matter by gradual decomposition, and to aid in maintaining in the ocean, the "balance of organic nature."

XVIII. "An Inquiry into the Variations of the Human Skull, particularly in the Antero-posterior Direction." By John Cleland, M.D., Professor of Anatomy and Physiology, Queen's College, Galway. Communicated by Dr. Allen Thomson. Received June 15, 1869.

(Abstract.)

1. A method of notation is suggested by which material sufficient for the formation of a perfectly accurate diagram of a skull may be registered by means of a line or two of figures. This is accomplished by marking the vertical and horizontal distance of a number of points from the postauricular depression.

2. The longest base-lines, from fronto-nasal suture to back of foramen magnum, are found in savage skulls. This base-line is distinctly longer in males than females; and the proportion which the arch bears to the base-line is greater in children than in the adult. In the Irish, the base-line is short, and the arch extensive.

3. The mesial base being considered in three parts, viz. length of

foramen magnum, orbital length or profile distance of fronto-nasal suture from foramen opticum, and the foramino-optic line uniting the other lines together, it is found that the long base-line of savage skulls depends both on amount of orbital length and on long foramino-optic line.

4. The angle at which the line of orbital length lies to the foramen magnum is distinguished as the cranial curvature. This angle in adult Europeans on an average exceeds 180°, and in negro and other savage types falls short of that amount. It is also less in infants than in adults, and greater in females than in males. But the variation of the angles at which the foramen magnum and orbital depth respectively lie to the foramino-optic line, is much greater than the variation in cranial curvature; therefore the two angles mentioned are in a certain degree of mutual relation; and according to their size, the base may be termed "steep" or "level." The infant base is much more level than the adult male base; the levelness of childhood sometimes persists in the female.

5. The different regions of the arch do not grow equally. The parietal region reaches its greatest predominance in the last month of fœtal life, and after birth the frontal region grows most rapidly, and the occipital region next most rapidly. There is no foundation whatever, so far as mesial measurements are concerned, for the supposition that the lower races of humanity have the forehead less developed than the more civilized nations. Neither is it the case that the forehead in the lower races slopes more backwards on the floor of the anterior cranial fossa than it does in others.

6. The local prominence of different parts of the arch of the skull being measured by means of the angles joining lines passing from point to point in the arch, it is shown that the angles furnish a means of collecting various precise details with regard to national characteristics of form, from which important results may be expected if the plan be worked on an extensive scale. Flatness of the angle formed by lines from the extremities to the midpoint of the parietal arc is shown to be correlated with length of base-line.

7. As age advances, "gravitation changes" take place, the base being driven in and the lateral wall bulging out, the forehead becoming more retreating, and the condyles flat.

8. It is sought to be shown that if Dolichocephali and Brachycephali are to continue to be a natural and not an artificial division of skulls, the distinction must be based on the various characters pointed out by Retzius, and not on the mere amount of the "cephalic index." The proportion of height to length, according to the writer, is more important than the proportion of breadth to length. He proposes that Hindoo skulls should be considered as belonging to a subdivision Brachycephali angustiores, and that the Germans should be considered as Dolichocephali latiores.

9. The value of "radial" measurements from the postauricular depression is tested, and it is shown that a classification of some value may be based on them, but that they are defective in consequence of the variability

of the position of the postauricular depression, in both vertical and horizontal direction, as compared with the front of the foramen magnum. That position varies in different races, and is affected by gravitation changes.

10. The position of greatest breadth varies according to the time of life, and as the spaces adjacent to the mesial and lateral roof-ridges are well filled or ill filled; and an hypothesis is advanced in explanation of this, and of the mesial ridge being prominent in savage skulls, although the ridge on the fœtal skull disappears in childhood.

11. Orthognathism and prognathism are shown to be concrete results of a variety of circumstances, some of them not connected with the anatomy of the face, as, for example, the degree of cranial curvature. The extent to which the face projects from underneath the skull must be measured by an angle contained between the fore part of the face and the floor of the anterior fossa only of the skull, the curves of the base of the skull further back having really nothing to do with the matter. This projection of the face is great in French skulls, considerable in Scotch, and small in Irish and German skulls.

12. The facial angle is affected by the height of the ear above the foramen magnum, while prognathism is not.

13. The condyles of the skull become more and more prominent in front from infancy to adult life, and thus tilt the skull more and more backwards. By this rotation balance is preserved, seeing that the fore part of the head and the face are the parts which proportionally increase in size as growth proceeds, and their increased proportion of weight is made up for by a greater amount being thrown behind the vertebral column. There is less tilting back in the female head than the male.

14. This principle is shown to be most important in Artistic Anatomy.

15. In the lower animals the cerebral curvature is of very different amount in different species, the most advanced animals having it greatest.

XIX. "Researches on Vanadium."—Part II. By Henry E. Roscoe, B.A., Ph.D., F.R.S. Received June 16. Read June 17, 1869.

(Abstract.)

On the Chlorides of Vanadium and Metallic Vanadium

In the first part of these researches ('Bakerian Lecture,' Phil. Trans. 1868, pt. i.) the author stated that the chlorides of vanadium, and probably also the metal itself, could be prepared from the mononitride, the only compound of vanadium not containing oxygen then known. The process for obtaining the mononitride described in the last communication was that adopted by Berzelius for preparing the substance which he conceived to be metal, but which in reality is mononitride. This method consists in the action of ammonia on the oxitri-chloride; but it cannot be

employed for the preparation of large quantities of nitride, owing to the violence of the action and consequent loss of material. The author, seeking for a more economical method, found that if the ammonium metavanadate (NH_4VO_3) be heated for a sufficiently long time at a white heat in a current of dry ammonia, pure vanadium mononitride remains behind. Analysis of a sample thus prepared gave 79·6 per cent. of vanadium and 20·2 per cent. of nitrogen, theory requiring 78·6 and 21·4 per cent. respectively. The mononitride may likewise be directly prepared by igniting vanadium trioxide (V_2O_3) in a current of ammonia at a white heat in a platinum tube, and also by subjecting the dichloride to the same treatment.

The Chlorides of Vanadium.—Three chlorides of vanadium have been prepared, viz. :—

Vanadium tetrachloride	VCl_4
Vanadium trichloride	VCl_3
Vanadium dichloride	VCl_2

1. *Vanadium Tetrachloride* VCl_4, molec. wt. = 193·3, V.D. = 96·6 (H = 1).—This chloride is formed as a dark reddish brown volatile liquid, when metallic vanadium or the mononitride is burnt in excess of chlorine. The first method adopted for the preparation of this chloride was to pass dry chlorine over the mononitride heated to redness; the whole of the nitride volatilizes and a reddish-brown liquid comes over. In one operation 44 grammes of the crude tetrachloride was thus prepared; the liquid is purified by distillation first in a current of chlorine and then in a stream of carbonic acid gas. On fractionating, the liquid was found to boil at 154° C. (corrected) under 760^{mm} of mercury. The second method depends upon a fact already noticed in the preceding communication, that the oxitrichloride ($VOCl_3$), prepared, according to the directions of Berzelius, by passing dry chlorine over a mixture of the trioxide and charcoal, possesses a port-wine colour instead of the canary-yellow tint of the pure substance. This dark colour is due to the formation of the tetrachloride of vanadium, and if the vapours of the oxitrichloride, together with excess of dry chlorine, be passed several times over a column of red-hot charcoal the whole of the oxygen of the oxichloride can be removed, and at last perfectly pure tetrachloride, boiling constantly at 154° is obtained. This reaction, it will be remembered, served first to demonstrate the existence of oxygen in the oxitrichloride. In each distillation of the tetrachloride a peach-blossom-coloured solid residue remained in the bulbs; this substance is vanadium trichloride, and it slowly burns away in excess of chlorine when heated, forming tetrachloride.

The composition of the tetrachloride was established by six well-agreeing analyses, made from several different preparations. The mean result is :—

		Calculated.		Found.
$V = 51·3$	......	26·54	......	26·87
$Cl_4 = 142·0$	.....	73·46	.....	73·02
193·3		100·00		99·89

Owing to the facility with which the tetrachloride splits up into trichloride and chlorine a solid residue was left in the vapour-density bulb, and the density of the vapour (at 219°) was found by Dumas's method to be 99·06 (or 6·86) instead of 96·6 (or 6·69). By volatilising the liquid in a small bulb, and allowing the vapours to pass into a large bulb already heated above the boiling-point of the liquid this deposition of trichloride was avoided, and the density was found to be 96·6 or 6·69 at 205°, and 93·3 or 6 48 at 215°, the last determination indicating that a partial decomposition into VCl_3 and Cl had occurred. The specific gravity of the liquid tetrachloride at 0° is 1·8584; it does not solidify at −18°, nor does it at this or any higher temperature undergo change of properties on treatment with chlorine. It not only undergoes decomposition on boiling, but at the ordinary atmospheric temperatures it splits up into VCl_3 and Cl. Tubes in which the liquid tetrachloride had been sealed up have burst by the pressure of the evolved chlorine. Thrown into water, the tetrachloride is at once decomposed, yielding a blue solution identical in colour with the liquid obtained by the action of sulphurous or sulphydric acids on vanadic acid in solution, and containing a *vanadous* salt, derived from the tetroxide V_2O_4. In order to prove that a vanadous salt is formed when the tetrachloride is thrown into water, the solution thus obtained was oxidized to vanadic acid by a standard permanganate solution. The calculated percentage of oxygen thus needed according to the formula $2VCl_4 + O + 4H_2O = V_2O_5 + 8HCl$ is 4·14; the percentage of oxygen found by experiment was 4·11.

The solution of the tetrachloride in water does not bleach; but if the vapour be led into water a liquid is obtained which bleaches litmus. Vanadium tetrachloride acts violently on dry alcohol and ether, forming deep-coloured liquids. The author is engaged upon the examination of this reaction.

Bromine and vanadium tetrachloride, sealed up and heated together, do not combine; on the contrary, trichloride is deposited. Hence it is clear that vanadium does not readily form a pentad compound with the chlorous elements.

2. *Vanadium Trichloride.*—$VCl_3 = 157·8$. The trichloride is a solid body, crystallizing in splendid peach-blossom-coloured shining tables, closely resembling in appearance the crystal of chromium sesquichloride. It is non-volatile in hydrogen, and, when heated in the air, it decomposes, glowing with absorption of oxygen, and forming the pentoxide. Heated in hydrogen the trichloride first loses one atom of chlorine, forming the dichloride (VCl_2), and afterwards, on exposure to a higher temperature, loses all its chlorine, leaving metallic vanadium as a grey lustrous powder.

The trichloride is extremely hygroscopic, deliquescing on exposure to air to a brown liquid. The trichloride is best prepared by the quick decomposition of the tetrachloride at its boiling point, or by its slow decomposition at the ordinary temperature of the air. The crystalline powder obtained by either of these methods only requires freeing from adhering tetrachloride by drying in carbon dioxide at 160° in order to yield good analytical results.

		Calculated.	Mean of 4 analyses.
V =	51·3	32·5	32·57
Cl_3 =	106·5	67·5	67·42
	157·8	100·0	99·99

The trichloride thrown into water does not at once dissolve; but, as soon as the crystals get moistened, a brown solution is formed, which becomes green on addition of a drop of hydrochloric acid, and contains a *hypovanadic* salt in solution. This green tint is identical with that got by reducing a solution of vanadic acid in presence of magnesium. According to the equation $2VCl_3+O_2+3H_2O=V_2O_5+6HCl$ the solution of the trichloride requires 10·14 per cent. of oxygen to bring it up to vanadic acid, whilst analysis showed that 10·1 per cent. was necessary. The specific gravity of the trichloride at 18° is 3·00.

3. *Vanadium Dichloride* $VCl_2=122·3$.—The dichloride is a solid crystallizing in fine bright apple-green micacious plates. It is prepared by passing the vapour of vanadium tetrachloride mixed with hydrogen through a glass tube heated to dull redness. If the heat be pushed further a blackish crystalline powder, consisting of a mixture of lower chloride and metal, is obtained. The dichloride, when strongly heated in hydrogen, loses all its chlorine, leaving vanadium in the metallic state in grey crystalline grains. Analysis gave :—

		Calculated.	Mean of 2 analyses.
VCl =	51·3	41·95	42·16
Cl_2 =	71·0	58·05	57·88
	122·3	100·00	100·00

Vanadium dichloride is extremely hygroscopic; when thrown into water a violet-coloured solution is formed, identical in tint with the liquid containing a *hypovanadous* salt obtained by reducing vanadic acid in solution in presence of zinc- or sodium-amalgam ; and like this latter liquid, the solution of dichloride in water bleaches strongly by reduction.

Oxidized by permanganate this liquid required 18·78 per cent. of oxygen (on the dichloride taken) to bring it up to vanadic acid, whereas the equation $2VCl_2+O_2+2H_2O=V_2O_5+4HCl$ requires 19·6 per cent. The specific gravity of vanadium dichloride at 18° is 3·23.

Metallic Vanadium $V=51·3$.—Although from what we now know of the characters of vanadium it appeared unlikely that any compound con-

taining oxygen would yield the metal by direct reduction, the author has repeated the experiments of other chemists on this subject, but without success. There is no doubt that the metal cannot be obtained by any of the processes described in the books. The only methods which promised possible results were :—

1. The reduction of a vanadium chloride (free from oxygen) in hydrogen gas, either with or without sodium.
2. The reduction of the mononitride at a white heat in hydrogen.

The first of these methods has proved to be successful, whilst the second does not appear to yield metal, inasmuch as the nitride exposed for $3\frac{1}{2}$ hours in a platinum tube to the action of hydrogen at a white heat, lost only 8 per cent., whereas it must lose 21·4 per cent. on conversion into metal.

Notwithstanding the apparent simplicity of the method, the author has found it exceedingly difficult to obtain the metal perfectly free from oxygen. This arises from the fact that whilst vanadium is quite stable at the ordinary temperature, it absorbs oxygen with the greatest avidity at a red heat, and that therefore every trace of air and moisture must be excluded during the reduction. Another difficulty consists in the preparation of the solid chlorides in large quantity and free from oxygen or moisture, as also in the length of time needed to reduce these chlorides in hydogen, during which time unavoidable diffusion occurs and traces of oxygen enter the tube. Again, the reduction can only be effected in platinum boats placed in a porcelain tube, as the metal acts violently on glass and porcelain, and tubes of platinum are porous at a red heat.

A description of the apparatus employed is then given, the main points being to guard against diffusion, and to introduce the powdered dichloride into the platinum boat in such a way that it shall not for an instant be exposed to moist air. After all precautions are taken the tube is heated to redness, torrents of hydrochloric acid come off, and the evolution of this gas continues for from 40 to 80 hours, according to the quantity of dichloride taken. After the evolution of any trace of hydrochloric acid has ceased to be perceptible, the tube is allowed to cool, and the boat is found to contain a light whitish grey-coloured powder, perfectly free from chlorine.

Metallic vanadium thus prepared examined under the microscope reflects light powerfully, and is seen to consist of a brilliant shining crystalline metallic mass possessing a bright silver-white lustre. Vanadium does not oxidize or even tarnish in the air at the ordinary temperature; nor does it absorb oxygen when heated in the air to 100°. It does not decompose water even at 100°, and may be moistened with water and dried *in vacuo* without gaining weight. The metal is not fusible or volatile at a bright red heat in hydrogen; the powdered metal thrown into a flame burns with the most brilliant scintillations. Heated quickly in oxygen it burns vividly, forming the pentoxide; but slowly ignited in air it first glows to form a brown oxide (possibly V_2O), and then again ab-

sorbs oxygen and glows with formation of the black trioxide and blue tetroxide till it at last attains its maximum degree of oxidation. The specific gravity of metallic vanadium at 15° is 5·5. It is not soluble in either hot or cold hydrochloric acid ; strong sulphuric acid dissolves it on heating, giving a yellow solution ; hydrofluoric acid dissolves it slowly with evolution of hydrogen ; nitric acid of all strengths acts violently on the metal, evolving red nitrous fumes and yielding a blue solution ; fused with sodium hydroxide the metal dissolves with evolution of hydrogen, a vanadate being formed.

One sample yielded on oxidation a percentage increase of 77·94, whereas that calculated from metal to pentoxide is 77·98. Another preparation gave a percentage increase of 70·8, showing the presence of a small quantity of oxide. On treatment in a current of chlorine metallic vanadium burns and forms the reddish black tetrachloride ; heated in a current of pure nitrogen the mononitride is formed.

The properties of the compounds of vanadium with silicon and platinum are then described in the memoir.

XX. " On *Palæocoryne*, a genus of the Tubularine Hydrozoa from the Carboniferous formation." By Dr. G. Martin Duncan, F.R.S., Sec. Geol. Soc., and H. M. Jenkins, Esq., F.G.S. Received June 14, 1869.

(Abstract.)

Palæocoryne is a new genus containing two species, and belongs to a new family of the Tubularidæ. The forms described were discovered in the lower shales of the Ayrshire and Lanarkshire coal-field, and an examination of their structure determined them to belong to the Hydrozoa, and to be parasitic upon Fenestellæ. The genus has some characters in common with *Bimeria* (St. Wright), and the polypary is hard and ornamented. The discovery of the trophosome, and probably part of the gonosome of a tubularine Hydrozoon in the Palæozoic strata brings the order into geological relation with the doubtful Sertularian Graptolites of the Silurian formation, and with the rare medusoids of the Solenhofen stones.

XXI. Bakerian Lecture.—" On the Continuity of the Gaseous and Liquid States of Matter." By Thomas Andrews, M.D., F.R.S., &c. Received June 14, 1869.

(Abstract.)

In 1863 the author announced, in a communication which Dr. Miller had the kindness to publish in the third edition of his 'Chemical Physics,' that on partially liquefying carbonic acid by pressure, and gradually raising at the same time the temperature to about 88° Fahr., the surface of de-

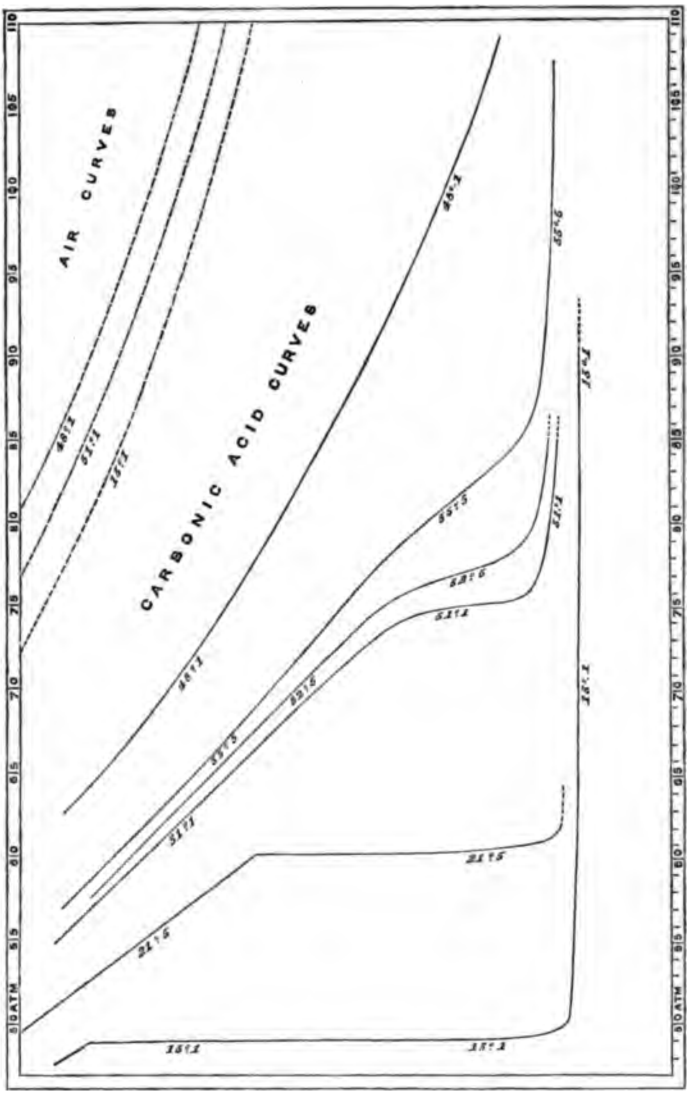
marcation between the liquid and gas became fainter, lost its curvature, and at last disappeared, the tube being then filled with a fluid which, from its optical and other properties, appeared to be perfectly homogeneous. The present paper contains the results of an investigation of this subject, which has occupied the author for several years. The temperature at which carbonic acid ceases to liquefy by pressure he designates the critical point, and he finds it to be 30°·92 C. Although liquefaction does not occur at temperatures a little above this point, a very great change of density is produced by slight alterations of pressure, and the flickering movements, also described in 1863, come conspicuously into view. In this communication, the combined effects of heat and pressure upon carbonic acid at temperatures varying from 13° C. to 48° C., and at pressures ranging from 48 to 109 atmospheres, are fully examined.

At 13°·1 C., and under a pressure, as indicated approximately by the air manometer, of 48·89 atmospheres, carbonic acid, now just on the point of liquefying, is reduced to $\frac{1}{80 \cdot 9}$ of the volume it occupied under one atmosphere. A slight increase of pressure, amounting to $\frac{1}{20}$ of an atmosphere, which has to be applied to condense the first half of the liquid, is shown to arise from the presence of a trace of air ($\frac{1}{1000}$ part) in the carbonic acid. After liquefaction, the volume of the carbonic acid, already reduced to about $\frac{1}{470}$ of its original volume, continues to diminish as the pressure augments, and at a much greater rate than in the case of ordinary liquids. Similar results were obtained at the temperature of 21°·5. A third series of experiments was made at 31°·1, or 0°·2 above the critical point. In this case the volume of the carbonic acid diminished steadily with the pressure, till about 74 atmospheres were attained. After this, a rapid but not (as in the case of liquefaction) abrupt fall occurred, and the volume was diminished to one-half by an additional pressure of less than two atmospheres. Under a pressure of 75·4 atmospheres, the carbonic acid was reduced to $\frac{1}{311}$ of its original volume under one atmosphere. Beyond this point it yielded very slowly to pressure. During the stage of rapid contraction there was no evidence at any time of liquefaction having occurred, or of two conditions of matter being present in the tube. Two other series of experiments were made, one at 32°·5, the other at 35°·5, with the same general results, except that the rapid fall became less marked as the temperature was higher. The experiments at 35°·5 were carried as far as 107 atmospheres, at which pressure the volume of carbonic acid was almost the same as that which it should have occupied if it had been derived directly from liquid carbonic acid, according to the law of the expansion of that body for heat.

The last series of experiments was made at 48°·1, and extended from 62·6 to 109·4 atmospheres of pressure. The results are very interesting, inasmuch as the rapid fall exhibited at lower temperatures has almost, if not altogether, disappeared, and the curve representing the changes of volume approximates closely to that of a gas following the law of Mariotte.

The diminution of volume is at the same time much greater than if that law held good.

The results just described are represented in a graphical form in the figure given below. Equal volumes of air and carbonic acid, measured at



0° C. and 760 millimetres, when compressed at the temperatures marked on each curve, undergo the changes of volume indicated by the form of the curve. The figures at the top and bottom indicate the approximate pres-

sures in atmospheres; the volumes of the gas and air are measured upwards from the dotted horizontal line.

The author has exposed carbonic acid, without making precise measurements, to higher pressures than any of those mentioned, and has made it pass, without breach of continuity, from what is universally regarded as the gaseous to what is, in like manner, universally regarded as the liquid state. As a direct result of his experiments, he concludes that the gaseous and liquid states are only widely separated forms of the same condition of matter, and may be made to pass into one another by a series of gradations so gentle that the passage shall nowhere present any interruption or breach of continuity. From carbonic acid as a perfect gas, to carbonic acid as a perfect liquid, the transition may be accomplished by a continuous process, and the gas and liquid are only distant stages of a long series of continuous physical changes. Under certain conditions of temperature and pressure, carbonic acid finds itself, it is true, in a state of instability, and suddenly passes, without change of pressure or temperature, but with the evolution of heat, to the condition which, by the continuous process, can only be reached by a long and circuitous route.

The author discusses the question, as to what is the condition or state of carbonic acid, when it passes at temperatures above 31° from the ordinary gaseous state down to the volume of the liquid, without giving any evidence during the process of the occurrence of liquefaction, and arrives at the conclusion that the answer to this question is to be found in the intimate relations which subsist between the gaseous and liquid states of matter. In the abrupt change which occurs when the gases are compressed to a certain volume at temperatures below the critical point, molecular forces are brought into play, which produce a sudden change of volume, and during this process it is easy to distinguish, by optical characters, the carbonic acid which has collapsed from that which has not changed its volume. But when the same change is effected by the continuous process, the carbonic acid passes through conditions which lie between the ordinary gaseous and ordinary liquid states, and which we have no valid grounds for referring to the one state rather than to the other.

Nitrous oxide, hydrochloric acid, ammonia, sulphuric ether, sulphuret of carbon, all exhibited critical points when exposed under pressure to the required temperatures.

The author proposes for the present arbitrary distinction between vapours and gases, to confine the term vapour to gaseous bodies at temperatures below their critical points, and which therefore can be liquefied by pressure, so that gas and liquid may exist in the same vessel in presence of one another.

The possible continuity of the liquid and solid states is referred to as a problem of far greater difficulty than that which forms the subject of this communication, and as one which cannot be resolved without careful investigation.

XXII. "The Physiological Action of Atropine, Digitaline, and Aconitine on the Heart and Blood-vessels of the Frog." By Frederic B. Nunneley, M.D. Lond. Communicated by Dr. Bastian. Received June 16, 1869.

(Abstract.)

These experiments were undertaken with the view of determining more exactly the physiological action of atropine, digitaline, and aconitine on the heart and blood-vessels, by methods of experiment which have hitherto been little followed out.

My experiments on atropine have led me to the conclusion that it exerts no action on the blood-vessels, a result which differs from that of Mr. Wharton Jones and of M. Meuriot. This opinion was adopted after a lengthened examination of the natural circulation in the frog's web, and of the numerous spontaneous changes which it undergoes, and also of those which are the result of the slightest irritation. It is some of these changes which have, I think, been assigned by the observers just named to the special action of atropine.

Different opinions have been entertained with regard to the action of digitaline on the heart; the result of my observations is given below. Aconitine has also a very marked action on the heart, the opposite of that of digitaline; its physiological effects are stated.

In actually conducting the experiments, the general symptoms of a poisonous dose were first observed, so as to show the period at which the heart lost its vitality in relation to the rest of the body.

Next, the visible effect produced on the heart, exposed *in situ*, was noted as regards the quality, frequency and rhythm of its contractions; and also the alteration seen to occur in a heart removed from the body and immersed in a solution of the alkaloid.

Lastly, the effect on the blood-vessels of the web, was examined with the aid of the microscope.

The results obtained have been thrown into the form of conclusions.

Atropine, digitaline, and aconitine do not produce any effect on the vessels of, or circulation in, the frog's web, whether locally applied in the form of solution, or injected under the skin at a distant part, so as to influence the animal generally; in the latter case, as they tend to impair or abolish the functions of the heart, the circulation necessarily undergoes secondary changes; but these alterations do not occur until the *heart* is visibly affected.

Atropine.—1. A few minutes after a dose of about $\frac{1}{20}$ gr., the frog becomes quiet, sinks down on the plate containing it and makes ineffectual efforts to jump, the respiratory movements cease and it dies in from $1\frac{1}{2}$ to 3 hours. On exposing the heart, it is found beating, and the contractions continue for some hours.

2. The action of atropine on the heart is neither considerable nor ener-

getic, a progressive weakening of its power being the most prominent visible effect. The heart continues to beat for some time after the manifestations of life in the rest of the animal have disappeared; finally it slowly dies itself, the ventricle being left in a state of relaxation; this occurs at the end of ten, twelve, or several more hours.

3. The heart's contraction gradually decreases in frequency and there is no primary acceleration.

The rhythm of the heart's action is not interfered with; the auricles continue to beat for some time after the ventricle has ceased to do so.

4. When the heart is removed from the body and immersed in a solution of sulphate of atropine, it ceases to contract in about the same time that a heart does placed in water; its appearance does not undergo any change.

5. The pupil of the frog's eye is not dilated by atropine, either when locally applied or injected under the skin.

6. The lymphatic hearts cease to contract long before the blood-heart.

Digitaline.—1. After the injection of about $\frac{1}{80}$ gr. under the skin, the frog at first jumps about, then becomes quiet, sinks down on the plate, cannot be easily roused and dies in about from twenty to forty minutes. Sometimes the frog has paroxysms of gasping movements, lasting from twenty to fifty seconds, in which it holds its mouth wide open, leaning on its fore paws. These attacks are paroxysmal, whilst the embarrassment of the heart is continuous. On opening the frog, the heart is found motionless and usually unirritable, the ventricle being small and pale.

Where digitaline if put into the mouth it causes a great secretion of fluid; in cats the salivation is very marked.

2. Digitaline acts with great energy on the heart, throwing it into violent and disorderly contractions which quickly end in a cessation of movement. The first visible effect occurs a short time after the injection under the skin, and consists in a diminished range of the heart's movements; but the most marked alteration is a certain embarrassment and loss of smoothness in the heart's contractions, as if there were a want of coordination in the contractions of the individual fibres.

The ventricular systole presents a peculiar appearance and takes a longer time for its performance than in health; it appears to travel along, squeezing the heart up, as it were, and forcing the blood into one spot, which becomes bright red and projecting; at the same time there are prominent muscular bundles on the surface of the ventricle giving it an irregular motion.

During diastole, the ventricle does not everywhere assume a red colour, but one or more irregular red spots appear as if it were so firmly contracted as only to permit the entrance of a small quantity of blood. These spots become smaller and smaller, until at last the ventricle is left very pale, strongly contracted and motionless, whilst the auricles are distended with blood.

3. The frequency of the heart's contractions is not increased, but is progressively diminished.

4. The functions of the heart are abolished very early, voluntary power, as shown by the frog's ability to jump about with its heart motionless and contracted, reflex acts and the contractility of the lymphatic hearts surviving the death of that organ.

5. The rhythm of the heart's contractions is but little interfered with until near the end, when they become irregular in frequency and force.

6. Immersion of the heart, removed from the body, in a solution of digitaline causes the ventricle to become somewhat uneven in outline; the contractions get weak and infrequent and at last cease, sooner by some minutes than in a heart placed in water; the appearance of the ventricle when it has ceased to beat, presents little that is peculiar, except that it often looks uneven.

Aconitine.—1. After the injection under the skin of about $\frac{1}{50}$ gr. the frog jumps about for a few minutes and then either sinks down on the plate, or else falls over on to its back, as if it had lost both muscular power and the ability to direct its movements; in either case it dies in from twenty to forty minutes. If the dose is larger, the frog falls as if stunned, almost immediately after the injection; from this state it partially revives, but dies at the end of a few minutes. On exposing the heart it is found beating, but rather feebly, and continues to do so for one or two hours.

2. When the heart is exposed *in situ*, aconitine is seen to have a very distinct and powerful action upon it. Its contractile power is quickly impaired giving rise to a peculiar perversion of rhythm. The interval of relaxation of the ventricle is considerably lengthened, whilst the auricles go on contracting regularly, the consequence is that the ventricle becomes more and more distended with blood, at last a limited part of it contracts, this area of contraction increases with each systole until, in time, the blood is forced, at each contraction, to one part of the heart which projects as a nodule, in a short time the whole ventricle becomes involved in contraction and empties itself in the ordinary way; soon after the ventricle again enters into a state of relaxation when the same series of acts is repeated. Finally, the ventricle is left large, dark and distended with blood, in a condition exactly contrary to that of a heart arrested by digitaline.

3. When a heart is removed from the body and immersed in a solution of aconitine it ceases to beat a little sooner than one does placed in water, but presents nothing peculiar in its appearance.

4. The frequency of the heart's pulsation is increased by a few beats at first, but in a short time there is a progressive diminution.

5. Although aconitine abolishes the functions of the heart in a comparatively short time, voluntary power, reflex acts and the contractility of the lymphatic hearts disappear some time before the blood-heart ceases to beat. The results obtained in about 170 experiments formed the basis for these conclusions.

XXIII. "Fourth and concluding Supplementary Paper on the Calculation of the Numerical Value of Euler's Constant." By William Shanks. Communicated by Professor Stokes, Sec. R.S. Received June 14, 1869.

When $n=10000$, we have

$1+\frac{1}{2}+\frac{1}{3}+\ldots\frac{1}{10000}=$

9·78760 60360 44382 26417 84779 04851 60533 48592 62945 57772 17183 89460 97673 221+

Log ϵ $10000+\frac{1}{20000}=$

9·21039 03719 76182 73607 19658 18737 45683 04044 05954 51509 19041 33305 21764 185+

Result of "Bernouilli's" =+

·00000 00008 33333 33250 00000 03968 25392 65873 02344 87732 37845 49617 88207 355, &c.

E=

·57721 56649 01532 86060 65120 90082 40243 10421 59335 93995 35988 05773 64116 391.

On comparing the value of E when n is taken 10000, with former values already given, we cannot but conclude that the limits assigned to the value of E in the Third Supplementary Paper have been confirmed, and that nothing more seems requisite as to the determining of the numerical value of this curious constant.

XXIV. "On the Refraction-Equivalents of the Elements." By J. H. Gladstone, Ph.D., F.R.S. Received June 17, 1869.

(Abstract.)

This paper is a continuation of the researches on refraction which have been already published by the author in conjunction with the Rev. T. Pelham Dale*.

It is divided into two parts—the data, and the deductions. The data consist of the refraction-equivalents of some simple and many compound bodies, calculated from the indices observed by various chemists and physicists, or by the author himself; together with a series of observations on about 150 salts in solution. The method of examining these, and the nature of the inference to be drawn from such experiments, have already been explained in the Proceedings of the Royal Society, 1868, pp. 440–444.

The deductions consist of a comparison of the evidence bearing on each elementary substance, beginning with carbon, hydrogen, and oxygen, which were in the first instance determined by Landolt. In the case of some elements all the means of calculation lead to the same number within probable errors of experiment; but in the case of others two or more

* Phil. Trans, 1863, p. 317.

different equivalents are indicated. Thus iron has one value in the ferrous and another in the ferric salts; and the more highly oxidized compounds of sulphur, phosphorus, arsenic, and nitrogen give different numbers from those given by their simpler combinations. The refraction-equivalent of potassium is estimated from a variety of sources, and the number thus arrived at is employed for the calculation of the other metals that give soluble salts, and for the radicals with which they are combined.

The following Table gives the general results of these deductions:—

Element.	Atomic weight.	Refraction-equivalent.	Specific refractive energy.
Aluminium	27·4	8·4	0·307
Antimony	122	24·5 ?	0·201 ?
Arsenic	75	15·4 (other values ?)	0·205
Barium	137	15·8	0·115
Boron	11	4·0	0·364
Bromine	80	15·3 In dissolved salts 16·9	0·191 or 0·211
Cadmium	112	13·6	0·121
Cæsium	133	13·7 ?	0·103 ?
Calcium	40	10·4	0·260
Carbon	12	5·0	0·417
Cerium	92	13·6 ?	0·148 ?
Chlorine	35·5	9·9 In dissolved salts 10·7	0·279 or 0·301
Chromium	52·2	15·9 In chromates 23 ?	0·305 or 0·441 ?
Cobalt	58·8	10·8.	0·184
Copper	63·4	11·6	0·183
Didymium	96	12·8 ?	0·133 ?
Fluorine	19	1·4 ?	0·073 ?
Gold	197	24·0 ?	0·122 ?
Hydrogen	1	1·3 In hydracids 3·5	1·3 or 3·5
Iodine	127	24·5 In dissolved salts 27·2	0·193 or 0·214
Iron	56	12·0 In ferric salts 20·1	0·214 or 0·359
Lead	207	24·8	0·120
Lithium	7	3·8	0·543
Magnesium	24	7·0	0·292
Manganese	55	12·2 In permanganate 26·2 ?	0·222 or 0·476 ?
Mercury	200	20·2 ?	0·101 ?
Nickel	58·8	10·4	0·177
Nitrogen	14	4·1 In high oxides 5·3	0·293 or 0·379
Oxygen	16	2·9	0·181
Palladium	106·5	22·4 ?	0·210 ?
Phosphorus	31	18·3 (other values ?)	0·590
Platinum	197·4	26·0	0·132
Potassium	39·1	8·1	0·207
Rhodium	104·4	24·2 ?	0·232 ?
Rubidium	85·4	14·0	0·164
Silicon	28	7·5 ? In silicates 6·8	0·268 ? or 0·243
Silver	108	15·7 ?	0·145 ?
Sodium	23	4·8	0·209
Strontium	87·5	13·6	0·155
Sulphur	32	16·0 (other values ?)	0·500
Thallium	204	21·6 ?	0·106 ?
Tin	118	19·2 ?	0·163 ?
Titanium	50	25·5 ?	0·510 ?
Vanadium	51·2	25·3 ?	0·494 ?
Zinc	65·2	10·2	0·156
Zirconium	89·6	21·0 ?	0·234 ?

The equivalents that have been deduced from only one compound, or of which the different determinations are not fairly accordant, are marked ? in the above Table.

The specific refractive energy of a body is in some respects worthy of more consideration than the refraction-equivalent, since, being only the refractive index minus 1 divided by the density, it is a physical property independent of chemical theories as to the atomic weight. Among suggestive facts are noticed the extreme energy of hydrogen; the existence of pairs of analogous elements having the same, or nearly the same, energy, —as bromine and iodine, arsenic and antimony, potassium and sodium, manganese and iron, nickel and cobalt; and that among the metals capable of forming soluble salts there is some connexion between their power to saturate the affinities of other elements, and their power to retard the rays of light.

XXV. "On the Structure of the Cerebral Hemispheres." By W. H. Broadbent, M.D., Lecturer on Physiology at St. Mary's Hospital Medical School, and Senior Assistant Physician to the Hospital, Physician to the Fever Hospital. Communicated by F. Sibson, M.D. Received June 17, 1869.

(Abstract.)

The object of the investigation has been twofold. First and chiefly, to endeavour to ascertain minutely the course of the fibres by which the convolutions of the hemisphere are connected with each other and with the crus and central ganglia.

Secondly, to endeavour to ascertain whether there is a constant similarity between the corresponding sides of different brains as compared with the opposite sides of the same brain; and should this be the case, to endeavour to trace the relation between any anatomical difference which might be discovered and such physiological difference as seems in the present state of our knowledge to be indicated by the association of loss of the faculty of language with disease of the *left* hemisphere rather than the right.

The present communication relates almost exclusively to the first branch of the investigation, and the method pursued has been to harden the brain by prolonged immersion in strong spirit, by which the fibres are rendered perfectly distinct and fairly tenacious, so that with care and patience their course and arrangement may be accurately ascertained.

Previous researches on the structure of the cerebrum have been mainly directed to the examination of the course and distribution of the fibres radiating from the crus and central ganglia, which have been assumed or supposed to occupy ultimately the axis of every convolution, the different convolutions being connected by fibres which crossed under the sulci from one to another. It is here shown that the commissural communication

between different parts of the hemisphere is much more extensive than has hitherto been described, and that the fibres more commonly run longitudinally in the convolutions than cross from one to another, while large tracts of convolutions have no direct connexion with the crus, central ganglia, or corpus callosum.

The preponderance of commissural over radiating fibres is indicated by a comparison of the sectional area of the latter as they issue from the central ganglia with the large surface of white matter displayed in the centrum ovale. The dissection by which this is shown in detail is begun on the under surface of the temporo- or occipito-sphenoidal lobe.

In this lobe the fibres are almost entirely longitudinal in their general direction. From near the apex fibres can be followed backwards in the two or three convolutions on the outer side of the gyrus uncinatus to near the centre of this surface of the lobe, where they end in the grey matter of a sort of lobule which I have ventured to call the collateral lobule. From the collateral lobule other fibres pass to the convolutions at the occipital extremity of the lobe, to convolutions on its outer side and to the calcarine end of the uncinate gyrus. These convolutions, comprising all those of the temporo-sphenoidal lobe except the gyrus uncinatus, the infra-marginal and parallel gyri, and the continuation of the two latter round the apex receive no fibres whatever from the crus, central ganglia or corpus callosum, but the ant. commissure spreads into them.

Beneath these is a beautiful plane of fibres which forms the floor of the descending cornu of the lateral ventricle, except at the anterior end; it forms the floor also of the ventricle at the entrance to the cornu, *i. e.* in the eminentia accessoria and of the posterior cornu; but here fibres of the C. callosum are mingled with those of the plane spoken of. This plane is formed as follows: along the axis of the lobe, in the hollow left by the removal of the superficial convolutions, runs a band of fibres from the apex to the posterior extremity; anteriorly this band contains numerous fibres, but in passing backwards they spread out towards the inner border of the lobe into a continuous lamina, which rests upon the lining membrane of the ventricle and its cornua. Some of the fibres run in the upper wall of the calcarine fissure to the postero-parietal lobule, others form a layer in the lower wall of this fissure, *i. e.* in the calcarine division of the gyrus uncinatus. The G. uncinatus remains as an elevation along the inner side of the shallow valley resulting from the dissection described, little encroached upon by it; its superficial fibres, however, must be removed to display the plane just mentioned. It incloses the cornu of the ventricle and the hippocampus, and is thus not a solid mass. Its fibres can be divided into two layers, a superficial set, the general direction of which is from the outer or collateral side anteriorly, backwards and inwards to the grey matter on its flat surface; and a deeper set, the fibres of which at the anterior part of the gyrus occupy its entire width, in passing backwards they converge, and near the inner border have a twisted arrangement, the inner fibres passing

beneath the outer to the grey matter of the hippocampus and to the splenium C. callosi, the outer fibres crossing over and reaching the upper wall of the calcarine fissure, in which they pass to the posteroparietal lobule and to the callosal gyrus.

The anterior enlarged extremity of the uncinate gyrus, sometimes called the uncinate lobule, is connected by bands of fibres with various parts; it is very firmly adherent to the subjacent structures, and when torn away leaves a patch of exposed grey matter, which has been named the internal grey nucleus. This is about in the same transverse line with the C. albicans, a little to the outer side of the optic tract.

By the removal of the uncinate lobule and gyrus fibres can be seen to pass from the apex of the lobe forwards in the fasciculus uncinatus, backwards and inwards along the roof of the cornu to the thalamus, and inwards to the grey nucleus.

On further dissection, which will consist in tracing the fibres from the apex backwards to various parts, and in removing little by little more of the convolutions along the outer edge of the lobe, and in a careful investigation of the parts about the calcarine fissure, the following appearances will be presented.

Along the axis of the lobe a longitudinal ridge with a slight convexity outwards, prominent posteriorly, subsiding anteriorly. On its inner side, from behind forwards, first the posterior cornu: next the outer wall of the ventricle, where the cornua enter it; this is formed by fibres curving directly backwards into the ridge from the thalamus (also from crus and corpus striatum, but more deeply), they are crossed transversely, however, by a thin lamina of fibres from the under surface of the splenium, which bend down from the roof of the ventricle and then curve forwards in the ridge: next the posterior end of the thalamus, which bends forwards round the crus, and gives off forwards from a pointed extremity the optic tract and laminæ of fibres on the outer side of this, which run above the roof of he cornu to the apex. Anteriorly this longitudinal ridge is continuous with the fasciculus uncinatus, and on its inner side are the internal grey nucleus, and more anteriorly the anterior perforated space between which the anterior commissure dips forwards and inwards in its canal.

On the outer side of the ridge fibres may be seen to start at the edge of the lobe, run inwards to the ridge, and curve forwards in it; to leave it again on its outer or inner side, or to pass with it to the fasciculus uncinatus.

A bundle of fibres taken up from the posterior part of the ridge would pass mainly to the thalamus; but some would proceed forwards in the ridge, and either turn outwards to some part of the inframarginal gyrus or apex, or inwards to the internal grey nucleus, or behind it. Others again go on in the F. uncinatus.

Fibres taken from the middle part of the ridge, and traced backwards, would mostly curve outwards to some part of the outer edge of the lobe,

but some would go to the tip; followed forwards, they spread out into a thin fan, and pass to the various points already indicated.

By repetition of this process the temporo-sphenoidal lobe will be exhausted, with the exception of a considerable lamina of fibres from the posterior part of the inframarginal gyrus, which passes backwards and inwards to the end of the fissure of Sylvius, round which it curves into the supramarginal gyrus, and another large band from the posterior end of the parallel gyrus, which curves upwards and turns forwards in the axis of the parietal lobe close behind the fibres which curve upwards from the corpus callosum to the margin of the longitudinal fissure.

It should be added that large bands of fibres run obliquely backwards in the parallel gyrus to the bottom of the sulcus of the same name, under which they turn to the inframarginal gyrus. When these are removed, the deep parallel sulcus is converted into a deep narrow valley.

The fasciculus uncinatus, in the dissection just described, has been seen to receive fibres from the occipital extremity of the hemisphere, and from various convolutions along its outer side, occipital, annectent, angular, parallel, and inframarginal; fibres are traceable into it also from the internal grey nucleus, these mostly lying beneath those from the convolutions, and it is probable that a few fibres from the thalamus and splenium find their way into it. As it emerges from under the temporo-sphenoidal lobe to cross the entrance to the fissure of Sylvius, it receives a considerable contribution from the overhanging apex of this lobe, and some from the uncinate lobule. Its general direction is forwards; but a superficial set of fibrse mainly from the apex of the temporo-sphenoidal lobe, passes inward as well as forwards, and spreads out mainly to the edge of the longitudinal fissure, passing under the olfactory sulcus; another lamina appears from beneath the edge of this, having a still more transverse direction, and its fibres go to the rostrum corporis callosi, and to the callosal gyrus, detaching the pointed origin of this convolution from the anterior perforated space. The fibres passing directly forwards spread out under the orbital convolutions to end in the grey matter around the edge of this lobule, some of the more superficial turning into one or two of the gyri at its posterior and outer margin. Deeper fibres run outwards as well as forwards, beneath the convolutions of the island of Reil to the posterior part of the inferior frontal gyrus; this is a tract of considerable size.

The convolutions of the orbitar lobule being entirely superficial to the radiating fibres of the fasciculus uncinatus, must be added to those on the under surface of the temporo-sphenoidal lobe as belonging to the class which have no direct central communications.

To this class also must be added, with a reservation to be noted presently, the gyri operti of the island. The summit and the anterior convolutions rest upon the part of the F. uncinatus which passes to the outer corner of the orbitar lobule and the third frontal gyrus, and the fibres arising in the grey matter of this portion of the island curve forwards across the fissure to

the same convolutions; the corner of the orbital lobule in fact is carried away entirely by the fibres from the fasciculus and island. In the same way fibres starting in the remaining convolutions of the island cross the fissure and turn up in the supramarginal gyrus, leaving the outer surface of the C. striatum perfectly smooth, and converting the Sylvian fissure into a deep wide valley. The wall of the C. striatum thus exposed consists of a lamina of fibres, which radiate in all directions from a small patch of grey matter laid bare at the middle and highest point of the eminence this ganglion forms as seen from this aspect; and it is possible that there may be here some sort of continuity or connexion between the grey matter of the C. striatum and the overlying part of the convolutions of the island. Except at this point, the convolutions are separated from the C. striatum by a very distinct plane of fibres.

The gyri operti are thus connected mainly with the supramarginal gyrus and its continuation along the anterior wall of the fissure. Some fibres, however, pass from the grey matter of the overhanging inframarginal gyrus near the apex into the corresponding part of the island, and about the grey nucleus exposed at the summit of the C. striatum deep fibres from the posterior extremity of the hemisphere and from the F. uncinatus seem to join both the nucleus and the overlying grey matter of the island.

The temporo-sphenoidal lobe having been gradually removed, and with it a great part of the occipital lobe, a stage of the dissection is reached at which the distribution of the fibres of the splenium C. callosi and the relations of the crus and central ganglia, as seen from the under aspect, may be conveniently described.

On the inferior surface of the posterior extremity of the C. callosum is seen a transverse flattened elevation, which may be compared to the rostrum at the anterior extremity on a smaller scale and adherent to the body of the great commissure. It would thus be looked upon as a recurved part of the C. callosum. In the middle line it is adherent, but the fibres it sends transversely outwards leave the C. callosum proper, and bend downwards so as to cross the floor of the ventricle instead of the roof; they pass to the hippocampus major and minor, which they contribute to form, and run across the eminentia accessoria, and along the floor of the posterior cornu.

The hippocampus minor is formed by the projection into the posterior cornu of the bottom of the calcarine fissure; but an incision through the bottom of the fissure into the cornu would not split up the hippocampus, but would leave it attached entire to the upper wall of the cornu. The fibres from the splenium, which contribute to the formation of the hippocampus minor, run longitudinally along it immediately beneath the lining membrane of the ventricle, and when reached by dissection from without present a delicate lamina in the form of a groove between two curved tracts passing backwards to the posterior extremity of the hemisphere, the upper from the C. callosum proper, the lower from its recurved process.

The hippocampus major may be briefly described as a curved groove or "gutter" (Gratiolet) of fibres, the upper border of which is formed by the posterior pillar of the fornix, while the lower is concealed by the gyrus uncinatus, the grey matter of which folds over it into the groove, and after reaching the bottom bends up the other wall for a short distance, forming the plicated "C. fimbriatum," or "Pli godronné." The outer surface of the case of fibres is smooth, and for the most part free in the descending cornu; it adheres to the inferior wall formed by the plane of fibres previously described, but can easily be detached. The course of the fibres forming the case or groove is from the lower edge backwards and upwards round the convexity to the upper edge, where they pass into the pillar of the fornix, or where the hippocampus joins the splenium, into the recurved process. Further details are given in the paper itself.

The fibres crossing the floor of the ventricle curve forward, apparently towards the apex, but are too few to be followed absolutely to their termination.

From the body of the C. callosum, at its posterior part, the fibres mostly radiate backwards and outwards into the cuneus and occipital lobe generally; but a considerable number on the under surface bend from the roof of the ventricle down its outer wall, across the longitudinal fibres from the thalamus, &c., and curve forwards in the ridge. A considerable proportion of these has been traced to the internal grey nucleus, others seem to pass forwards to the grey matter near the apex of the temporo-sphenoidal lobe.

The relations of the crus and great central ganglia may be described as follows. The crus, as it plunges into the hemisphere, is encircled on its inferior aspect by the optic tract; it then expands into a large fan of fibres, the edges of which are antero-posterior, the surfaces obliquely upwards and inwards, and downwards and outwards. The two great ganglia, the C. striatum and thalamus may be said to sit astride the anterior and posterior edge respectively of the fan, each having an intra- and extraventricular part, the C. striatum being much the larger, and situate above, as well as in front of the thalamus.

When the optic tract is removed, the groove in which it rests is seen to present fibres having the same general direction round the crus; they have been called by Gratiolet "l'anse du pédoncle," a term which may be translated by the expression "the collar of the crus." The most conspicuous part of the collar consists of fibres from the thalamus, which curve forward round the crus to end in the tuber cinereum, or run up in the wall of the third ventricle to the velum interpositum, &c. Within this fibres are seen to turn forwards from the posterior border of both crust and tegment of the crus, to end in the C. striatum, and anteriorly a considerable mass of fibres from the tegmentum curves with a bold sweep round the edge of the crust, and passes backwards and outwards into this same ganglion.

The extra-ventricular part of the thalamus is seen in the descending cornu curving round the crus. From its anterior pointed extremity it is continued onwards by the optic tract, and it sends fibres,—1. Forwards in the collar of the crus. 2. Forwards and outwards to the convolutions about the apex in a succession of laminæ, the deeper fibres passing more outwards than the superficial sets, and emerging from under them along the outer edge of the roof of the cornu. 3. From under the fibres which pass forwards, it sends backwards a large mass along the outer wall of the ventricle and posterior cornu to the occipital end of the hemisphere.

The extraventricular corpus striatum has been exposed on two sides; it forms a very large mass, and has a large rounded anterior end, while posteriorly it narrows to a tail-like extremity. The outer aspect forms an elongated eminence, rising out of the Sylvian valley, highest at the centre, subsiding towards each end; at the summit is the external grey nucleus, from which radiate fibres forwards, backwards, and outwards. Those passing forwards form a large bundle; they spread out into a fan, and proceed mainly to the third frontal convolution; those passing backwards accompany the fibres from the thalamus to the occipital extremity of the hemisphere; those passing outwards with varying degrees of obliquity descend the wall of the ganglion to the Sylvian valley; but instead of crossing it to the convolutions on the other side, as might be expected from the apparent continuity of the walls and floor, dip between the fibres of the floor, which are the radiating fibres of the crus issuing from the C. striatum, and pass to convolutions in the frontal lobe. A remarkable fact respecting the planes of radiating fibres which form the limiting wall of the C. striatum on this aspect is, that the fibres all seem to have their origin in the small patch of grey matter here called the external grey nucleus, and they come off clean from the mass of soft grey matter forming the body of the ganglion.

On the under surface of the C. striatum, which is flat, are seen the internal grey nucleus and the anterior perforated space, between which the anterior commissure passes outwards and backwards from the ventricle in a distinct canal to emerge on this surface. The external grey nucleus also appears in the outer border, and is about in the same transverse line as the C. albicans and internal grey nucleus, from which last it is only separated by a narrow band of longitudinal fibres. Here again the planes of fibres, which form the limiting wall of the ganglion, end in the grey nuclei, and seem to have no communication with the mass of soft grey matter they inclose.

The anterior edge of the fan-like expansion of the crus emerges from the large end of the C. striatum, and, properly speaking, divides the intra-ventricular C. striatum from the extra-ventricular division; the anterior perforated space, being on the inner side of the radiating fibres, belongs to the former.

Before the dissection of the fronto-parietal portion of the hemisphere is

described, a brief account is given of the intraventricular thalamus and C. striatum.

When the tænia semicircularis is removed, and the edge of the C. striatum pushed back, large rounded cords of fibres are seen radiating outwards in all directions from the thalamus with the fibres of the crus, posteriorly slender flat bands of fibres curve backwards from the narrowing extremity of the C. striatum to dip down between them (together with fibres apparently belonging to the tænia); they can be traced through the fan of radiating fibres to the extraventricular C. striatum. Anteriorly the soft grey matter of the C. striatum fills the spaces between the diverging cords; but no distinct origin of fibres in the mass of grey matter is here met with.

The plan of construction of the frontoparietal portion of the hemisphere seems to be as follows:—

The C. callosum divides into two main planes of fibres, one of which turns up to the margin of the great longitudinal fissure, the other passes onward to the supramarginal gyrus of the fissure of Sylvius. The radiating central fibres approach the under surface of these at the acute angle, and pass obliquely between them before the ascending and descending planes have well separated from each other, the central as well as the callosal fibres going mainly to the margins of the hemisphere. An angle is thus left along the axis of the frontal and parietal lobes, which is occupied by a vast longitudinal system of fibres, some of which have already been mentioned as entering this part of the hemisphere from the temporo-sphenoidal lobe. Large bands turn upwards and then forwards from the parallel and angular gyri, that from the parallel gyrus running forward close behind the ascending callosal lamina; other fibres turn forwards from the annectent gyri, and more anteriorly from the posteroparietal lobule; still further forwards some of these fibres coming from behind bend upwards, and end in the parietal convolutions; while others start in the same gyri, and pass forwards, the principle of construction being apparently simple, but the details extremely intricate. At the decussation the central and callosal fibres are worn into a compact inextricable mass, and the difficulty of following the different sets is increased by the fact that the central fibres are not transverse in direction like those of the C. callosum, but mostly very oblique backwards or forwards, as may best be seen by examining the bands radiating under the C. striatum from the thalamus; this necessitates corresponding obliquity in the fissures through which the central fibres penetrate the C. callosum. A few fibres from the under surface of the C. callosum turn inwards to the centres; but the statement of Gratiolet that all the fibres of this commissure can be traced from the central radiations on one side to the convolutions on the other, is not confirmed.

The detailed dissection of the parieto-frontal convolutions need not be given here. It will be sufficient to mention that the posteroparietal and

supramarginal lobules are connected by numerous bands of fibres, that the ascending parietal gyri have central and callosal fibres entering their extremities, the middle portion receiving comparatively few; the first, however, sometimes called the ascending frontal gyrus, seems to have numerous fibres from the centres and C. callosum along its entire length. The second frontal convolution sends bands of fibres obliquely to the two others, and has fewer radiating fibres than they have. When it is removed, fibres can be traced transversely across the valley left from the first to the third.

A few additional particulars are given respecting the arrangement and course of the fibres in the callosal and marginal gyri on the internal surface of the hemisphere, and the contrast between the thalamus and C. striatum as to structure and relations is pointed out, the thalamus sending large masses of fibres in every direction, chiefly with the radiating crus, the corpus striatum consisting of soft grey matter enclosed in fibrous planes which arise in the comparatively small grey nuclei, and have apparently no communication with the main body of the ganglion. The thalamus again does not seem to receive terminating ascending fibres, while both divisions of the crus give off numerous fibres, which are seen to end in the C. striatum.

The differences in naked-eye appearances indicate differences in the relations between cells and fibres in the two ganglia, the exact nature of which can be ascertained only by the microscope.

XXVI. "On the Rhizopodal Fauna of the Deep Sea." By WILLIAM B. CARPENTER, M.D., V.P.R.S. Received June 17, 1869.

(Abstract.)

The Author commences by referring to the knowledge of the Rhizopodal Fauna of the Deep Sea which has been gradually acquired by the examination of specimens of the bottom brought up by the Sounding-apparatus; and states that whilst this method of investigation has made known the vast extent and diffusion of Foraminiferal life at great depths,—especially in the case of *Globigerina-mud*, which has been proved to cover a large part of the bottom of the North Atlantic Ocean,—it has not added any new Generic types to those discoverable in comparatively shallow waters. With the exception of a few forms, which, like *Globigerina*, find their most congenial home, and attain their greatest development, at great depths, the general rule has seemed to be that *Foraminifera* are progressively dwarfed in proportion to increase of depth, as they are by a change from a warmer to a colder climate; those which are brought up from great depths in the Equatorial region bearing a much stronger resemblance to those of the colder-temperate, or even of the Arctic seas, than to the littoral forms of their own region.

The Author then refers to the recent researches of Prof. Huxley upon

the indefinite protoplasmic expansion which he names *Bathybius*, and which seems to extend itself over the ocean-bottom under great varieties of depth and temperature, as among the most important of the results obtained by the Sounding-apparatus.

By the recent extension of Dredging-operations, however, to depths previously considered beyond their reach, very important additions have been made to the Foraminiferal Fauna of the Deep Sea. Several new generic types have been discovered, and new and remarkable varieties of types previously known have presented themselves. It is not a little curious that all the *new* types belong to the Family LITUOLIDA,—consisting of Foraminifera which do not form a calcareous shell, but construct a "test" by the agglutination of sand-grains,—which was first constituted as a distinct group in the author's 'Introduction to the Study of the Foraminifera' (1862). The first set of specimens described seems referable to the Genus *Proteonina* of Prof. Williamson; but the test, instead of being composed (as in his specimens) of sand-grains, is constructed of sponge-spicules, cemented together with great regularity, so as to form tubes, which are either fusiform or cylindrical, being in the former case usually more or less curved, and in the latter generally straight. Of the genus *Trochammina* (Parker and Jones), many examples were found of considerable size, resembling Nodosarians in their free moniliform growth, but having their tests constructed of sand-grains very firmly cemented together, with an intermxure of fragments of sponge-spicules, which give a hispid character to the surface.—The Genus *Rhabdammina* of Prof. Sars is based on a species (the *R. abyssorum*) first obtained in his Son's dredgings, of which the test is very regularly triradiate, sometimes quadriradiate, and is composed of sand-grains very regularly arranged, and firmly united by a ferruginous cement. Not only was this type represented by numerous specimens in the 'Lightning' dredgings, but another yet more considerable collection was formed of irregularly radiating and branching tubes, which are composed of an admixture of sand-grains and sponge-spicules, united by ferruginous cement. These seem to originate in a "primordial chamber" of the same material, which extends itself into a tube that afterwards branches indefinitely. This type may be designated *R. irregularis*.—Of the protean Genus *Lituola* (Lamarck), a large form was met with, which bears a strong resemblance to the *L. Soldani* of the Sienna Tertiaries. Its nearly cylindrical test is composed of sand-grains very loosely aggregated together, forming a thick wall; and its cavity is divided by septa of the same material into a succession of chambers, arranged in rectilineal series, each having a central orifice prolonged into a short tube.—The Genus *Astrorhiza*, instituted a few years ago by Dr. O. Sandahl, was represented by a wide range of forms, referable to two principal types,—the one an oblate spheroid, with irregular radiating prolongations, the other more resembling a stag's horn, with numerous digitations,—passing into one another by insensible gradations. The composition of

[redacted]

its thick arenaceous test is exactly the same as that of the test of the *Lituola* found on the same bottom; but its cavity is undivided, and there is no proper orifice, the pseudopodial extensions having apparently found their way out between the sand-grains that formed the termination of the radiating extensions or digitations.—The Genus *Saccamina* (Sars) is characterized by a very regular spherical test, built up of large angular sand-grains strongly united by ferruginous cement, which are so arranged as to form a wall-surface well smoothed off externally, whilst its interior is roughened by their angular projections. The cavity is undivided, and is furnished with a single orifice, which is surrounded by a tubular prolongation of the test, giving to the whole the aspect of a globular flask.

The family MILIOLIDA, consisting of Porcellanous-shelled Foraminifera, was represented at the depth of 530 fathoms by a *Cornuspira foliacea* of extraordinary size; and at the depth of 650 fathoms by a series of *Biloculinæ*, of dimensions not elsewhere seen except in tropical or subtropical regions.

Of the family GLOBIGERINIDA a considerable number of forms presented themselves; but with the exception of the ordinary *Globigerina* and *Orbulina*, these were not remarkable either for number or size. The *Globigerina-mud* brought up in large masses by the Dredge, exhibited the same composition as had been previously determined by the examination of Soundings; but it included a large amount of animal life of higher types, whilst it seemed everywhere permeated by the protoplasmic *Bathybius* of Huxley, as described in the Author's "Preliminary Report." The *Globigerinæ* vary enormously in size; and the Author gives reason for the belief that this variation is not altogether the result of growth, but that many small as well as large individuals have (speaking generally) attained their full dimensions. He describes the sarcodic body obtained by the decalcification of the shell; and discusses the question whether (as some suppose) *Orbulina* is the reproductive segment of *Globigerina*, as to which he inclines to a negative conclusion. He describes the curious manner in which the shells of *Globigerinæ* are worked-up into cases for Tubicolar Annelids; of which cases several different types presented themselves, the Foraminiferal shells in some of them being combined with sponge-spicules.—A remarkably fine specimen of *Textularia* was met with alive, of which the porous shell was encased by sand-grains; this being laid open by section showed the sarcodic body of an olive-greenish hue, corresponding with that of the *Lituolæ* and *Astrorhizæ* also found alive.—Several Rotaline types presented themselves sparingly in the Globigerina-mud, which are specially characteristic of the Cretaceous Formation.

The family LAGENIDA was represented not merely by its smaller forms, but also by a large and beautiful living *Cristellaria*, that closely corresponds with one of the forms described by Fichtel and Moll from the Siennese Tertiaries, whilst even exceeding it in dimensions.

These results conclusively show that reduction in the Size of *Foraminifera*

cannot be attributed to increase of Pressure; since the examples of *Cornuspira*, *Biloculina*, and *Cristellaria* found at depths exceeding 500 fathoms, were *far larger* than any that are known to exist in the shallower waters of the colder temperate zone. But as these all occurred in the *warm area*, whose bottom-temperature indicates a movement of water from the Equatorial towards the Polar region, it is probable that their size is related to the *temperature* of their habitat, which is found to be in like relation to the general character of the Fauna of which they formed part. On the other hand, as we now know that the climate of the deepest parts of the ocean-bottom, even in Equatorial regions, has often (if not universally) Arctic coldness, the dwarfing of the abyssal *Foraminifera* of those regions is fully accounted for on the same principle.

Besides these examples of new or remarkable forms of *Foraminifera*, the 'Lightning' dredgings yielded some peculiar bodies, the examination of which would seem to throw light upon the obscure question of the mode of Reproduction in this group. One set of these are cysts, of various shapes and sizes, composed of sand-grains loosely aggregated, as in the tests of *Lituola* and *Astrorhiza*; which, when broken open, are found to be filled with aggregations of minute yellow spherules, not enclosed in any distinct envelope. These are supposed by the Author to be *reproductive gemmules* formed by the segmentation of the sarcodic body of a Rhizopod, in the same manner as 'zoospores' are formed in Protophytes by the segmentation of their endochrome. Of such segmentation he formerly described indications in the sarcodic body of *Orbitolites*; and corresponding phenomena have been witnessed by Prof. Max Schulze. But in another set of cysts, of similar materials but of firmer structure, bodies are found having all the characters of *ova*, with *embryos* in various stages of development. In none of these, however, does the embryo present characters sufficiently distinctive to enable its nature to be determined; and the hypothesis of the Foraminiferal origin of these bodies chiefly rests upon the conformity in the structure of the wall of the cysts with that of the tests of *Lituola* and *Astrorhiza*, and upon the improbability that such cysts should have been constructed by animals of any higher type.

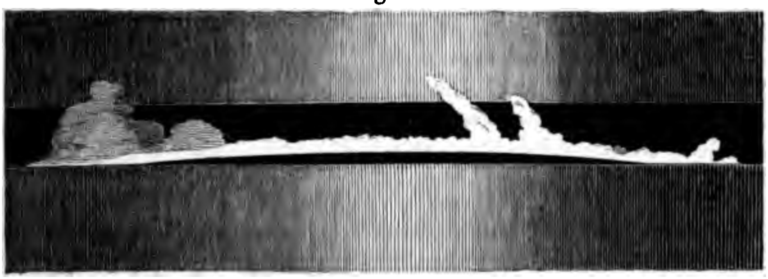
"Spectroscopic Observations of the Solar Prominences, being Extracts from a Letter addressed to Sir J. F.W. Herschel, Bart., F.R.S., by Captain Herschel, R.E., dated 'Bangalore, June 12th and 15th, 1869*.'" Communicated by Sir J. Herschel. Received July 19, 1869.

I have too little time to devote to lengthy descriptions, and so I send you a *sketch* of what I saw this morning (fig. 1). I have seen many such views during the last month, but none so distinct in outline as to-day—more by

* Received since the end of the Session.

token I have been waiting many days for sunshine since I brought my apparatus to its present state. I can only devote a single morning hour to it (before breakfast), but I make a little advance every day. The dark band across is a slit-image corresponding to C (aperture about 1′). Through the slit, as through a screen, is seen the *monochromatic* image of the "chro-

Fig. 1.



mosphere," a continuous envelope, which may be seen of nearly the same width everywhere. I estimate it at 20″ to 30″. Through the slit comes also a segment of the true limb, whose light is scattered up and down. It is wanting in C-light, and therefore within the C-image of the slit is seen a *dark* segment of the sun's limb, an inversion which nothing but "luminology" can enable one to understand. There are two classes of solar cloud* represented here; viz. the fleecy and the well defined: in both cases I have taken the liberty of seeing round the corner (so to speak), and giving the whole form as it might be seen by slightly pressing on the tube. With this exception, and a like one due to my having (to avoid confusion) retained a slightly stronger definition in *the central* parts than one actually obtains when so much of the limb is seen, there is, I believe, no exaggeration†. The whole picture, of course, is to be supposed seen on a background of pretty strong solar spectrum; and the vertical streaky light is to be supposed just short of dazzling—as strong, in fact, as the eye can bear without losing its power of distinguishing relative intensities.

A large group of spots (of which more anon) was visible just within the limb yesterday, but was not traceable to-day; it must have gone off near those horns.

The universality of the hydrogen envelope, now beyond dispute, would account satisfactorily for the dark C and F lines in solar light; and one might well rest content there; but the δ (bright) line is as persistent in this envelope as α and β (C and F); yet there is no *trace* of any absorption-line, corresponding to δ, in the solar light. The discrepancy between fact

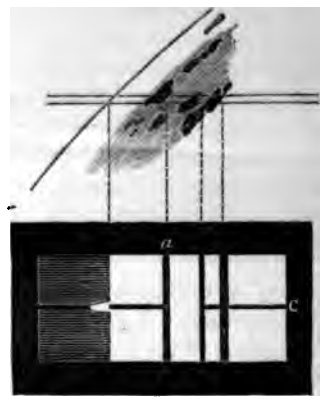
* The word has been objected to as inappropriate; but so long as we may speak of "clouds of smoke," "clouds of dust," "clouded vision," &c., it is crippling language to object on the score of inaccuracy.

† [The original sketch was in pencil, and the contrast between light and shade is exaggerated in the woodcut.—G. G. S.]

and theory covers something radical. What that may be remains to be *dis*covered. [The position of δ is 1015·3 (K) $\pm$ ·8.]

On the 10th I remarked (and observed till perfectly certain) that the C-line *on the disk* varied sensibly in strength; and at one place, which, I believe, corresponded to the penumbra of a spot-group, near the limb there was a *total* absence of the line, and a strong suspicion of a reversal (fig. 2). Faculæ were noticed round about, especially between the group and limb; but there was nothing of the kind visible where the hiatus in the C-line seemed to indicate. The hiatus extended, as did also the suspected *bright* part *within* the spot-sp. (*a*).

Fig. 2.



The observation was repeated on the 11th (yesterday) with the same result, except that the *bright* part of the line was not noticed. To-day (the 12th) the spot was round the corner. On no other spot examined was anything so decided seen, but the suppression of the dark line has been more than suspected elsewhere.

Lastly, I detected to-day, and put beyond doubt, that the bright accumulations on the disk (which I believe are the so-called "faculæ") give a continuous spectrum! I first noticed that every now and then there were bright streaks up and down the spectrum as the slit passed over the disk (or *vice versâ*). It is no easy thing in general to identify *certainly* the exact source of light whose spectrum you see; but in one case I had a spot near the limb, and one of these luminous streaks between the two; so, knowing the *direction* of the slit, it *was* easy, on removing the spectroscope, to determine precisely from what point of the disk the light in question emanated. In this case it was clear that it proceeded from a facula in that region.

I do not pretend to speculate on the constitution of the sun's surface, but here are three facts which require explanation:—

(1) A luminous line in the envelope corresponds with no visible line of absorption in the solar spectrum.

(2) The absorption is absent in an (apparently) penumbral region,

(3) The facula spectrum is an intensified solar spectrum.

June 15th. A Number of 'Scientific Opinion' has just been lent me, in which I see a notice of a paper, "A fourth communication by Mr. N. Lockyer" to the Royal Society. It tells me (what I might have expected) that I am just two months behind in all that I am seeing. However that may be, the

sights themselves are so beautiful and interesting that no other incentive is needed. This morning I showed —— a magnificent prominence upwards of 3' in height; and she testifies that my sketches do not do them justice.

The instrument I am now using is the Royal Society's spectroscope as fitted up for the eclipse; but I have increased the dispersion nearly threefold by inserting four compound prisms (extracted from the hand-spectroscopes). These amount to 7 inches of glass and sixteen surfaces; so you may imagine that there is some loss of light and definition. I have also had to shorten the focal distance (and therefore diminish the magnifying-power) by interposing a hand-telescope's object-glass—an additional obstruction and complication. I lost a great deal of fine weather (of which I get very little now) while trying to perfect this arrangement. I can still further increase the dispersion (without much loss of definition for *monochromatic light*) by turning the main prism, and so departing from the position of minimum deviation. But this is a resource which I keep to go on with when I tire of the advantage I have gained already.

The long train of compound prisms (as at present arranged) unfortunately bars me from the violet end of the spectrum. This is unfortunate, as it would be in the highest degree interesting to compare the α and γ images. Some day I shall get impatient, pull the whole affair to pieces, and arrange afresh with this object. As it is, I have to be very chary of quitting beaten ground, as we boast of no instrument-makers here!

I wish I had time to write fully and connectedly on the subject. It is only necessary to put people on the track. It is one easily followed, and will amply repay any expenditure in arranging prisms to get a maximum dispersion, for there is any amount of light.

XXVII. "Some Experiments with the Great Induction Coil at the Royal Polytechnic." By John Henry Pepper, F.C.S., Assoc. Inst. C.E. Communicated by J. P. Gassiot, Esq. Received June 12, 1869.

The length of the coil from end to end is 9 feet 10 inches, and the diameter 2 feet; the whole is cased in ebonite; it stands on two strong pillars covered with ebonite, the feet of the pillars being 22 inches in diameter. The ebonite tubes &c. are the largest ever constructed at Silver-Town Works.

The total weight of the great coil is 15 cwt., that of the ebonite alone being 477 pounds.

I am indebted to Mr. Apps for the following details. The primary wire is made of copper of the highest conductivity, and weighs 145 lbs.; the diameter of this wire is ·0925 of an inch, and the length 3770 yards. The number of revolutions of the primary wire round the core of soft iron is 6000, its arrangement being 3, 6, and 12 strands.

The total resistance of the primary is 2·201400 British Association units; and the resistances of the primary conductors are, respectively, for three strands ·733800 B.A.U., six ·366945 B.A.U., twelve ·1834725 B.A.U.

The primary core consists of extremely soft straight iron wires 5 feet in length, and each wire is ·0625 of an inch in diameter. The diameter of the combined wires is 4 inches, and their weight is 123 lbs.

The secondary wire is 150 miles in length; it is covered with silk throughout; and the average diameter is ·015 of an inch.

The total weight of this wire is 606 lbs., and the resistance 33,560 B.A. units. The insulation throughout is greater by 95 per cent. than the strain upon the coil during its action. The secondary wire is insulated from the primary by means of an ebonite tube ½ an inch in thickness and 8 feet in length.

The length of the secondary coil is 54 inches, the diameter is 19 inches, and, without the internal ebonite tube containing the primary wire and iron core, it is a hollow cylinder 19 inches in diameter and 6 inches thick.

The condenser, made in the usual manner with sheets of varnished paper and tinfoil, is arranged in six parts, each containing 125 superficial feet, or 750 square feet of tinfoil in the whole.

A large and substantially made Contact-breaker, detached from the great coil and worked by an independent electromagnet, was constructed, and worked very well with a comparatively moderate power of 10 or 20 large Bunsen's cells; when, however, the battery was increased to 30 or 40 cells, it became unmanageable.

A Foucault break, with the platinum amalgam and alcohol above it, was now tried, and answered very much better than the ordinary contact-breaker: there was no longer any burning or destruction of the contact points, although the great power of the instrument appeared to cause continued decomposition in the water of the alcohol placed above the platinum amalgam, and the spirit was frequently ejected, probably by explosion of the mixed gases taking place in the amalgam, in which they collected in bubbles; the alcohol took fire constantly, and had to be extinguished. A large and very strong glass vessel (in fact the inverted glass cell of a bichromate battery) was bored through, and the neck fitted into a cap with cement, a thick wire covered with platinum being inserted in the cap; the platinum amalgam was poured on this, and over it a pint of alcohol; the contact wire was also very large, and pointed with a thick stud of platinum, and, being attached to a spring, contact was easily made and broken. Flashes of light could be seen between the amalgam and the alcohol; but explosions did not occur, and the height of the column of the latter prevented the forcible ejection of the spirit, which no longer took fire. This break was used for eight hours in a continuous series of experiments.

The Bunsen's battery used in the experiments was made with the largest porous cells that could be obtained, and each cell contained about one pint of nitric acid, the immersed carbon being 50 superficial inches in each cell.

The resistance of a single cell of this large Bunsen battery was found to be ·2585 B.A.U.

In the following experiments the battery was arranged for intensity, and used with the complete condenser of 750 square feet of tinfoil and 2000 square feet of paper in 1500 sheets.

Number of cells of battery.		Length of spark. inches.
5	complete condenser	12·0
10	„ „	14·0
15	„ „	17·5
20	„ „	21·25
25	„ „	23·0
30	„ „	23·5
35	„ „	26·0
40	„ „	27·5
50	„ „	28·0 to 29·0

The longest spark yet obtained is therefore 29 inches in length.

In order to ascertain whether any variation in the size of the condenser (of which, as already stated, 1, 2, 3, 4, 5, or 6 parts could be used) would affect the length of the spark, a number of experiments were tried; and it will be noticed in the tabulated results that when half the condenser was used the spark increased in length up to 20 cells, but not after. The experiment of dividing the condenser and using one half led to a very serious accident, and the coil was rendered useless for a time by the destruction of the insulating material of a part of the primary coil; the particular strands affected threw out minute spicula of metal, which communicated with each other, and the battery-current, instead of passing through 1257 yards, now only traversed a very short length. The accident, however, proved to be useful, inasmuch as it showed that the coil could be easily taken to pieces and repaired in a comparatively short space of time. In the annexed Table the experiments with the half of the condenser are marked with a cross.

Number of cells.			Length of spark. inches.
5	full condenser		12·00
„	reduced $\frac{1}{6}$		10·75
„	„ $\frac{2}{6}$		13·00
„	*one-half* or $\frac{3}{6}$	+	13·50
„	reduced $\frac{4}{6}$		13·00
„	„ $\frac{5}{6}$		11·75
10	full condenser		14·00
„	reduced $\frac{1}{6}$		15·00
„	„ $\frac{2}{6}$		15·75

			inches.
10	*one-half* $\frac{3}{6}$	+	18·00
,,	reduced $\frac{4}{6}$		17·25
,,	,, $\frac{5}{6}$		17·
20	full condenser		21·25
,,	reduced *one-half*	+	22
30	full condenser		23·5
,,	reduced *one-half*	+	23·5
40	full condenser		25·0
,,	*one-half*	+	25·0
50	full condenser		28
,,	*one-half*	+	28

Experiments were now tried to ascertain whether any increase in the length of the spark could be obtained by arranging the battery and the primary coil for *quantity*.

		inches.
5+5 cells,	length of spark	14·5
10+10 cells,	,,	18·0
20+20 cells,	,,	21·75
25+25 cells,	,,	23·75
15+15+15 cells,	,,	20·00

It is evident that no material advantage was obtained by the above arrangement except in the first experiment; and even where three groups were connected, as in the last experiment, a decrease in the length of the spark is observed when compared with the 45 or 50 cells arranged for intensity, the difference being as 20 to 28.

The spark obtained from the large coil presents some novel and curious features. It is thick and flame-like in its appearance, and therefore it will be alluded to as the "flaming spark."

When the discharging-point and circular plate are brought within 6 or 7 inches of each other, the flaming nature of the spark becomes still more apparent.

Two light yellow flames curving upwards appear to connect the opposite poles. If a blast of air from powerful bellows is directed against the flaming spark, the flaming portion can be blown away and increased in area; and thin wiry sparks are now seen darting through it, sometimes in one continuous stream, at another time divided into three or more sparks, all following the direction in which the flame is blown.

The heat of this is very great, and, if passed through asbestos (supported on an insulating pillar), quickly causes the latter to become red-hot.

When powdered charcoal is shaken from a pepper-box into the flaming

spark in a vertical line and in considerable quantities, the greater part of the light is obscured, and the whole form of the flaming spark presents the appearance of a black cloud with a line of brightly ignited particles fringing the lower parts. If the charcoal is dusted through in small quantities, each particle becomes ignited, like charcoal blown into a hydrogen-flame.

When the flaming spark is directed on to a glass plate upon which a little solution of lithium chloride is placed, the latter colours the flame upwards to the height of 3 or 4 inches in the most beautiful manner; and if the point of the discharge is tipped with paper or sponge moistened with a little solution of sodium chloride, the two colours (the yellow from the salt and the crimson from the lithium) meet each other, a neutral point being found about halfway, thus illustrating apparently the dual character of electricity, and that + passes to – electricity, and *vice versâ.*

The flaming spark can be obtained in perfectly dry air.

Whilst passing through common air, if blown against a sheet of damp litmus-paper, the latter is rapidly changed red. In order to ascertain whether the acid product was nitric acid, the flaming spark (9 or 10 inches in length) was passed through a tube connected by a cork and bent tube with a bottle containing distilled water, from which another tube passed to the air-pump; on drawing the air slowly over the spark, and passing the former into the bottle, nitric acid was obtained in large quantities—so much so that it could be detected by the smell and taste as well as by the ordinary tests. The popular notion that nitric acid is always produced during a thunder-storm would therefore appear to be correct. To determine the effect of a cooling surface on the flaming spark, a hole one inch and a half in diameter was bored through a thick block of Wenham-lake ice, and the spark passed through the air in the tube of ice; no change took place, and the spark was still a flaming one.

When the spark was received on the ice, it lost its flaming character, and became thin and wiry, spreading out in all directions.

If the discharging-wires were tipped with ice, the spark was always flaming when any thickness of air intervened between them. Even over the ice, if the spark passed a fraction of an inch above the surface, it was always a flaming one, but changed to the thin spark when the point of the discharging-wire was thrust into the ice.

If one of the discharging-wires of the great coil is brought to the centre of a large swing looking-glass and the other wire connected with the amalgam at the back, the sparks are thin and wiry, arborescent, and very bright (see figure, p. 69), the crackling noise of these discharges being quite different from that of the heavy thud or blow delivered by the flaming spark.

When the discharging-wire is brought close to the frame of the looking-glass, or if a sufficient thickness of air intervenes, the spark again becomes flaming; or, as sometimes occurs, if the discharging wire is placed about 5 inches from the frame, the spark is partly flaming and partly wiry, *i. e.* when it impinges on the glass.

The examination of the flaming spark with the spectroscope has not as yet settled anything definitely. The spectrum is a continuous one with the sodium-line. When the blast of air is used, and the wiry sparks made apparent, then the nitrogen line appears.

The flaming spark has been ascribed by some experienced observers to the incandescence of the dust in the air, and especially sodium chloride.

If the salt &c. is thus made hot, can the air in which it is mechanically diffused remain cool?

Is not the salt &c. in the same condition as a platinum-wire held in the non-luminous part of the hot burnt gas escaping from the chimney of an Argand burner?

Will gaseous elements when combining (and in this case the nitrogen and oxygen do unite, as proved by the formation of nitric acid) give a continuous spectrum?

To ascertain whether the "flaming spark" could be obtained with a small number of cells, the large Bunsen's battery was reduced to 3 cells; and it was found that no appreciable spark could be produced when the whole primary wire was used with less than 5 cells.

By reducing the length of the primary wire, and using the 4 divisions separately, the following results were arrived at:—

5 cells.

	inches.	
1st section, nearest core	$4\frac{1}{2}$,	wiry spark.
2nd "	$6\frac{1}{2}$,	" "
3rd "	$4\frac{3}{4}$,	" "
4th "	$6\frac{1}{2}$,	" "

10 cells.

	inches.
1st section, nearest core	$8\frac{1}{8}$, wiry spark.
2nd „	$8\frac{3}{4}$, „ „
3rd „	8, bright blue wiry spark.
4th „	$9\frac{3}{4}$, slightly flaming.

15 cells.

	inches.
1st section, nearest core..........	10, slightly flaming.
2nd „	$10\frac{7}{8}$, „ „
3rd „	$9\frac{3}{4}$, „ „
4th „	$11\frac{5}{8}$, flaming spark.

20 cells.

	inches.
1st section, nearest core..........	$11\frac{1}{2}$, flaming spark.
2nd „	12, „ „
3rd „	11, „ „
4th „	$12\frac{1}{2}$, „ „

If the two wires from the secondary coil are placed in water, no spark is perceptible, even when they are brought very close together, until they touch.

If the negative wire is passed through a cork, on which a glass tube (a lamp-glass) is fixed containing a depth of 5 inches of water, and the positive wire is brought within half an inch of the surface of the water in the tube, it becomes red-hot; and if drawn further away from the surface, the upper part of the tube is filled with a peculiar glow or light abounding in Stokes's rays.

The experiments with the vacuum-tubes, and especially Gassiot's cascade, are, as might be expected, very beautiful. When a coal-gas vacuum-tube of considerable diameter, and conveying the full discharge from the secondary coil, is supported over a powerful electromagnet axially, the discharge is condensed and heat is produced.

If placed equatorially, the heat increases greatly; the discharge is condensed and impinges upon the sides of the glass tube, which becomes too hot to touch; and if the experiment had been continued too long, no doubt the tube would have cracked.

The enormous quantity of electricity of high tension which the coil evolves when connected with a battery of 40 cells, is shown by the rapidity with which it will charge a Leyden battery.

Under favourable circumstances, three contacts with the mercurial break will charge 40 square feet of glass.

Mr. Gassiot was present on one occasion, and particularly observed with myself the rapidity with which a series of 12 large Leyden jars arranged in cascade were discharged. The noise was great; and each time the spark (which was very condensed and brilliant) struck the metallic disk, the latter emitted a ringing sound, as if it had received a sharp blow from a small hammer.

The discharges were made from a point to a metallic disk; and when the former was positive the dense spark measured from 18½ to 18¾ inches, and fell to 8½ inches when the metallic plate was positive and the point negative.

A variations of the Leyden-jar experiments was tried, by connecting the coil worked by a quantity battery of 25+25 cells with six Leyden jars arranged in cascade; and the spark obtained measured 8½ inches.

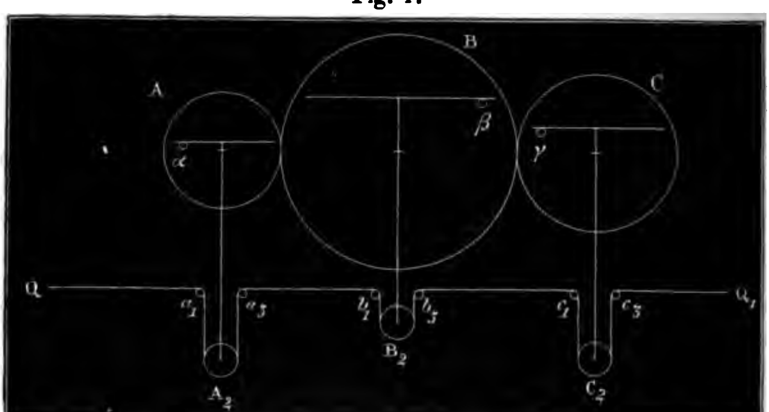
The same six jars connected with the coil when the 50 cells were arranged continuously for intensity gave a spark of 12 inches of very great density and brilliancy.

Other experiments are being tried with the great coil, the results of which will be duly brought before the Society if thought of sufficient importance.

XXVIII. "On the Mechanical Description of Curves."
By W. H. L. Russell, F.R.S. Received June 17, 1869.

Let A, B, C be three wheels rolling in one another (fig. 1); they may of course be supposed to describe simultaneously the angles $m\theta$, $n\theta$, $r\theta$, when m, n, and r are constant.

Fig. 1.

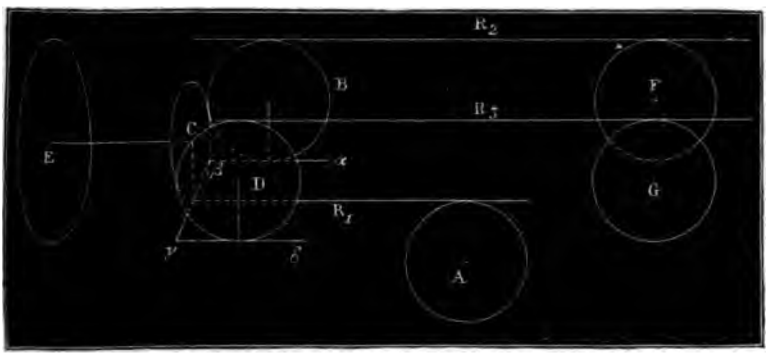


Let α, β, γ be three nuts situated on A, B, C respectively, at distances a, b, c from their centres. Then if these nuts work in horizontal bars (as exemplified in many sewing-machines), the bars will descend vertically

through the spaces $a \sin m\theta$, $b \sin n\theta$, $c \sin r\theta$ respectively. We may combine all these vertical motions together; for if vertical rods be attached to the horizontal bars, and a cord fixed at Q pass over the pulleys a_1, A_2, a_2, b_1, B_2, b_2, c_1, C_2, c_2, as shown in the figure, the other extremity Q_1 will describe the space $a \sin m\theta + b \sin n\theta + c \sin r\theta$. By this contrivance we are able to combine any number of vertical descents, so that it is readily seen that $a \sin (m\theta+\alpha)+b \sin (n\theta+\beta)+$ &c. may be described mechanically. A machine on the same principle as this had been previously invented by Mr. Bashforth.

I soon perceived that in order to describe the general equation of the *rth* order by continued motion, it was necessary to make a wheel revolve through an angle equal to the sum and difference of the angles described in the same time by two given wheels; to effect this I invented the apparatus shown in fig. 2.

Fig. 2.



In fig. 2 let A be a vertical wheel working truly in a horizontal rack R_1, which propels the horizontal frame α, β, γ, δ. On this frame stand the wheels B and D parallel to the plane of the paper. The wheel C, supposed perpendicular to the plane of the paper, works by teeth in the wheels B and D, and the four wheels A, B, C, D are precisely equal.

To the centre of C is attached a square axis, which passes through the centre of the wheel E, so that the wheel E in revolving may, without changing its plane, communicate motion to C as the frame moves forward. Two horizontal racks, R_2, R_3, parallel to the plane of the paper, are urged by the wheels B and D; and these, again, work in the fixed wheels F and G, equal to A, B, C, D in all respects. Then if the wheel A describe in a given time the angle θ, and the wheel E in the same time the angle ϕ, the wheels F and G will revolve respectively in the same time through the angles $\theta+\phi$ and $\theta-\phi$.

We shall call the wheel A an abscissa wheel, the wheel E, an ordinate wheel, for reasons which will appear directly, also F an addition wheel, and G a subtraction wheel.

Let $x=a \sin\theta$, $y=a \sin\phi$, then the general equation of the rth order may be written

$$\alpha \sin(m\theta+n\phi)+\alpha' \sin(m'\theta-n'\phi)+\alpha'' \sin(m''\theta+n''\phi)+\ldots=a \sin\theta.$$

Let a number of machines like the foregoing be placed side by side with their ordinate wheels rolling in one another, and their abscissa wheels duly connected. Let one abscissa wheel describe an angle $m\theta$, and the corresponding ordinate wheel the angle $n\phi$, then a nut placed on the corresponding addition wheel, at a distance α from its centre, will cause a horizontal bar to descend vertically through a space $\alpha \sin(m\theta+n\phi)$. In the same way a nut properly placed on the subtraction wheel will cause a horizontal bar to descend vertically through a space $\alpha' \sin(m\theta-n\phi)$. By means of the adjacent machines we may in like manner cause bars to descend through the vertical spaces, $\alpha'' \sin(m'\theta+n'\phi)$, $\alpha''' \sin(m'\theta-n'\phi)$, &c. Now let motion be communicated to the ordinate wheels, and let all the vertical motions due to the addition and subtraction wheels be combined together and made to act vertically upon a nut in one of the abscissa wheels; then the angles θ, ϕ, will satisfy the equation

$$\alpha \sin(m\theta+n\phi)+\alpha' \sin(m\theta-n\phi)+\alpha'' \sin(m'\theta+n'\phi)\ldots=a \sin\theta,$$

which is the general equation of the rth order.

Therefore two bars moved respectively horizontally and vertically by nuts in the wheels describing the angles θ and ϕ will trace by their intersection the required curve.

Communications received since the end of the Session.

I. "Spectroscopic Observations of the Sun."—No. V. By J. Norman Lockyer, F.R.S. Received July 8, 1869.

Since the date of my last communication under the above title the weather has, if possible, been worse for telescopic work than during the winter and spring; my opportunities of observation, therefore, have been very limited: still the sun has occasionally been in such a disturbed state, and our atmosphere has at times been so pure, that several new facts of importance have come out.

I will state them here as briefly as possible, reserving a discussion of them and my detailed observations for a future occasion.

I. The extreme rates of movement in the chromosphere observed up to the present time are:—

Vertical movement	40 miles a second
Horizontal or cyclonic movement .	120 „

II. I have carefully observed the chromosphere when spots have been near the limb. The spots have sometimes been accompanied by prominences, at other times they have not been so accompanied. Such observations show that we may have spots visible without prominences in the same region,

and prominences without spots; but I do not say that a spot is not accompanied by a prominence *at some stage of its life*, or that it does not result from some action which, in the majority of cases, is accompanied by a prominence.

III. At times, when a prominence is seen bright on the sun itself, the bright F line varies considerably, both in thickness and brilliancy, within the thickness of the dark line. The appearances presented are exactly as if we were looking at the prominences through a grating.

IV. Bright prominences, when seen above spots on the disk, if built up of other substances besides hydrogen, are indicated by the bright lines of those substances in addition to the lines of hydrogen. The bright lines are then seen very thin, situated centrally (or nearly so) on the broad absorption-bands caused by the underlying less-luminous vapours of the same substances.

V. I have at last detected an absorption-line corresponding to the orange line in the chromosphere. Father Secchi states* that there is a line corresponding to it much brighter than the rest of the spectrum. My observation would seem to indicate that he has observed a bright line less refrangible than the one in question, which bright line is at times excessively brilliant. It requires absolutely perfect atmospheric conditions to see it in the ordinary solar spectrum. It is best seen in a spot-spectrum when the spot is partially covered by a bright prominence.

VI. In the neighbourhood of spots the F bright line is sometimes observed considerably widened out in several places, as if the spectroscope were analyzing injections of hydrogen at great pressure in very limited regions into the chromosphere.

VII. The brilliancy of the bright lines visible in the ordinary solar spectrum is extremely variable. One of them, at 1871·5, and another, at 1529·5 of Kirchhoff's scale, I have detected in the chromosphere at the same time that they were brilliant in the ordinary solar spectrum.

VIII. Alterations of wave-length have been detected in the sodium-, magnesium-, and iron-lines in a spot-spectrum. In the case of the last substance, the lines in which the alteration was detected were *not* those observed when iron (if we accept them to be due to iron alone) is injected into the chromosphere.

IX. When the chromosphere is observed with a tangential slit, the F bright line close to the sun's limb shows traces of absorption, which gradually diminish as the higher strata of the chromosphere are brought on to the slit, until the absorption-line finally thins out and entirely disappears. The lines of other substances thus observed do not show this absorption.

X. During the most recent observations I have been able to detect traces of magnesium and iron in nearly all solar latitudes in the chromosphere. If this be not merely the result of the good definition lately, it would indicate an increased general photospheric disturbance as the maximum sun-

* Comptes Rendus, 1869, 1^{r} sem. p. 358.

spot period is approached. Moreover I suspect that the chromosphere has lost somewhat of its height.

I append a list of the bright lines, the positions of which in the chromosphere I have determined absolutely, with the dates of discovery, remarking that in the case of C and F my observations were anticipated by M. Janssen:—

Hydrogen.

C.	October 20, 1868.
F.	October 20, 1868.
near D.	October 20, 1868*.
near G.	December 22, 1868.
h.	March 14, 1869.

Sodium.

D. February 28, 1869.

Barium.

1989·5†.	March 14, 1869.
2031·2.	July 5, 1869.

Magnesium and included line.

b^1, b^2, b^3, b^4 } February 21, 1869.

Other Lines.

Iron......	1474.	June 6, 1869.
?	1515·5.	June 6, 1869.
Bright line	1529·5.	July 5, 1869.
?	1567·5.	March 6, 1869
?	1613·8.	June 6.
Iron......	1867·0.	June 26.
Bright line	1871·5.	,,
Iron......	2001·5.	,,
?	2003·4.	,,
? band or line near black line, very delicate....	2054·0	July 5.

I have seen other lines besides these at different times; but I do not include them, as their positions have not been determined absolutely.

I refrain from dwelling on this list at present, except to point out that, taking iron as an instance, and assuming that the iron-lines mapped by Ångström and Kirchhoff are due to iron only, I have only been able, up to the present time, to detect 3 lines out of the total number (460) in the spectrum of the lower regions of the chromosphere,—a fact full of promise as

* [*Hydrogen* ?—G. G. S.]

† This reference is to Kirchhoff's scale.

ward by Dr. Frankland and myself—namely, that the chromosphere
e photosphere form the true atmosphere of the sun, and that under
y circumstances the absorption is continuous from the top of the
osphere to the bottom of the photosphere, at whatever depth from
ttom of the spot that bottom may be assumed to be.

theory was based upon all our observations made from 1866 up to
ne at which it was communicated to the Royal Society and the Paris
ny of Sciences, and has been strengthened by all our subsequent
but several announcements made by Father Secchi to the Paris
ny of Sciences and other learned bodies are so opposed to it, and
o much from my own observations, that it is necessary that I should
them, and give my reasons for still thinking that the theory above
d to is not in disaccord with facts. At the same time I must state
ather Secchi does not combat this theory; indeed it is not to be
ed from any of his communications that he has seen any of the papers
unicated by myself to the Royal Society.

ner Secchi states that the chromosphere is often separated from the
phere, and that between the chromosphere and the photosphere
exists a stratum giving a continuous spectrum, which he considers to
base of the solar atmosphere, and in which he thinks that the in-
of the spectrum takes place.

h regard to the first assertion, I may first state that all the observa-
have made have led me to a contrary conclusion. Secondly, in an
nent of comparatively small dispersive power, such as that employed
her Secchi, in which the widening out of the F line at the base of
romosphere is not clearly indicated, it is almost impossible to deter-
by means of the spectroscope, whether the chromosphere rests on the

With regard to the second assertion, I would remark that if such a continuous-spectrum-giving envelope existed, I entirely fail to see how it could be regarded as a region of selective absorption. Secondly, my observations have indicated no such stratum, although injections of sodium, magnesium, &c. into the chromosphere not exceeding the limit of the sun's limb by 2″ have been regularly observed for several months past. To-day I have even detected a low level of barium in the chromosphere not 1″ high. This indicates, I think, that my instrument is not lacking in delicacy; and as I have never seen anything approaching to a continuous spectrum when my instrument has been in perfect adjustment, I am inclined to attribute the observation to some instrumental error. Such a phenomenon might arise from a local injection of solid or liquid particles into the chromosphere, if such injection were possible. But I have never seen such an injection. If such an occurrence could be observed, it would at once settle that part of Dr. Frankland's and my own theory, which regards the chromosphere as the last layer of the solar atmosphere; and if it were possible to accept Father Secchi's observation, the point would be settled in our favour.

The sodium experiments to which I have referred, however, and the widening out of the lines in the spot-spectra, clearly indicate, I think, that the base of the atmosphere is below the spot, and not above it. I therefore cannot accept Father Secchi's statement as being final against another part of the theory to which I have referred—a conclusion which Father Secchi himself seems to accept in other communications.

Father Secchi remarks also that the F line is produced by the absorption of other bodies besides hydrogen, because it never disappears. This conclusion is also negatived by my observations; for it has very often been observed to disappear altogether and to be replaced by a bright line. At times, as I pointed out to the Royal Society some months ago, when a violent storm is going on accompanied by rapid elevations and depressions of the prominences, there is a black line on the less-refrangible side of the bright one; but this is a phenomenon due to a change of wave-length caused by the rapid motion of the hydrogen.

With regard to the observation of spot-spectra, I find that every increase of dispersive power renders the phenomenon much more clear, and at the same time more simple. The selective absorption I discovered in 1866 comes out in its most intense form, but without any of the more complicated accompaniments described by Father Secchi. I find, however, that by using three prisms this complexity vanishes to a great extent. We get portions of the spectrum here and there abnormally bright, which have given rise doubtless to some of the statements of the distinguished Roman observer; but the bright lines, properly so-called, are as variable as they are in any other part of the disk, but not much more so. I quite agree that the "interpretation" of sun-spot phenomena to which Father Secchi has referred*,

* Comptes Rendus, 1869, 1r sem. p. 764.

which ascribes the appearances to anything but selective plus general absorption, is erroneous. But as I was not aware that it had ever been propounded, I can only refer to my own prior papers in support of my assertion, and to Mr. Huggins's indorsement of my observations, which were communicated to the Royal Society some three years ago.

II. "Researches on Gaseous Spectra in relation to the Physical Constitution of the Sun, Stars, and Nebulæ."—Third Note. By E. FRANKLAND, F.R.S., and J. NORMAN LOCKYER, F.R.S. Received July 14, 1869.

1. It has been pointed out by one of us that the vapours of magnesium, iron, &c. are sometimes injected into the sun's chromosphere and are then rendered sensible by their bright spectral lines *.

2. It has also been shown (1) that these vapours, for the most part, attain only a very low elevation in the chromosphere, and (2) that on rare occasions the magnesium vapour is observed like a cloud separated from the photosphere.

3. It was further established on the 14th of March, 1869, and a drawing was sent to the Royal Society indicating, that when the magnesium vapour is thus injected the spectral lines do not all attain the same height.

Thus of the b lines, b^1 and b^2 are of nearly equal height, but b^4 is much shorter.

4. It has since been discovered that of the 450 iron lines observed by Ångström, only a very few are indicated in the spectrum of the chromosphere when iron vapour is injected into it.

5. Our experiments on hydrogen and nitrogen enabled us at once to connect these phenomena, always assuming, as required by our hypothesis†, that the great bulk of the absorption to which the Fraunhofer lines are due takes place in the photosphere itself.

It was only necessary, in fact, to assume that, as in the case of hydrogen and nitrogen, the spectrum became simpler where the density and temperature were less, to account at once for the reduction in the number of lines visible in those regions where, on our theory, the pressure and temperature of the absorbing vapours of the sun are at their minimum.

6. It became important, therefore, to test the truth of this assumption by some laboratory experiments, the preliminary results of which we beg to communicate in this Note, reserving details, and an account of the further experiments we have already commenced, for another paper under the above title.

We took the spark in air between two magnesium poles, so separated that the magnesium spectrum did not extend from pole to pole, but was visible only for a little distance, indicated by the atmosphere of magnesium vapour, round each pole.

* Proc. Roy. Soc. vol. xvii. p. 351. † *Ibid.* p. 290.

We then carefully examined the disappearance of the *b* lines, ***and found that they behaved exactly as they do on the sun.*** Of the three lines the most refrangible was the shortest; and shorter than this were other lines, ***which one of us has not yet detected in the spectrum of the chromosphere.***

This preliminary experiment, therefore, quite justified our assumption, and must be regarded as strengthening the theory on which the assumption was based—namely, that the bulk of the absorption takes place in the photosphere, and that it and the chromosphere form the true atmosphere of the sun. In fact had the experiment been made in hydrogen instead of in air, the phenomena indicated by the telescope would have been almost perfectly reproduced; for each increase in the temperature of the spark caused the magnesium vapour to extend further from the pole, and where the lines disappeared a band was observed surmounting them, which is possibly connected with one which at times is observed in the spectrum of the chromosphere itself when the magnesium lines are not visible.

III. "On the Thermodynamic Theory of Waves of Finite Longitudinal Disturbance." By W. J. MACQUORN RANKINE, C.E., LL.D., F.R.SS. Lond. & Edin. Received August 13, 1869.

(Abstract.)

The object of the present investigation is to determine the relations which must exist between the laws of the elasticity and heat of any substance, gaseous, liquid, or solid, and those of the wave-like propagation of a finite longitudinal disturbance in that substance—in other words, of a disturbance consisting in displacements of particles along the direction of propagation, the velocity of displacement of the particles being so great that it is not to be neglected in comparison with the velocity of propagation. In particular, the investigation aims at ascertaining:—in the first place, what conditions as to the transfer of heat from particle to particle must be fulfilled in order that a finite longitudinal disturbance may be propagated along a prismatic or cylindrical mass without loss of energy or change of type—the word type being used to denote the relation between the extent of disturbance at a given instant of a set of particles and their respective undisturbed positions; and, secondly, according to what law the type of a wave of finite longitudinal disturbance must change when the substance through which it is propagated has, under the circumstances of the disturbance, no appreciable power of transferring heat from particle to particle, being in the condition which, in the language of thermodynamics, is called *adiabatic*. The disturbed matter in these inquiries may be conceived to be contained a straight tube of uniform cross section and indefinite length.

The investigation is facilitated by the use of a quantity which the author calls the *Mass-velocity* or *Somatic Velocity*—that is to say, the mass of matter through which a disturbance is propagated in a unit of time while advancing along a prism of the sectional area unity,—also by expressing the re-

lative positions of a series of transverse planes that travel along with a wave by means of the masses of matter contained between them, instead of by their distances apart.

Let such a transverse advancing plane coincide with that part of a wave of longitudinal disturbance at which the pressure P and bulkiness S * are equal to those corresponding to the undisturbed condition; it is shown that the value of the square of the mass-velocity is

$$m^2 = -\frac{dP}{dS}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (A)$$

The linear velocity of advance of the wave is obviously mS.

Let a second transverse plane advance along with the wave in such a manner that an invariable mass of matter is contained between it and the first advancing plane. The condition of *permanence of type of disturbance* is, that the distance between those planes shall be invariable. Let $\frac{dx}{dt}$ be the rate at which that distance varies, being positive when the second plane gains on the first plane; it is shown that this quantity has the following value—

$$\frac{dx}{dt} = \frac{p-P}{m} - m(S-s); \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (B)$$

in which p and s respectively are the pressure and bulkiness at the second plane. Hence the condition of permanence of type is expressed symbolically as follows:

$$\frac{p-P}{S-s} = -\frac{dp}{ds} = -\frac{dP}{dS} = m^2 \text{ (a constant)}. \quad . \quad . \quad . \quad (C)$$

This relation between pressure and bulkiness is not fulfilled by any known substance, when either in an absolutely non-conducting' state (called, in the language of thermodynamics, the *adiabatic* state) or in a state of uniform temperature. In order that it may be fulfilled, transfer of heat must go on between the particles affected by the wave-motion, in a certain manner depending on the *thermodynamic function.* The value of the thermodynamic function is

$$\phi = Jc \text{ hyp} \log \tau + \chi(\tau) + \frac{dU}{d\tau}; \quad . \quad . \quad . \quad . \quad . \quad (D)$$

in which J is the dynamical equivalent of a unit of heat, c the real specific heat of the substance, τ the absolute temperature, $\chi(\tau)$ a function of the absolute temperature, which is $=0$ for all temperatures at which the substance is capable of approximating indefinitely to the perfectly gaseous state, and U the work which the elastic forces in unity of mass of the substance are capable of doing at the constant temperature τ. The thermodynamic condition to be fulfilled by a wave of permanent type is expressed by

$$\int \tau d\phi = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (E)$$

* The word *bulkiness* is used to denote the reciprocal of the density.

In applying this equation to particular cases, ϕ and τ are to be expressed in terms of p and s.

It is shown to be probable that the only longitudinal disturbance which can be propagated with absolute permanence of type is a sudden disturbance; and that the consequence of the non-fulfilment of the condition of permanence of type is a tendency for every wave of gradual longitudinal disturbance to convert itself by degrees into a wave of sudden disturbance. But although suddenness of disturbance may be approximated to, it cannot be absolutely and permanently realized; whence it follows that the propagation of waves of longitudinal disturbance of absolutely permanent type for an indefinite distance is impossible; and this may be the cause of the absence of longitudinal vibrations from rays of light.

The laws of the advance of *adiabatic waves* are investigated; that is, waves of longitudinal disturbance in which there is no transfer of heat, and in which consequently $d\phi=0$; and it is shown, by the aid of the equation marked (B) in this abstract, that the compressed parts of those waves tend to gain upon and at last overtake the rarefied parts, just as the crests of rolling waves in shallow water gain upon and at last break into the troughs, the consequence being a gradual conversion of the adiabatic waves into waves of sudden disturbance, followed by a mutual interference of the compressed with the rarefied parts which leads to the energy of the waves being spent in molecular agitation.

It is also shown that the extreme values of the pressure and of the bulkiness are constant during the change of type; and consequently that the respective velocities with which the plane of greatest compression gains upon and the plane of greatest rarefaction falls behind the plane of undisturbed density are uniform.

The values of the linear velocity of advance, mS, found for various modes of finite disturbance, all approximate, when the disturbance becomes indefinitely small, to the well-known value of the velocity of sound, viz. $\sqrt{\left\{\frac{d\mathrm{P}}{d.\frac{1}{\mathrm{S}}}\right\}}$, the relation between P and S being determined by the condition $d\phi=0$.

Supplement. Received October 1, 1869.

(Abstract.)

In this supplement the author of the paper refers to the previous investigations on waves of finite longitudinal disturbance by the following authors:—

Poisson, 'Journal de l'Ecole Polytechnique,' vol. vii. cahier 14, p. 319
Stokes, Philosophical Magazine, Nov. 1848, S. 3. vol. xxxiii. p. 349.
Airy, Philosophical Magazine, June 1849, S. 3. vol. xxxiv. p. 401.
Earnshaw, Philosophical Transactions, 1860, p. 133.

He points out to what extent the results arrived at in his own paper are

identical with those of the above-mentioned previous researches; and he claims the following results as new:—The conditions as to transfer and transformation of heat which must be fulfilled in order that permanence of type may be realized, exactly or approximately, in a wave of finite longitudinal disturbance in any elastic medium; the types of wave which enable such conditions to be fulfilled with a given law of the conduction of heat; the velocity of advance of such waves; and some special results as to the rate of change of type in adiabatic waves. He also claims as new the method of investigation by the aid of *mass-velocity* and *mass-coordinates*, which he alleges to possess great advantages in point of simplicity.

IV. "Researches into the Constitution of the Opium Bases.—Part III. On the Action of Hydrochloric Acid on Codeia." By AUGUSTUS MATTHIESSEN, F.R.S., Lecturer on Chemistry in St. Bartholomew's Hospital, and C. R. A. WRIGHT, B.Sc. Received July 23, 1869.

§ 1. *On the Action of Hydrochloric Acid on Codeia.*

In Part II. (Proc. Roy. Soc. vol. xvii. p. 460) it was shown that when codeia is heated with excess of hydrochloric acid under pressure that it splits up into chloride of methyl, water, and apomorphia, thus—

$$C_{18}H_{21}NO_3+HCl=CH_3Cl+H_2O+C_{17}H_{17}NO_2.$$

At the time it appeared probable that one of the two following reactions would first take place, forming an intermediate product:—

$$\text{I.}\quad C_{18}H_{21}NO_3+HCl=CH_3Cl+C_{17}H_{19}NO_3.$$

$$\text{II.}\quad C_{18}H_{21}NO_3 \quad = H_2O+C_{18}H_{19}NO_2.$$

On investigation, however, it has been found that neither the one nor the other takes place, at least as the chief reaction; for by heating codeia with excess of hydrochloric acid on the water-bath, a body is obtained by the following reaction—

$$C_{18}H_{21}NO_3+HCl=H_2O+C_{18}H_{20}ClNO_2;$$

and this base, when heated under pressure with hydrochloric acid, splits up into chloride of methyl and apomorphia,

$$C_{18}H_{20}ClNO_2=CH_3Cl+C_{17}H_{17}NO_2.$$

The new base may be obtained in a state of purity thus:—codeia is heated under paraffin on the water-bath with ten to fifteen times its weight of strong hydrochloric acid for twelve to fifteen hours, and the resulting brownish liquid evaporated to dryness on the water-bath; the residue is dissolved in water, and excess of bicarbonate of sodium added, whereby a voluminous white precipitate is formed, consisting chiefly of the new base mixed with a trace of apomorphia. The filtrate contains the unaltered

codeia mixed with a little of the new base, which is not quite insoluble in bicarbonate of sodium solution; the precipitate is washed with ammonia-water to remove the trace of apomorphia (which is much more soluble in ammonia than the new base), and is then dissolved in hydrochloric acid and fractionally precipitated by bicarbonate of sodium: the second fraction is pure white, and is free from apomorphia and codeia; this is then extracted with ether, which dissolves almost the whole. The clear ethereal solution is then shaken up with a few drops of hydrochloric acid, and the solution of hydrochlorate thus obtained, if coloured, must be fractionally precipitated by bicarbonate of sodium, and the ether and hydrochloric-acid process repeated; the resulting product is a viscid colourless solution of the hydrochlorate of the new base, which refuses to crystallize. When evaporated on the water-bath, the dry residue yielded the following numbers on analysis:—

(I.) 0·3270 gramme, burnt with lead chromate, gave 0·7230 carbonic acid and 0·1820 water.

(II.) 0·3080 gramme gave 0·6870 carbonic acid and 0·1720 water.

(III.) 0·3010 gramme, burnt with lime, gave 0·2450 silver chloride.

	Calculated.		Found.		
			I.	II.	III.
C_{18}	216	61·01	60·30	60·83	
H_{21}	21	5·93	6·18	6·20	
Cl_2	71	20·04			20·14
N	14	3·95			
O_2	32	9·07			
$C_{18}H_{20}ClNO_2, HCl$	354	100·00			

The hydrochlorate, when fractionally precipitated by bicarbonate of sodium, yielded the base as a snow-white mass, scarcely affected by exposure to air, very soluble in alcohol and ether, but not crystallizable from those menstrua owing to decomposition; when well washed and dried, first over sulphuric acid and then at about 60°, it gave the following results on analysis:—

(I.) 0·3380 gramme gave 0·8410 carbonic acid and 0·1900 water.

(II.) 0·2680 gramme, burnt with lime, gave 0·1160 silver chloride.

	Calculated.		Found.	
			I.	II.
C_{18}	216	68·03	67·87	
H_{20}	20	6·29	6·25	
Cl	35·5	11·18		10·71
N	14	4·41		
O_2	32	10·09		
$C_{18}H_{20}ClNO_2$	317·5	100·00		

Another portion of hydrochlorate converted into platinum-salt gave a

yellow precipitate permanent in the air, but decomposed when heated to 100° in a moist state; dried several days over sulphuric acid, 0·5380 gramme gave 0·1000 metallic platinum.

	per cent.
Found	18·60
The formula $(C_{18}H_{20}ClNO_2, HCl)_2 PtCl_4$ requires	18·81
The codeia salt requiring	19·50

From these numbers it appears that the new base is formed from codeia by the replacement of an atom of hydroxyl by one of chlorine, thus—

$$\underset{\text{Codeia.}}{C_{18}H_{21}NO_3} + HCl = H(HO) + \underset{\text{New base.}}{C_{18}H_{20}ClNO_2}.$$

If codeia be regarded as being formed on the mixed type $\begin{matrix}H_3N\\H_2O\end{matrix}$, then the new base may be looked upon as formed on the mixed type $\begin{matrix}H_3N\\HCl\end{matrix}$; and, using Professor Foster's nomenclature* for these types (viz. oxynitride and chloronitride respectively), codeia would be oxycodide, and the new base chlorocodide; but until further investigation affords some knowledge of the nature of the radicals occurring in codeia and morphia, it would be premature to attempt to give rational formulæ for these bases.

The following Table (p. 86) exhibits the comparative reactions of solutions containing 1 per cent. of the hydrochlorates of morphia, codeia, apomorphia, and chlorocodide respectively.

The physiological action of chlorocodide appears to be much less marked than that of apomorphia. Doses of ¼ grain of the hydrochlorate taken internally and $\frac{1}{10}$ grain injected subcutaneously produced no appreciable effect; Dr. Gee is now engaged in studying this subject.

§ 2. *Action of Hydrochloric Acid on Chlorocodide.*

When the hydrochlorate of this base is sealed up with eight to fifteen times its weight of strong hydrochloric acid and heated to 140°–150° for three hours, the tube is found, after cooling, to contain a layer of liquid chloride of methyl floating at the top: the tarry contents of the tube, when dissolved in water and precipitated by bicarbonate of sodium, yield, on shaking up the ethereal extract with a few drops of hydrochloric acid, a copious supply of crystals of hydrochlorate of apomorphia; these, when drained from the mother liquors, washed with cold water, and recrystallized, had all the physical properties of the hydrochlorate of apomorphia from morphia, gave the same qualitative reactions, and produced the same physiological effects, and gave the following numbers on analysis after drying at 100°:—

(I.) 0·3090 gramme, burnt with lead chromate, gave 0·7595 carbonic acid and 0·1740 water.

(II.) 0·4030 gramme, burnt with lime, gave 0·1910 silver chloride.

* Watts's Dictionary, vol. iv. p. 124.

	Ferric Chloride.	Nitric Acid.	Sulphuric Acid and Bichromate of Potassium.	Bleaching-powder Solution and a drop of Hydrochloric Acid.	Corrosive Sublimate Solution.	Nitrate of Silver.
Morphia	Greenish-blue coloration. Morphia alone yields a pure blue.	Yellow coloration.	—	—	— Stronger solutions give a white precipitate soluble on boiling, crystallizing out on cooling.	Slowly reduced.
Codeia	—	Yellowish coloration.	—	—	Same as morphia.	Scarcely any reduction
Apomorphia..	Dark purple amethyst coloration.	Dark blood-red colour.	Dark blood-red colour.	Dark blood-red colour.	White precipitate decomposed on boiling with blood-red coloration.	Reduced more quickly than the morphia.
Chlorocodide..	Pale amethyst coloration.	Pale red colour.	Evanescent pale red coloration.	Pale red colour.	White precipitate decomposed on boiling with pale red colour.	Same as apomorphia.

	Caustic Potash.	Carbonate of Sodium.	Ammonia.	Iodide of Potassium.	Oxalate of Ammonium.	Phosphate of Sodium.
Morphia	— Stronger solutions give a white precipitate scarcely soluble in excess.	— Stronger solutions give a white precipitate insoluble in excess.	— Stronger solutions give a crystalline precipitate insoluble in excess.	—	—	—
Codeia	— Stronger solutions give a white precipitate scarcely soluble in excess.	— Very strong solutions cause codeia to crystallize out slowly after some hours.	—	—	—	—
Apomorphia..	White precipitate readily soluble in excess; solution soon decomposes.	White precipitate slighly soluble in excess.	White precipitate soluble in excess; solution soon decomposing.	White precipitate.	White precipitate.	White precipitate.

	Calculated.		Found. I.	II.
C_{17}	204	67·22	67·04	
H_{18}	18	5·93	6·25	
N	14	4·61		
O_2	32	10·54		
Cl	35·5	11·70		11·72
$C_{17}H_{17}NO_2, HCl$	303·5	100·00		

Hence the reaction which takes place is

$$C_{18}H_{20}ClNO_2 = CH_3Cl + C_{17}H_{17}NO_2.$$

It is worthy of notice that this reaction probably takes place gradually, while that whereby apomorphia and chloride of methyl are formed direct from codeia appears to occur suddenly, thereby bursting the large majority of sealed tubes used: this never occurred with chlorocodide.

§ 3. *Action of Water on the Hydrochlorate of Chlorocodide.*

When the hydrochlorate (obtained by dissolving the base freed from codeia &c. by the process previously described in as small an excess of hydrochloric acid as possible) is sealed up with ten to fifteen times its weight of water and heated for three hours to 130°–140°, it splits up into hydrochloric acid and hydrochlorate of codeia, no gas whatever being formed during the reaction.

$$C_{18}H_{20}ClNO_2, HCl + H_2O = HCl + C_{18}H_{21}NO_3, HCl.$$

In two experiments the amount of free hydrochloric acid thus formed was estimated by titration with a solution of carbonate of sodium, and after subtraction of the small amount due to the excess of hydrochloric acid in the original liquid (estimated in the same way), was found to amount respectively to 9 and 10 per cent. of the hydrochlorate employed, the theoretical amount according to the above equation being 10·3; the amount of undecomposed chlorocodide was found to be very small, the liquid resulting from the digestion giving but a minute precipitate with carbonate of sodium. The filtrate from the carbonate-of-sodium precipitate was extracted with ether, and the ethereal solution obtained shaken with a few drops of hydrochloric acid; an oily liquid was thus obtained, which on standing several hours deposited crystals; these, when drained from the mother liquors and recrystallized, had the character of hydrochlorate of codeia, and gave the following numbers after drying in an ordinary water-bath till constant in weight.

As crystallized hydrochlorate of codeia is stated to lose one-fourth of its water of crystallization at 100°, a sample was prepared by dissolving codeia in hydrochloric acid and recrystallizing the product; after drying in the same water-bath it was also burnt as a comparison (III.).

(I.) 0·3110 gramme of hydrochlorate of codeia from chlorocodide gave 0·6595 carbonic acid and 0·2015 water.

(II.) 0·2790 gramme of the same, burnt with lime, gave 0·1090 silver chloride.

(III.) 0·3460 gramme of hydrochlorate of codeia made from codeia gave 0·7360 carbonic acid and 0·2300 water.

	Calculated.		Found. I.	Found. II.	Found. III.
C_{18}	216	58·13	57·83		58·01
H_{26}	26	7·00	7·20		7·38
O_5	80	21·54			
N	14	3·77			
Cl	35·5	9·56		9·67	
$C_{18}H_{21}NO_3, HCl+2H_2O$	371·5	100·00			

A portion of the regenerated hydrochlorate of codeia was precipitated by caustic potash and crystallized from benzole; after drying at 120° it yielded these numbers.

0·3325 gramme, burnt with oxide of copper and oxygen, gave 0·8795 carbonic acid and 0·2185 water.

	Calculated.		Found.
C_{18}	216	72·24	72·14
H_{21}	21	7·02	7·30
N	14	4·68	
O_3	48	16·06	
$C_{18}H_{21}NO_3$	299	100·00	

The influence of mass upon chemical reactions is well illustrated by the inverse reactions taking place between codeia and hydrochloric acid in excess, and chlorocodide with water in excess.

$$\underset{\text{Codeia.}}{C_{18}H_{20}(HO)NO_2}+HCl=H\,HO+\underset{\text{Chlorocodide.}}{C_{18}H_{20}Cl\,NO_2}$$

$$\underset{\text{Chlorocodide.}}{C_{18}H_{20}Cl\,NO_2}+H(HO)=HCl+\underset{\text{Codeia.}}{C_{18}H_{20}(HO)NO_2}.$$

The codeia employed in the foregoing experiments forms part of a second supply kindly given to us by Messrs. M'Farlane of Edinburgh.

§ 4. *On the Crystalline Form of Chloride of Apomorphia.* By Prof. W. H. Miller, For. Sec. R.S.

Prof. Miller has kindly determined the crystalline form of the chloride of apomorphia, which we here annex.

Chloride of apomorphia.

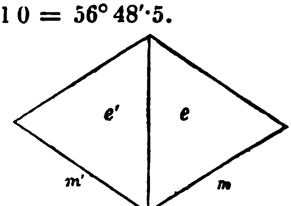
Prismatic:—

0 0 1, 1 0 1 = 29° 26′·5; 1 0 0, 1 1 0 = 56° 48′·5.

Simple forms:—*e*, 1 0 1 : *m*, 1 1 0.

Angles between normals to the faces:—

e e′ .. 58° 53′
m m′ .. 66 23
e m .. 74 23



No cleavage observable.

The crystals are small, the length and breadth of one of the largest being 0·9 and 0·22 millimetres respectively.

V. "On the Action of Cyanogen on Anthranilic Acid." By P. Griess, F.R.S. Received June 29, 1869.

Some time ago* I pointed out the action which takes place when cyanogen gas is passed into an alcoholic solution of amidobenzoic acid. The principal product of this reaction is, as I have shown, a yellow compound of cyanogen and amidobenzoic acid of the formula $C_7\,H_6(NH_2)O_2, 2CN$, which separates in large quantities as soon as the alcoholic solution of amidobenzoic acid is nearly saturated with cyanogen. When anthranilic acid, a body isomeric with amidobenzoic acid, is submitted under the same condition to the same reagent, a totally different reaction takes place. In this case the solution remains either perfectly clear, or only traces of a similar yellow compound are precipitated. By allowing the alcoholic solution of anthranilic acid, saturated with cyanogen, to stand for several days, the acid is almost entirely converted into a new compound of the empirical formula $C_{10}\,H_{10}\,N_2O_2$; two other new compounds (an acid and an indifferent body) are at the same time formed. It is worthy of remark that none of these compounds are isomeric with any of the bodies which by the same process are formed from amidobenzoic acid. Each of them belongs to a perfectly different type.

I propose on this occasion to treat only of the principal product of the reaction, viz. the compound $C_{10}\,H_{10}\,N_2\,O_2$. It is prepared in the following manner. An alcoholic solution of anthranilic acid is saturated with cyanogen gas and left to stand for about eight days. The alcohol is then evaporated at a low temperature, and the crystalline residue washed several times with dilute solution of carbonate of ammonia, by which any traces of the new acid (one of the by-products of the reaction) are removed. It is then further purified by recrystallization from alcohol with the addition of a little animal charcoal. The indifferent body already referred to, which is very little soluble in alcohol, is thus separated. The new compound, $C_{10}\,H_{10}\,N_2\,O_2$, is then obtained in the form of white acicular crystals, which are very little soluble in boiling water, but dissolve readily in boiling alcohol and ether. It fuses at 173° C., and can be distilled in small quantities without undergoing decomposition. Its formation may be expressed as follows:—

$$\underbrace{C_7\,H_7\,NO_2}_{\text{Anthranilic acid.}}+\underbrace{2CN}_{\text{Cyanogen.}}+\underbrace{C_2\,H_6\,O}_{\text{Alcohol.}}=\underbrace{C_{10}\,H_{10}\,N_2\,O_2}_{\text{New compound.}}+\underbrace{CHN}_{\text{Hydrocyanic acid.}}+\underbrace{H_2\,O.}_{\text{Water.}}$$

According to this equation, alcohol as well as anthranilic acid and cyanogen take place in the reaction. Confirmatory experiments which I have made show that the compound in question is really an ether.

Action of Hydrochloric Acid upon the Compound $C_{10}\,H_{10}\,N_2\,O_2$.—Ordinary hydrochloric acid dissolves this body, and when cold does not act upon it. On boiling, however, speedy decomposition sets in and a new

* Zeitschrift für Chemie. New series, vol. iii. p. 533, and vol. iv. p. 389.

body separates, the formation of which is represented by the following equation :—

$$C_{10}H_{10}N_2O_2+H_2O=\underbrace{C_8H_6N_2O_2}_{\text{New compound.}}+\underbrace{C_2H_6O}_{\text{Alcohol.}}$$

This new compound, $C_8H_6N_2O_2$, is very difficultly soluble in boiling water, alcohol, and ether, and crystallizes in small white brilliant plates. It is likewise disolved by solutions of caustic alkalies, but is again, however, separated by carbonic acid. On adding a solution of silver salt to its aqueous or alcoholic solution (neither of which has any action on vegetable colours), a white precipitate is formed. Fuming nitric acid converts this body into a nitro-compound, crystallizing in honey-yellow prisms of the composition $C_8H_5(NO_2)N_2O_2$. On treating the latter with sulphide of ammonium or with tin and hydrochloric acid, it is reduced and furnishes a basic amido-compound crystallizing in slightly yellowish-tinted needles, difficultly soluble in all neutral liquids. Its composition is $C_8H_5(NH_2)N_2O_2$. Compounds of this amido-body with acids crystallize well generally, but are for the most part difficultly soluble.

Action of Ammonia on the Compound $C_{10}H_{10}N_2O_2$.—On digesting the body for several days at 100° C. in sealed tubes with alcoholic ammonia, it is gradually converted into a base, almost insoluble in water and difficultly soluble in boiling alcohol; from this it crystallizes in brilliant nacreous plates.

Its composition agrees with the formula $C_8H_7N_3O$, and its formation takes place according to the equation

$$C_{10}H_{10}N_2O_2+NH_3=\underbrace{C_8H_7N_3O}_{\text{New base.}}+\underbrace{C_2H_6O}_{\text{Alcohol.}}$$

This new base is monacid. Its nitrate is especially characteristic, for it is almost insoluble in water and alcohol. It separates out from very dilute solutions of the base in the form of small white plates on the addition of nitric acid. Its platinum-salt crystallizes in thick yellow needles, and has the composition $2(C_8H_7N_3O), 2HCl, PtCl_4$.

The compounds just described may one and all be viewed as substitution products of anthranilic acid, viz. :—

Anthranilic acid	$C_7H_7NO_2$	$=C_7H_6NO.HO.$
New ether	$C_{10}H_{10}N_2O_2$	$=C_7H_5(CN)NO.C_2H_5O.$
Product of decomposition of the former with HCl..................	$C_8H_6N_2O_2$	$=C_7H_5(CN)NO.HO$
Nitro-compound	$C_8H_5(NO_2)N_2O_2$	$=C_7H_4(NO_2)(CN)NO.HO.$
Amido-compound	$C_8H_5(NH_2)N_2O_2$	$=C_7H_4(NH_2)(CN)NO.HO.$
Base obtained from the ether by the action of NH_3	$C_8H_7N_3O$	$=C_7H_5(CN)NO.H_2N.$

As I intend taking an early opportunity of considering the rational constitution of these bodies somewhat more fully, I content myself for the

present with remarking that I am inclined to regard the base $C_8H_7N_3O$ as the creatinine of the benzoic series; it stands to anthranilic acid exactly in the same relation as creatinine "par excellence" does to sarcosine :—

$$\underbrace{C_8H_7N_3O.}_{\text{Benzo-creatinine.}} \qquad \underbrace{C_4H_7N_3O.}_{\text{Creatinine.}}$$

$$\underbrace{C_7H_7NO_2.}_{\text{Anthranilic acid.}} \qquad \underbrace{C_3H_7NO_2.}_{\text{Sarcosine.}}$$

Herr Neubauer has shown* that creatinine, when treated in a sealed tube with baryta-water, undergoes the following change :—

$$\underbrace{C_4H_7N_3O}_{\text{Creatinine.}}+H_2O=\underbrace{C_4H_6N_2O_2}_{\text{Methylhydantoine.}}+NH_3.$$

I consider it highly probable that the base $C_8H_7N_3O$ will split up in like manner with the formation of the above-described compound $C_8H_6N_2O_2$, according to the equation

$$C_8H_7N_3O+H_2O=C_8H_6N_2O_2+NH_3.$$

Indeed this latter compound exhibits great resemblance in its chemical deportment to the methylhydantoine of Herr Neubauer.

In conclusion, I should point out that the azodioxindol described by Herrn Baeyer and Knop in their paper on indigo-blue* is isomeric with the before-mentioned compound, $C_8H_6N_2O_2$. These two bodies show, moreover, great similarity in other respects, so much so that I should feel inclined to view them as identical if their fusing-points did not differ essentially. Herrn Baeyer and Knop state that the fusing-point of their azodioxindol is 300° C., while the compound I obtained fuses above 350° C. Should it turn out, however, on further investigation that the two bodies are identical, the compound $C_8H_6N_2O_2$ would have to be regarded as the first derivative of indigo which has ever been prepared synthetically, and which, like indigo-blue itself, contains eight atoms of carbon.

VI. "On the successive Action of Sodium and Iodide of Ethyl on Acetic Ether." By J. ALFRED WANKLYN, F.C.S. &c. Communicated by Professor WILLIAMSON. Received July 16, 1869.

In a remarkable paper which appeared in the Philosophical Transactions, vol. clvi. p. 37 (1866), Frankland and Duppa described the products obtained on treatment with iodide of ethyl of the yellow wax-like mass given by the action of sodium on acetic ether. Besides the description of the compounds, Frankland and Duppa give a theory of their origin,

* Ann. der Chem. und Pharm. vol. cxl. p. 26.

which theory is embodied in four equations expressive of Frankland and Duppa's view of the origin of the wax-like mass. As I have already pointed out, each one of these four equations affirms the evolution of an equivalent of hydrogen by every equivalent of sodium employed.

I have shown that acetic ether does not evolve hydrogen by reaction with the alkali metals. Equations which assume evolution of hydrogen in these reactions are therefore, in my opinion, inadmissible.

At the end of my paper in the January Number of Liebig's 'Annalen,' I promised to give an explanation of Frankland and Duppa's products, which should not involve the assumption of evolution of hydrogen. That explanation I now give.

On reference to Frankland and Duppa's paper just cited, it will be found that the products described by them as obtained from the "wax-like mass" and iodide of ethyl are the following:—

A. $C_8\,H_{14}\,O_3$, liquid boiling at 195° C.,
B. $C_{10}\,H_{18}\,O_3$, liquid boiling at 210° C. to 212° C.,

butyric ether, caproic ether, and also some unacted upon acetic ether, and a considerable quantity of common ethylic ether.

The history of these compounds is therefore the task set before me.

I have already shown that the direct products of the action of sodium on acetic ether are ethylate of sodium and sodium-triacetyl. Nothing else seems to be produced directly. But the excess of acetic ether, which is necessarily taken, acts on some of the ethylate of sodium, producing alcohol and acetate of ethylene-sodium in the manner described by me on a former occasion. (Of course the extent to which this secondary action takes place will be determined by the exact circumstances of the experiment.) We have, therefore, in the wax-like mass got by prolonging the action of sodium on acetic ether:—

Ethylate of sodium	$C_2\,H_5\,NaO$
Sodium-triacetyl	$C_6\,H_9\,O_3\,Na$
Acetate of ethylene-sodium............	$C_4\,H_7\,Na\,O_2$
Alcohol	$C_2\,H_6\,O$.

On the first three iodide of ethyl acts, giving iodide of sodium and organic liquids.

From the ethylate of sodium comes the common ether.

From the sodium-triacetyl comes ethyl-triacetyl, which is $A=C_8\,H_{14}\,O_3$, having been got by Geuther from the pure sodium-triacetyl.

From isolated acetate of ethylene-sodium and iodide of ethylene I have recently obtained liquid B, $C_{10}\,H_{18}\,O_3$, thus:—

Acetate of ethylene-sodium. Alcohol.

$$2C_4\,H_7\,NaO_2 + 2C_2\,H_5\,I = 2Na\,I + C_2\,H_6\,O + C_{10}\,H_{18}\,O_3.$$

The liquid prepared by me boiled at 212° C., and gave carbonate of

baryta with baryta-water, and was identical with Frankland and Duppa's liquid B.

By the action of liquid A upon ethylate of sodium Geuther has recently shown that butyric ether is produced. Geuther's reaction I write thus :—

Acetate of ethylene-sodium. Butyric ether.

A. $C_8 H_{14} O_3 + C_2 H_5 NaO = NaC_2 H_4, C_2 H_3 O_2 + C_6 H_{12} O_2.$

Finally, I predict that liquid B will give

Caproic ether.

B. $C_{10} H_{18} O_3 + C_2 H_5 NaO = NaC_2 H_4 C_2 H_3 O_2 + C_8 H_{16} O_2.$

VII. "On Approach caused by Vibration." By FREDERICK GUTHRIE. Communicated by Prof. G. G. STOKES, Sec. R. S. Received August 26, 1869.

(Abstract.)

The author observes that when a vibrating tuning-fork is held near to a piece of cardboard, the latter has a tendency to approach the fork. Starting from this experiment, a series of experiments is described having for their object the determination of the cause and conditions of the fundamental observed fact.

It is shown that no sensible permanent air-currents, having their source at the fork's surface, are established; and hence that the approach of the card to the fork is not due to the expansion of such currents as in M. Clement's experiment.

The modifications are examined which Mr. Faraday's surface-whirlwinds on a vibrating tuning-fork undergo when the fork vibrates in the neighbourhood of a sensibly rigid plane.

It is shown that a delicately suspended card approaches the fork when either of the three essential faces of the fork is presented to the card, and that the approach takes place from distances far exceeding the range of Mr. Faraday's air-current. That the action between the card and fork is mutual is shown by suspending the latter. Also one vibrating fork tends to approach another in whatever sense their planes of vibration may be towards one another.

The mean tension of the air surrounding a vibrating fork is examined by enclosing one limb of the fork in a glass tube. It appears that the vibrating fork displaces air.

The question whether the equilibrium between two equal and opposite forces acting on a body is disturbed by submitting one of the forces to successive, rapid, equal, and opposite alterations in quantity, is answered in the negative by an experiment which shows that the equilibrium of a Cartesian diver is not disturbed by submitting the water in which it floats to vibration.

Various modifications are introduced into the nature of the surface

which receives the vibrations, such as making it a narrow cylinder with one end closed, making it of cotton-wool, &c. It is found that in all cases the suspended body approaches the vibrating one.

The author concludes that the effect of apparent attraction is due to atmospheric pressure, and that this pressure is due to undulatory dispersion. It is suggested that the dispersion of the vibrations which constitute radiant heat may cause bodies to approach, being pushed not pulled.

November 18, 1869.

Lieut.-General Sir EDWARD SABINE, K.C.B., President, in the Chair.

In pursuance of the Statutes, notice of the ensuing Anniversary Meeting was given from the Chair.

Dr. T. Graham Balfour, Mr. Fergusson, Mr. Hanbury, Dr. Hoskins, and Mr. Tomlinson, having been nominated by the President, were elected by ballot Auditors of the Treasurer's Accounts on the part of the Society.

Mr. William Esson, and Mr. Edward Walker were admitted into the Society.

The Presents received were laid on the Table, and thanks ordered for them, as follows :—

Transactions.

Apt (Vaucluse):—Société Littéraire, Scientifique et Artistique. Annales; quatrième année, 1866–67. 8vo. *Apt* 1869. The Society.

Batavia:—Koninklijke Natuurkundige Vereeniging in Nederlandsch Indië. Natuurkundig Tijdschrift. Deel XXX. Afl. 3–6. 8vo. *Batavia* 1868. The Society.

Berlin:—Königliche Akademie der Wissenschaften. Abhandlungen, aus dem Jahre 1868. 4to. *Berlin* 1869. Monatsbericht. April–August 1869. 8vo. *Berlin* 1869. The Academy.

Verein zur Beförderung des Gewerbfleisses in Preussen. Verhandlungen, Jan.-April 1869. 4to. *Berlin* 1869. The Society.

Boston Society of Natural History. Memoirs. Vol. I. Part 4. 4to. *Boston* 1869. The Society.

Cambridge Philosophical Society. Transactions. Vol. X. Title and Index. Vol. XI. Part 2. 4to. *Cambridge* 1869. Proceedings. Parts 3–6. 8vo. *Cambridge* 1866–67. The Society.

Copenhagen:—Kongelige Danske Videnskabernes Selskab. Skrifter. 5 Række. Nat. og Math. Afd. Bind. VIII. 2 (Lütken). 4to. *Kjöbenhavn* 1869. Oversigt 1815–16, 1816–17, 1817–18, 1818–19, 1819–20, 1820–21, 1821–22, 1822–23, 1824–25, 1825–26, 1826–27, 1827–28, 1828–29, 1829–30, 1830–31, 1833–34, 1836–37. 4to. 1842, 1860, 1867, Nos. 6–7; 1868, Nos. 1–4; 1869, No. 1. 8vo. *Kjöbenhavn.* The Society.

Transactions (*continued*).

Dublin:—Trinity College. Observations made at the Magnetical and Meteorological Observatory, under the direction of Humphrey Lloyd, D.D. Vol. II. 1844–50. 4to. *Dublin* 1869.
The Provost and Senior Fellows.

Edinburgh:—Royal Physical Society. Proceedings. Session 1865–66. 8vo. *Edinburgh.* The Society.

Royal Scottish Society of Arts. Transactions. Vol. VII. Parts 4, 5. 8vo. *Edinburgh* 1868. The Society.

Greenwich:—Royal Observatory. Astronomical, Magnetical, and Meteorological Observations made in the year 1867, under the direction of G. B. Airy. 4to. *London* 1869. The Admiralty.

Halle:—Naturw. Verein für Sachsen und Thüringen. Zeitschrift für die gesammten Naturwissenschaften: redigirt von C. Giebel und M. Siewert. Band XXXII. 8vo. *Berlin* 1868. The Society.

Heidelberg:—Naturhistorisch-medizinischer Verein. Verhandlungen. Band II., IV., V., 2. 8vo. *Heidelberg* 1862–69. The Society.

Kiel:—Universität. Schriften, aus dem Jahre 1868. Band XV. 4to. *Kiel* 1869. The University.

London:—Anthropological Society. Anthropological Review. Nos. 16, 17, 26, 27. 8vo. *London* 1867–69. The Society.

Army Medical Department. Report for the year 1867. Vol. IX. 8vo. *London* 1869. The Department.

British Association for the Advancement of Science. Report of the Thirty-eighth Meeting, held at Norwich in August 1868. 8vo. *London* 1869. The Association.

Entomological Society. Transactions. Third Series. Vol. III. Part 7. For the year 1869, Parts 2–4. 8vo. *London* 1869. The Society.

Institution of Civil Engineers. Minutes of Proceedings. Vols. XXVII., XXVIII. 8vo. *London* 1868–69. The Institution.

Institution of Naval Architects. Transactions. Vol. X. 4to. *London* 1869. The Institution.

Linnean Society. Transactions. Vol. XXVI. Part 3. 4to. *London* 1869. Proceedings, 1868–69, pp. xli–cxxxiv. Journal. Botany, Vol. X. No. 48; Vol. XI. Nos. 50 & 51; Vol. XII. Zoology, Vol. X. No. 46. 8vo. *London* 1869. The Society.

Meteorological Office. Weather Reports, Jan. 1 to June 30, 1869. Fol. *London.* Report of the Meteorological Committee of the Royal Society for the year ending 31st December 1868. 8vo. *London* 1869. Report to the Committee on the Meteorology of the North Atlantic between the parallels of 40° and 50° N., by Captain Henry Toynbee. 8vo. *London* 1869. Charts showing the Surface-temperature of the South-Atlantic Ocean in each month of the year. Fol. *London* 1869. The Office.

Transactions (*continued*).

Odontological Society of Great Britain. Transactions. Vol. VI. New Series. Vol. I. No. 8. 8vo. *London* 1868–69. The Society.

Royal Agricultural Society. Journal. Second Series. Vol. V. Part 2. 8vo. *London* 1869. The Society.

Zoological Society. Transactions. Vol. VI. Part 8. 4to. *London* 1869. Proceedings of the Scientific Meetings for the year 1869. Part 1. 8vo. *London.* The Society.

Luxembourg:—Société des Sciences Naturelles. Bulletin. Tome X. Années 1867 et 1868. 8vo. *Luxembourg* 1869. The Society.

Lyon:—Académie Impériale des Sciences, Belles-Lettres et Arts. Mémoires. Classe des Sciences, Tome X.–XV. 1860–66; Classe des Lettres, Nouvelle Série, Tome VII.–X., XII., XIII., 1858–68. 8vo. *Lyon.* The Academy.

Manchester:—Literary and Philosophical Society. Memoirs. Third Series. Vol. III. 8vo. *London* 1868. Proceedings. Vols. V.–VII. 8vo. *Manchester* 1866–68. The Society.

Milan:—Società Italiana di Scienze Naturali. Memorie. Tomo II. n. 3 Tomo IV. n. 1–3. 4to. *Milano* 1867–68. Atti. Vol. XI. fasc. 2–4. 8vo. *Milano* 1868–69. The Society.

Naples:—Società Reale. Rendiconto delle Tornate e dei Lavori dell' Accademia di Scienze Morali e Politiche. Anno 8, Gennaio-Agosto. 8vo. *Napoli* 1869. The Society.

Newcastle-upon-Tyne:—Chemical Society. Transactions. Part I. 8vo. *Newcastle-upon-Tyne* 1869. The Society.

Oxford:—Radcliffe Observatory. Results of Astronomical and Meteorological Observations made in the year 1866, under the superintendence of the Rev. Robert Main. Vol. XXVI. 8vo. *Oxford* 1869. The Radcliffe Trustees.

Palermo:—R. Istituto Tecnico. Giornale di Scienze Naturali ed Economiche. Anno 1869, Vol. V. fasc. 1–2; parte 1, Scienze Naturali. Roy. 8vo. *Palermo* 1869. The Institute.

Paris:—Institut Impérial de France. Annuaires pour 1810, 1811, 1838, 1839, 1841, 1844, 1845, 1848, 1849, 1850, 1858, 1862, 1863, 1866–69. 12mo. *Paris.* The Institute.

Société Géologique de France. Bulletin. 2e série. Tome XXV. ff. 56–64; Tome XXVI. no. 2. 8vo. *Paris* 1869. The Society.

Philadelphia:—Academy of Natural Sciences. Journal. New Series. Vol. VI. Part 3. 4to. *Philadelphia* 1869. Proceedings. 1868, Nos. 1–6. 8vo. *Philadelphia.* The Academy.

American Philosophical Society. Transactions. Vol. XIII. Part 3. 4to. *Philadelphia* 1869. The Society.

St. Petersburg:—Académie Impériale des Sciences. Mémoires. Tome XII. Nos. 4–5; Tome XIII. Nos. 1–7; Tome XIV. Nos. 1, 2. 4to.

Transactions (*continued*).

St. Pétersbourg 1869. Bulletin. Tome XIII. Nos. 4–5. 4to. *St. Pétersbourg* 1868–69. The Academy.

Salem (Mass.):—Peabody Academy of Science. Memoirs. Vol. I. No. 1. Roy. 8vo. *Salem* 1869. The Academy.

Venice:—Ateneo Veneto. Atti. Serie 2. Vol. IV. puntata 2–3; Vol. V. p. 1–3. 8vo. *Venezia* 1867–68. The Ateneo.

Reale Istituto Veneto di Scienze, Lettere ed Arti. Memorie. Vol. XIV. parte 1, 2. 4to. *Venezia*, 1868–69. Atti. Serie 3. Tomo XII. disp. 10; Tomo XIII. disp. 1–10; Tomo XIV. disp. 1–5. 8vo. *Venezia* 1866–69. The Institute.

Vienna:—Kaiserliche Akademie der Wissenschaften. Sitzungsberichte. Math.-Nat. Classe, I. Abtheilung: Band LVII. Hefte 4, 5. II. Abth.: Band LVII. Hefte 4, 5; Band LVIII. Heft 1. Phil.-hist. Classe: Band LIX. Hefte 1–4. 8vo. *Wien* 1868. Anzeiger. Jahrg. 1869, Nr. 12–21. 8vo. *Wien.* The Academy.

K.-K. Geologische Reichsanstalt. Jahrbuch. Band XIX. Nr. 1. 8vo. *Wien* 1869. The Society.

Zürich:—Allgemeine Schweizerische Gesellschaft für die gesammten Naturwissenschaften. Neue Denkschriften. Band XXXIII. 4to. *Zürich* 1869. The Society.

Naturforschende Gesellschaft. Vierteljahrsschrift. Redigirt von Rudolph Wolf. Jahrg. 12, 13. 8vo. *Zürich* 1867–68. An die zürcherische Jugend, auf die Jahre 1868, 1869. Stück 70, 71. 4to. *Zürich* 1868–69. The Society.

Attfield (J.) Chemistry, general, medical, and pharmaceutical. 8vo. *London* 1869. The Author.

Brunnow (F.) Traité d'Astronomie Sphérique et d'Astronomie Pratique, édition Française, publiée par E. Lucas et C. André. Astronomie Sphérique. 8vo. *Paris* 1869. The Translators.

Casey (J.) On Bicircular Quartics. 4to. *Dublin* 1869. The Author.

Clarke (B.) A New Arrangement of Phanerogamous Plants. Oblong 4to. *London* 1866. Dr. John Millar.

Edmonds (T. R.) On Vital Force, according to Age and the "English Life Table." 8vo. *London* 1869. The Author.

Faugère (P.) Défense de B. Pascal et accessoirement de Newton, Galilée, Montesquieu, etc., contre les faux documents présentés par M. Chasles à l'Académie des Sciences. 4to. *Paris* 1868. The Author.

James (Sir H., F.R.S.) Notes on the Great Pyramid of Egypt and the Cubits used in its design. 4to. *Southampton* 1869. The Author.

Jones (T. Wharton, F.R.S.) Failure of Sight from Railway and other Injuries of the Spine and Head; its nature and treatment. 8vo. *London* 1869. The Author.

Kops (J.) Flora Batava. Afl. 208–210. 4to. *Amsterdam.*
H.M. The King of the Netherlands.

Lea (Isaac.) Observations on the Genus Unio. Vol. XII. 4to. *Philadelphia* 1869. The Author.

Le Verrier (U. J., For. Mem. R.S.) Annales de l'Observatoire Impérial de Paris. Mémoires. Tome IX. 4to. *Paris* 1868. The Observatory.

——— Examen de la Discussion soulevée au sein de l'Académie des Sciences au sujet de la découverte de l'Attraction Universelle. 4to. *Paris* 1869. The Author.

Lubbock (Sir John, F.R.S.) Prehistoric Times, as illustrated by Ancient Remains and the Manners and Customs of Modern Savages. Second edition. 8vo. *London* 1869. The Author.

Miller (Dr. W. A., Treas. R.S.) Elements of Chemistry, theoretical and practical. Fourth edition. Part 3. Organic Chemistry. 8vo. *London* 1869. The Author.

Penrose (F. C.) On a Method of predicting by graphical construction Occultations of Stars by the Moon, and Solar Eclipses for any given place. 4to. *London* 1869. The Author.

Quadri (Achille.) Note alla Teoria Darwiniana. 8vo. *Bologna* 1869.
Charles Darwin, F.R.S.

Thomas (Lynall.) The True Basis for the Construction of Heavy Artillery. 8vo. *London* 1869. The Author.

Willis (Rev. R., F.R.S.) The Architectural History of the Conventual Buildings of the Monastery of Christ Church in Canterbury. 8vo. *London* 1869. The Author.

Royal Commission on Water Supply. Report of the Commissioners. Fol. *London* 1869. Dr. W. Pole, F.R.S.

Six Photographs of the National Memorial to Sir John Franklin, in Waterloo Place, London, by M. Noble. M. Noble, Esq.

Ordnance Survey of Jerusalem. Five Plans, in case. Folio 1865.
Sir H. James, F.R.S.

The following communication was in part read :—

"Preliminary Report of the Scientific Exploration of the Deep Sea in H.M. Surveying Vessel 'Porcupine' during the Summer of 1869," conducted by Dr. Carpenter, V.P.R.S., Mr. J. Gwyn Jeffreys, F.R.S., and Prof. Wyville Thomson, LL.D., F.R.S. Received November 18, 1869.

November 25, 1869.

Lieut.-General Sir EDWARD SABINE, President, K.C.B., in the Chair.

In accordance with the Statutes, notice was given from the Chair of the ensuing Anniversary Meeting, and the list of Officers and Council proposed for election was read as follows:—

President.—Lieut.-General Sir Edward Sabine, R.A., K.C.B., D.C.L., LL.D.

Treasurer.—William Allen Miller, M.D., D.C.L., LL.D.

Secretaries.— { William Sharpey, M.D., LL.D.
George Gabriel Stokes, Esq., M.A., D.C.L., LL.D.

Foreign Secretary.—Prof. William Hallows Miller, M.A., LL.D.

Other Members of the Council.—Frederick Currey, Esq., M.A.; Warren De La Rue, Esq., Ph.D.; Sir Philip de M. Grey Egerton, Bart.; William Henry Flower, Esq.; William Huggins, Esq.; John Gwyn Jeffreys, Esq.; John Marshall, Esq.; Augustus Matthiessen, Esq., Ph.D.; George Henry Richards, Capt. R.N.; The Marquis of Salisbury, M.A.; Charles William Siemens, Esq.; John Simon, Esq.; Archibald Smith, Esq., M.A.; Prof. Henry J. Stephen Smith, M.A.; Prof. John Tyndall, LL.D.; Prof. Alexander W. Williamson, Ph.D.

The following communication was read:—

"On the Action of Cyanogen on Anthranilic Acid." By P. Griess, F.R.S. Received June 29, 1869. (See page 89.)

The reading of the Preliminary Report by Dr. Carpenter, Mr. Gwyn Jeffreys, and Dr. Wyville Thomson was resumed and concluded.

The contents of the Report will be given in a future Number.

The Presents received were laid on the Table, and thanks ordered for them, as follows:—

Transactions.

Amsterdam:—Koninklijke Akademie van Wetenschappen. Verhandelingen. Afdeeling Letterkunde. Deel IV. 4to. *Amsterdam* 1869. Verslagen en Mededeelingen. Afdeeling Natuurkunde. Tweede Reeks. Deel III. 8vo. *Amsterdam* 1869. Jaarboek voor 1868. 8vo. *Amsterdam.* Processen-Verbaal van de gewone Vergaderingen. Afdeeling Natuurkunde, 1868–69. 8vo. *Amsterdam.*
The Academy.

Genootschap Natura Artis Magistra. Bijdragen tot de Dierkunde. Afl. 9. 4to. *Amsterdam* 1869. The Society.

Bologna:—Accademia delle Scienze dell' Istituto. Memorie. Serie 2.

Transactions (*continued*).

Tomo VII., VIII. 4to. *Bologna* 1868–69. Rendiconto delle Sessioni 1867–68, 1868–69. 8vo. *Bologna* 1868–69. The Academy.

Bombay:—Geographical Society. Transactions. From January 1865 to December 1867. Vol. XVIII. 8vo. *Bombay* 1868. The Society.

London:—Clinical Society. Transactions. Vol. II. 8vo. *London* 1869. The Society.

Royal Geographical Society. Journal. Vol. XXXVIII. 8vo. *London* 1868. Proceedings. Vol. XIII. Nos. 3–4. 8vo. *London* 1869. The Society.

Saint Bartholomew's Hospital. Reports. Vol. V. 8vo. *London* 1869. The Hospital.

Munich:—Kön. Bayerische Akademie der Wissenschaften. Abhandlungen. Math.-Phys. Classe: Band. X. Abth. 2. Hist. Classe: Band XI. Abth. 1. Philos.-Philol. Classe: Band. XI. Abth. 3. 4to. *München* 1868. Sitzungsberichte, 1869. I. Heft 1–3. 8vo. *München* 1869. Ueber die Entwicklung der Agriculturchemie. Festrede von August Vogel. 4to. *München* 1869. Denkschrift auf Carl Friedr. Phil. von Martius, von C. F. Meissner. 4to. *München* 1869. The Academy.

Newcastle-upon-Tyne:—Natural-History Transactions of Northumberland and Durham. Part 1, Vol. III. 8vo. *London* 1869. The Tyneside Naturalists' Field Club.

Stockholm:—Kongliga Vetenskaps-Akademie. Handlingar. Ny Följd Bandet V. Häftet 2; Bandet VI. Häftet 1–2; Bandet VII. Häftet 1. 4to. *Stockholm* 1864–67. Œfversigt af .... Förhandlingar Årgången 22–25. 1865–68. 8vo. *Stockholm* 1866–69. Meteorologiska Jakttagelser i Sverige. Bandet VI., VII., VIII. 4to. *Stockholm* 1864–66. Kongliga Svenska Fregatten Eugenies Resa Omkring Jorden under befäl af C. A. Virgin åren 1851–53. Zoologi VI. 4to. *Stockholm* 1868. Lefnadsteckningar. Band I. Häfte 1. 8vo. *Stockholm* 1869. Igelström (L. I.), A. E. Nordenskiöld, and F. L. Ekman. On the existence of rocks containing Organic substances in the fundamental gneiss of Sweden. 8vo. *Stockholm*. Lindström (G.) Om Gotlands Nutida Mollusker. 8vo. *Wisby* 1868. Linnarsson (J. G. O.) On some fossils found in the Eophyton Sandstone at Lugnås in Sweden. 8vo. *Stockholm* 1869. Lovén (S.) Om en märklig i Nordsjön lefvande art af Spongia. 8vo. *Stockholm* 1868. Nordenskiöld (A. E.) Sketch of the Geology of Spitzbergen. 8vo. *Stockholm* 1867. Stål (Carolus.) Hemiptera Africana. Tomi I.–IV. 8vo. *Holmiæ* 1864–66. Sundeval (C. J.) Conspectum Avium Picinarum. 8vo. *Stockholmiæ* 1866. Die Thierarten des Aristoteles von den Klassen der Säugethiere, Vögel, Reptilien, und Insecten. 8vo. *Stockholm* 1863. The Academy.

Coffin (M.) Métaphysique du Calcul Différentiel. 8vo. *Arras* 1869. The Author.

Galton (F., F.R.S.) Hereditary Genius: an Inquiry into its Laws and Consequences. 8vo. *London* 1869. The Author.

Gautier (—.) Notices Sommaires sur Divers Travaux, Rapports et Etablissements Astronomiques. 8vo. *Genève.* The Author.

Listing (J. B.) Ueber eine Art stereoskopischer Wahrnehmung. 12mo. *Göttingen* 1869. The Author.

Zeitschrift für Biologie, von L. Buhl, M. Pettenkofer, &c. Band II. Heft 2, 3; Band IV. Heft 2–4. 8vo. *München* 1866–69. The Editors.

November 30, 1869.

ANNIVERSARY MEETING.

Lieut.-General Sir EDWARD SABINE, K.C.B., President, in the Chair.

Dr. T. Graham Balfour, on the part of the Auditors of the Treasurer's Accounts appointed by the Society, reported that the total receipts during the past year, including a balance of £493 14*s.* 6*d.* carried from the preceding year, and the Oliveira and Davy bequests, amount to £6753 6*s.* 3*d.*; and that the total expenditure in the same period, including the Oliveira bequest on deposit, and the investment of the Davy bequest, amounts to £6429 1*s.* 8*d.*, leaving a balance of £298 18*s.* 5*d.* at the Bankers, and of £25 6*s.* 2*d.* in the hands of the Treasurer.

The thanks of the Society were voted to the Treasurer and Auditors.

The Secretary read the following Lists:—

Fellows deceased since the last Anniversary.

On the Home List.

The Rev. Henry Hervey Baber, M.A.
Arthur Kett Barclay, Esq.
The Rev. John Barlow, M.A.
Sir John Peter Boileau, Bart.
John Cam Hobhouse, Lord Broughton.
William Clark, M.D.
John Dickinson, Esq.
William Fishburn Donkin, Esq., M.A.
Sir Henry Ellis, K.H.
James David Forbes, LL.D.
Thomas Graham, M.A., D.C.L.
Joseph Hodgson, Esq.
John Hogg, Esq., M.A.
Levett Landon Boscawen Ibbetson, K.R.E. & H.
Joseph Beete Jukes, Esq., M.A.
Joseph Jackson Lister, Esq.
George Lowe, Esq.
Gilbert Wakefield Mackmurdo, Esq.
John Rogers, Esq.
Peter Mark Roget, M.D.
Sir James Emerson Tennent, Bart., LL.D.
Lieut.-General Thomas Perronet Thompson, M.A.
George Witt, Esq.

On the Foreign List.

Carl Friedrich Philip von Martius.

Johannes Evangelista Purkinje.

Change of Title.

The Bishop of London	to	Archbishop of Canterbury.
Lord Stanley	to	Earl of Derby.
Lord Justice Sir W. Page Wood	to	Lord Hatherley.

Fellows elected since the last Anniversary.

Sir Samuel White Baker, M.A.
John J. Bigsby, M.D.
Charles Chambers, Esq.
William Esson, Esq., M.A.
Prof. George Carey Foster, B.A.
Robert Arthur Talbot Gascoigne-Cecil, Marquis of Salisbury, M.A.
William W. Gull, M.D.
Richard Monckton Milnes, Lord Houghton, M.A., D.C.L.
J. Norman Lockyer, Esq.
John Robinson M'Clean, Esq.
St. George Mivart, Esq.
John Russell Reynolds, M.D.
Vice-Admiral Sir Robert Spencer Robinson, K.C.B.
Major James Francis Tennant, R.E.
Prof. Wyville Thomson, LL.D.
Col. Henry Edward Landor Thuillier, R.A.
Edward Walker, Esq., M.A.

On the Foreign List.

Alphonse De Candolle.
Charles Eugène Delaunay.
Louis Pasteur.

Readmitted.

Sir John Macneill.
Edward Solly, Esq.

The President then addressed the Society as follows:—

Gentlemen,

One of the first subjects to which I have to draw your attention is the Royal Society's Catalogue of Scientific Papers, the printing of which, I am happy to report, proceeds satisfactorily. The third volume, which I lay before you, is now completed, carrying the Index of Titles in alphabetical order as far as Lez inclusive; and good progress has been made in correcting the proofs of the fourth volume. Each succeeding volume of the work, of course, adds to its practical utility, which continues to be thankfully acknowledged by cultivators of science in various parts of the world.

But while the aid to be derived to scientific research from the index arranged according to authors' names is fully recognized, there can be no doubt that the value of the Catologue will be greatly enhanced by the fulfilment of the second part of the plan announced in the preface, namely, by the publication of an *Alphabetical Index of Subjects.* The preparation of such an "*Index Rerum*" as is contemplated has been for some time a subject of anxious as well as careful consideration by the Library

Committee, and they have at length arrived at what, they have reason to hope, will be a most satisfactory solution of the question, through a communication with Professor Julius Victor Carus, of Leipsic, who they found would be willing himself to undertake the task. I am happy to announce that the Council, acting on the recommendation of the Library Committee, have entered into a very satisfactory arrangement with Professor Carus, who will be able to commence his labours in the ensuing spring. From the well-known scientific accomplishment of Professor Carus, and his extensive experience in the peculiar work to be performed, as well as the confidence which will be reposed in him by all acquainted with the nature of the undertaking and interested in its success, we may consider the Society most fortunate in securing his services.

The Meteorological Department of the Board of Trade, superintended by a Committee of the Royal Society, is making good progress, under the able direction of Mr. Robert Scott, towards the fulfilment of the objects for which it was constituted. In respect to the Meteorology of the United Kingdom, the seven observatories distributed over its surface and maintained at the public expense are all in thoroughly good working order, transmitting their self-recorded results monthly to the central establishment, where they undergo a careful revision before their final acceptance. The first publication of the numerical results, which will be complete for each of the seven observatories for the year 1869, will take place towards the end of the first quarter of 1870, and similarly in subsequent years, and will be followed at brief intervals by graphical representations illustrating the phenomena of the weather at times of its most important disturbances.

The other departments of the office show also a healthy activity. As regards Ocean Meteorology, the Committee have been enabled to increase their staff, and so to accelerate materially the investigations alluded to in my address of last year; while the collection of new observations of a high character is also going on steadily. The system of Weather Telegraphy is making solid advances. The Drum Signal is now hoisted at upwards of 100 British Stations, and intelligence of atmospheric disturbances felt on our shores is transmitted to the coasts of the continent from Norway to Spain. The results of the transmission of such news to Hamburg have been especially satisfactory.

The extension of telegraphic communication to the north of Scotland has enabled the Committee to adopt Wick as an observing-station, while the Norwegian authorities have resolved to make use of the direct cable laid down last summer between Scotland and their coast, to exchange information daily with the office in London. Hitherto the reports from Norway have always reached us *viâ* Paris, whereby delays were occasioned.

The attention of the Committee has also been directed to instituting discussions of the statistics of our weather. The results already obtained in this field lead us to hope that the practical value of such inquiries will soon be manifested.

The great Melbourne Telescope arrived at its destination in November 1868, without injury of any importance—which, perhaps, could hardly have been expected after a voyage of 16,000 miles, for an instrument at once so massive and so delicate!

The Visitors of the Melbourne Observatory thought it advisable to adopt the suggestion of Dr. Robinson to provide the telescope with a covering, and for this purpose they preferred the second of the plans which he proposed—a rolling roof. This appears to have been satisfactorily executed. It protects the telescope completely, and can be removed by a single workman, leaving the telescope fully exposed to the sky.

In erecting the instrument some trifling difficulties seem to have been experienced, and it was not fit for actual work until the beginning of last June, which is midwinter there, a season when cloudy weather prevails to an extent which we were scarcely prepared to expect, and which is stated to have been this year excessive. For these reasons the habitual work of the telescope had not been commenced up to September.

Its performance since erection does not appear to have given altogether the same satisfaction at Melbourne that it did at Dublin; but the defects complained of may arise partly from an imperfect knowledge of the principles of the instrument and inexperience in the use of so large a telescope, partly from experimental alterations made at Melbourne, and partly from atmospherical circumstances. Those who are acquainted with the difficulties which Sir J. F. W. Herschel experienced at the Cape, will not be surprised that they should be felt at Melbourne to a much greater extent, on account of the far greater size of the speculum. But I have no doubt that if the instrument be kept in its original condition and as carefully adjusted as it was at Dublin, it will perform as well in ordinary observing-weather.

The high impression of its power produced by the trials which were made of it when at Dublin, is maintained by a sketch of a portion of the Great Nebula near η Argus, made by M. Le Sueur during two nights in June last.

Some change in this nebula from the time when it was described by Sir J. F. W. Herschel had been indicated by Mr. Powell and other observers, though with instruments so much inferior in power to his 20-foot reflector, that little reliance could be placed on them; however, here the evidences of change are indisputable. The peculiar opening in the nebula, which Sir John Herschel has compared to a lemniscate, is still very sharply marked, but its shape and magnitude have altered. Its northern extremity is opened out into a sort of estuary; one of the remarkable constrictions seen in 1834 has disappeared, and the other has shifted its place. Two stars which were then exactly on the edges of the opening are now at some distance within the bright nebulosity; the nebula has become comparatively faint near η Argus.

Another remarkable change is the formation of a V-shaped bay south

and preceding the lemniscate, whose edges are so bright that if it had then existed it could not have been overlooked in the 20-foot reflector. Another feature, which, however, was perhaps not within reach of that telescope, is an oval which M. Le Sueur describes as "full of complicated dark markings and pretty bright nebular filaments." The angular magnitude of the changes which have been thus observed is so great as to suggest a strong probability that this nebula is *much nearer* to us than the stars which are seen along with it. It may be also noticed that M. Le Sueur saw nothing to make him believe in any development of stars in addition to those seen by Sir J. F. W. Herschel.

The spectroscope and photographic apparatus belonging to the instrument have by this time reached Melbourne, and will no doubt give good results, subject to the condition that the fascination of their use shall not be permitted to interfere with the primary destination of the telescope, viz. the observation of nebulæ.

Celestial spectroscopy has indeed attained such importance, that it requires for its successful prosecution the undivided attention of the astronomer who devotes himself to it, as well as an observatory specially designed for it. Our great national observatories cannot supply this want, for they have their own specific destination; and the high optical power which is required, if we wish to make further progress, is scarcely within the reach of amateurs.

These considerations have induced your Council to believe that an attempt to encourage and aid this most interesting class of researches is an object in full unison with the highest purpose of the Royal Society's existence; and they have therefore, after most careful deliberation, resolved to act on this conviction by providing a telescope of the highest power that is conveniently available for spectroscopy and its kindred inquiries. The instrument will, of course, be the property of the Society, and will be intrusted to such persons as, in their opinion, are the most likely to use it to the best advantage for the extension of this branch of science; and, in the first instance, there can be but one opinion that the person so selected should be Mr. Huggins.

The execution of this project was much facilitated by the receipt of £1350 from a bequest made to the Society by the late Mr. Oliveira; and in the beginning of the year proposals were received from the chief opticians of the time, of which that of Mr. Grubb was accepted last April.

The conditions proposed were, that the object-glass of the telescope should be of 15 inches aperture, and not more than 15 feet focus, that the arrangements of its equatorial should be such that it could be easily worked by the observer without an assistant, and that the readings of its circles could be made without leaving the floor of the observatory. Mr. Grubb was fortunate enough to secure two disks which had been exhibited by Messrs. Chance at the French Exhibition. They are of first-rate transparency; and as the construction which has been adopted admits of

the lenses being cemented, this object-glass will transmit an unusually large portion of light. The respective indices of the glasses were determined by making facets on their edges at an angle of 60°, and observing spectral lines through the prisms thus formed with a spectroscope of such magnitude as to admit of their being placed on its table. The distinctness with which even faint lines were seen through 12 inches of the glass is a most satisfactory proof of its purity and clearness. From these Professor Stokes computed the curves for the lenses; and his numbers were almost identical with those which Mr. Grubb had obtained.

I may mention that some fears had been entertained that the equality of curvature in the adjacent surfaces might *call up a ghost*, if the lenses were used uncemented, and that this has been tried and no such effect was visible. Subsequently a rather novel addition has been made, bearing upon the radiation of heat from the stars. An object-glass intercepts so much of the heat-rays that, to economize the infinitesimal effect which is expected, a metallic mirror is more promising. The equatorial is therefore, at the suggestion of Mr. De La Rue, provided with the means of changing the 15-inch achromatic for an 18-inch reflector; and this has been accomplished by means notable for their facility and their safety.

The instrument will be ready for trial in December of the present year. In the meantime it may be said that the object-glass, notwithstanding the difficulty of working one of so short a focus, gives promise of very high excellence.

With respect to the equatorial, it has been ascertained that a force of 2 lb. applied at the eyepiece is sufficient to move the telescope easily on its declination axis, and $1\frac{1}{2}$ lb. on its polar axis; however, when all its parts are put together, these forces may require to be increased $\frac{1}{5}$.

The anticipations which I ventured to express in may last year's Address, of the renewal in the summer of the present year of the researches on the temperature of the sea at great depths, and on the nature of the sea-bottom and the life existing in its vicinity, have been realized by an ample provision on the part of Her Majesty's Government, and a devotion on the part of the Fellows of the Royal Society, viz. Dr. Carpenter, Prof. Wyville Thomson, and Mr. Gwyn Jeffreys, meriting the highest praise. The existence of persistent deep-sea currents of very different temperatures in proximity to each other, and their influence on the inhabiting forms of life and on the nature of the sea-bed, together with the great extension of our knowledge of the variety and characteristics of the new forms of life which have been discovered, justify the belief that we have embarked on a field of discovery and research which will not soon be exhausted, and which will have no unimportant bearing on the earlier geology of our globe, as well as on our knowledge of the life at present existing on the submerged portions of its surface.

It had long been inferred by naturalists that species of the marine

Invertebrata may have a far wider extension on the surface of the globe than is the case with the inhabiters of the land. The correctness of this inference received the fullest confirmation by the researches of the late Admiral Sir James Clark Ross, whose dredges brought up from the depths of the Antarctic Ocean individuals of species which were well known to him from his earlier dredging operations in the Arctic seas. These animals are known to be particularly sensitive in regard to temperature; and we have no reason to doubt his conclusion, that water of similar temperature to that of the Arctic and Antarctic seas exists in the depths of the intermediate ocean, and may have formed a channel for the dissemination of species. The barrier which the heated regions of the tropics present to the migrations of the land animals of colder climates does not exist in the case of many of those inhabitants of the sea whose remains constitute a large portion of the fossiliferous strata of the globe.

The Fellows will not have forgotten the important paper on the Flora of North Greenland by Prof. Oswald Heer, which was read last winter, and which will speedily be in their hands in the forthcoming volume of the Transactions. The inquiries carried on by this eminent botanist have determined, beyond the possibility of cavil, the climatological conditions of the Arctic regions at a geological epoch which is comparatively recent (the Miocene), and have shown that they must have resembled very closely those now prevailing in latitudes at least 20° lower; for such is the zone inhabited by the living representatives of the plants found fossil by him in the localities in which they grew.

The specimens brought by the recent Swedish Expedition from Spitzbergen have also been submitted to his examination; and it appears that a portion of these, from Advent Bay, belong to the Quaternary Epoch. It will therefore be a matter of no small interest to determine accurately the changes of climate which took place in that locality at the expiration of the Miocene era.

I proceed to the award of the Medals.

The Copley Medal has been awarded to M. Victor Regnault, Foreign Member of the Royal Society, for the second volume of his 'Relation des Expériences pour déterminer les lois et les données physiques nécessaires au calcul des Machines à Feu,' including his elaborate investigations on the Specific Heat of Gases and Vapours, and various papers on the Elastic Force of Vapours.

The name of M. Victor Regnault has been associated for the last quarter of a century with the most refined and delicate experimental inquiries connected with the measurement of heat. The amount of labour involved in his researches upon the specific heat of simple and compound bodies, upon the dilatation of gases and vapours, upon the comparison of the air-thermo-

meter with the mercurial thermometer, upon the elastic force of aqueous vapour, upon the determination of the density of gases, and upon hygrometry must excite the astonishment of all who can estimate the difficulty of the problems attacked, the precision of the results attained, and the fundamental character of the data which he has determined.

These researches were published before the year 1850; many of them were embodied in the first volume of his 'Relation des Expériences pour déterminer les lois et les données physiques nécessaires au calcul des Machines à Feu." The Royal Society marked their sense of the importance of these earlier labours of M. Regnault by the presentation of the Rumford Medal in the year 1848.

He has since published the second volume of that great work, to which more especially the Copley Medal is now awarded. It embraces a series of researches even more delicate and difficult; to use the words of one whose recent loss we all deplore, and whose opinion on this subject possesses a weight which is equalled by few, viz. the late Mr. Graham, "in these researches a degree of precision is attained, where precision is all-important, which appears never to have been surpassed, or perhaps even approached before in similar inquiries. The results are data of a fundamental character, to the completion of which chemists and natural philosophers have been looking anxiously for years past, and which they have now received from the hands of M. Regnault with a feeling of entire confidence."

The researches on the specific heat of gases and vapours, alone, constitute a monumental work. Upon this subject the most discordant results had been obtained by experimental investigators of tried skill and ingenuity; and the problem, notwithstanding its importance, exhibited a series of perplexing contradictions.

Before commencing his own experiments, M. Regnault submitted the various methods of previous inquirers on the subject to a minute comparison and criticism, particularly those of Delaroche and Bérard, of Haycraft, and of Apjohn and Suesman. M. Regnault finally adopted a method based upon the one proposed by Delaroche and Bérard. The principle of it may be explained in a few words.

A current of the compressed gas under experiment is made to traverse at a uniform velocity a metallic worm maintained at a uniform temperature. The heated gas is then transmitted through a calorimeter, and the amount of the following quantities determined, viz.:—1, the weight of the gas employed; 2, the cooling of the gas; 3, the rise of temperature of the water in the calorimeter. From these data the specific heat of the gas is calculated.

Amongst various special contrivances required for avoiding error, it was necessary to have the means of regulating the escape of the gas with sufficient uniformity, and for preventing its issue from the calorimeter at varying pressures.

Some idea of the enormous amount of labour bestowed upon these researches may be formed from the fact, that not fewer than eighty-four experiments were made upon the specific heat of air, under different pressures and temperatures, forty-three upon aqueous vapour, twenty-four upon carbonic acid, and a considerable number upon other important gases and vapours, embracing no fewer than thirty-six different elementary and compound bodies, many of which required special modifications of the method and apparatus employed.

Besides this remarkable series of researches, M. Regnault has embodied in his work :—investigations on the compressibility of gases under wide variations of pressure, and on the specific heat of liquids at different temperatures; also a second memoir on the elastic force of saturated vapours at different pressures, which he has extended to the compressed gases, and to the density of vapours emitted by saline solutions and by mixed liquids. In addition to all these, he has a memoir on the latent heat of vapours at different tensions.

This extended series of investigations is carried out with minute and scrupulous precision, and the sources and limits of error are traced and guarded against with unvarying skill and sagacity. The publication of this work, the greatest experimental contribution of any single individual to the science of heat, must indeed mark an era in the history of thermotics, and furnish data of enduring value both to the chemist and the physicist in all that concerns specific and latent heat, and the laws of elastic force as acting on aëriform bodies.

Professor Miller,

We greatly regret that we are deprived of the pleasure of Monsieur Regnault's presence by reason of illness in his family. I will therefore request you to receive the Medal on his behalf, and to transmit it to him with the assurance of the Society's highest respect.

The Council has awarded a Royal Medal to Sir Thomas Maclear, Astronomer Royal at the Cape of Good Hope, for his Measurement of an Arc of the Meridian at the Cape of Good Hope.

Our sole knowledge of the figure of the southern hemisphere rests on the arc of the meridian measured by La Caille, and now remeasured and extended by Maclear. The original measurement, notwithstanding the well-known ability of the great astronomer under whose superintendence it was executed, has not commanded confidence. The magnitude of the degree inferred from it is far too great, and, if accepted, would lead to the conclusion that the dimensions of the two hemispheres are dissimilar. But La Caille's angles were observed with a quadrant, not with a circle, and were therefore liable to errors of eccentricity and of figure; while the effects of local attraction, if recognized at all, were very imperfectly ap-

preciated. These considerations induced Maclear, shortly after his appointment to the Cape Observatory, to plan the verification which he has now accomplished. Pursuing the still earlier inquiries of Sir George Everest, he succeeded, though with considerable difficulty, in recovering La Caille's terminal stations; and, aided by the advice and encouragement of Sir John Herschel (then at the Cape) and of the Astronomer Royal, he commenced the work of a remeasurement in 1836. The proceedings were necessarily tedious: the measurements of the base, of the triangles, and of the zenith-distances were repeated to an extent and with precautions unpractised at the earlier period. The zenith-distances were observed with the sector with which Bradley discovered the aberration of light and the nutation of the earth's axis, intrusted to Maclear by the Admiralty. The terrestrial angles were taken with a 20-inch circle by Jones, and a smaller theodolite by Reichenbach, both of remarkable precision. The base, from which all the distances were derived, was measured with the compensation bars used in the Irish triangulation. Thus, in respect to the means employed, this arc of the meridian may be regarded as inferior to none on record. A full account of the whole was completed in 1866, and has been published by the Admiralty in two quarto volumes. It does not confirm the abnormal value obtained by La Caille, but shows a probable cause for the discordance. La Caille's northern station was in a hollow surrounded by mountains, one of which, half a mile distant to the north, was a mass of rock 2000 feet high; and others, at distances somewhat greater, were still near enough to create disturbance. A station so situated was obviously ill suited to be a terminal station; and the triangulation was therefore extended across an immense plain of sand to a point without any visible source of local attraction. By this extension, and by a similar one to the south, Maclear's arc has an amplitude nearly four times as great as that of La Caille, and is, on this account, as well as on account of the greater accuracy in detail, far more deserving of confidence. The degree which is derived from it is 1133 feet shorter than that of La Caille; and as La Caille's is 1051 longer than that given by the spheroid which, according to Airy, represents the average of northern arcs, Maclear's determination is evidently a near approximation to the truth. This is even more distinctly shown by the close agreement of the latitudes computed from the geodetic measurements with those given by the sector—that of the north extremity being $0{\cdot}4''$ in defect, that of the south extremity $0''{\cdot}5$ in excess.

Captain Richards,

We should indeed have been happy to have had Sir Thomas Maclear's presence among us; but in his present unavoidable absence I will request you to receive this Medal on his behalf, and to transmit it to him with the assurance of the very great pleasure which it will give to the Society to welcome him on his return to his native country.

A Royal Medal has been awarded to Dr. Augustus Matthiessen, F.R.S., for his researches on the electrical and other physical properties of metals and their alloys.

The earlier of Dr. Matthiessen's published researches related to the preparation of the metals of the alkaline earths. Having succeeded in establishing or perfecting methods for the production of these, he proceeded to institute a far more complete examination of their physical properties than had previously been attempted. These researches appear to have led to his investigation of the more important physical properties of the principal metals and their alloys. In some of these investigations Dr. Matthiessen associated himself with younger workers in science of proved ability, Messrs. Holzmann, Box, and Vogt; and the results arrived at were included in a series of nine papers published in the 'Philosophical Transactions.' They embrace the determinations of the specific gravities, the expansion due to heat, the thermo-electric properties, the electric conducting-power, and the effects of temperature upon the electric conducting-power.

The laws deduced from the results of Dr. Matthiessen's electrical experiments are now in constant use by telegraphic engineers. The causes of the great variations observed in the electric conducting-power of commercial copper were first elucidated by him, and an important report was made by him on this subject in 1860 to the Committee appointed by Government to inquire into the construction of Submarine Telegraph Cables. His investigation of this subject has resulted in very great improvement of the conducting-power of the copper wire used in submarine telegraphy. Closely connected with this branch of his researches are the investigations which Dr. Matthiessen carried out for the Electrical-Standard Committee of the British Association, of which he was one of the most active members. The resistance-coils issued by that Committee, which have been very generally adopted as standard instruments, are all constructed of an alloy of platinum and tin, which, after a long series of experiments, Dr. Matthiessen recommended as specially fitted for that purpose.

Under the auspices of the British Association, Dr. Matthiessen undertook, a few years ago, the investigation of the chemical constitution of cast iron, and of the influence exerted upon the physical properties of that metal by the several other elements which generally occur in association with it. With these objects in view he has laboured most perseveringly in the preparation of iron in a chemically pure condition, and in quantities sufficient to admit of the attainment of thoroughly trustworthy results in the study of the physical and chemical properties of the pure metal and of its alloys. His researches in this direction have recently been crowned with success; and the method of producing pure iron, which he has elaborated, promises to be fruitful in interesting and important results in the hands of himself and the other chemists with whom he has been associated in this inquiry.

Dr. Matthiessen's researches, published in the Philosophical Transactions, on the action of oxidizing agents upon organic bases and on the chemical constitution of narcotics (the latter investigation having been conducted in conjunction with Professor G. C. Foster), furnish proofs of the success of his labours in organic chemistry. The accounts published in our 'Proceedings,' of the results of his most recent researches in this branch of chemical science, show that he has entered upon a line of investigation as productive of interesting and important results as any which he has yet pursued. Thus, he has already established an intimate relation between the organic bases morphia and codeia, and has shown that when either of these is treated with hydrochloric acid, a new base is produced, which he has called apomorphia, and which, though only differing from the powerful narcotic morphia by the elements of water, possesses the very distinct characteristics of a most powerful emetic. The substance known as Papaverine, hitherto regarded as a distinct organic base, is at the present time the subject of Dr. Matthiessen's study, and promises to yield results of considerable interest.

Dr. Matthiessen's researches are distinguished as well for their diversity as for their uniformly complete and trustworthy character.

Dr. Matthiessen,

I have great pleasure in presenting you with this Medal, which you will receive as a mark of the value which the Royal Society attaches to your researches, and the interest with which it regards your continuation of them.

Before concluding, I have to acquaint you that the Society will in future years have an additional Medal to bestow. Dr. John Davy, brother of Sir Humphry Davy, has bequeathed to the Royal Society, in fulfilment of an expressed wish of his illustrious brother, a service of Plate, presented to Sir Humphry Davy for the invention of the Safety Lamp, to be employed in founding a Medal to be given annually for the most important discovery in Chemistry made in Europe or Anglo-America. The directions given in the will, respecting the manner in which the plate should be disposed of, have been fulfilled, and the proceeds invested in India securities, yielding a little more than £30 a year; and it now remains with your Council to determine the form of the Medal, and to specify the conditions under which it will be awarded.

On the motion of Capt. Richards, seconded by Mr. Abel, it was resolved,—"That the thanks of the Society be returned to the President for his Address, and that he be requested to allow it to be printed."

The Statutes relating to election of the Council and Officers having been read, and Mr. C. V. Walker and Dr. Webster having been, with the consent of the Society, nominated Scrutators, the votes of the Fellows

present were collected, and the following were declared duly elected as Council and Officers for the ensuing year:—

President.—Lieut.-General Sir Edward Sabine, R.A., K.C.B., D.C.L., LL.D.

Treasurer.—William Allen Miller, M.D., D.C.L., LL.D.

Secretaries.— { Willim Sharpey, M.D., LL.D.
George Gabriel Stokes, Esq., M.A., D.C.L., LL.D.

Foreign Secretary.—Professor William Hallows Miller, M.A., LL.D.

Other Members of the Council.—Frederick Currey, Esq., M.A.; Warren De La Rue, Esq., Ph.D.; Sir Philip de M. Grey Egerton, Bart.; William Henry Flower, Esq.; William Huggins, Esq.; John Gwyn Jeffreys, Esq.; John Marshall, Esq.; Augustus Matthiessen, Esq., Ph.D.; George Henry Richards, Capt. R.N.; The Marquis of Salisbury, M.A.; Charles William Siemens, Esq.; John Simon, Esq.; Archibald Smith, Esq., M.A.; Prof. Henry J. Stephen Smith, M.A.; Prof. John Tyndall, LL.D.; Prof. Alexander W. Williamson, Ph.D.

The thanks of the Society were voted to the Scrutators.

The following Table shows the progress and present state of the Society with respect to the number of Fellows:—

	Patron and Royal.	Foreign.	Compounders.	£2 12s. yearly.	£4 yearly.	Total.
November 30, 1868.	4	48	289	2	257	600
Since elected		+3	+5		+12	+20
Since re-admitted ..					+2	+2
Since compounded..			+3		−3	
Since deceased		−2	−13	−2	−8	−25
November 30, 1869.	4	49	284		260	597

Receipts and Payments of the Royal Society between December 1, 1868, and November 30, 1869.

	£	s.	d.
Balance at Bank and on hand	493	14	6
Annual Subscriptions, Admission Fees, and Compositions	1618	4	0
Rents	252	5	7
Dividends	1476	10	5
Ditto, Trust Funds	281	4	3
Oliveira Bequest (subject to Duty)	1506	17	1
Davy Bequest	736	8	5
Sale of Transactions, Proceedings, &c.	383	19	9
Repayments	4	2	3
	£6753	6	3

	£	s.	d.	£	s.	d.
Salaries, Wages, and Pension				1084	13	0
The Scientific Catalogue				406	7	6
Oliveira Bequest, Deposit				1506	17	1
Books for the Library and Binding				242	7	4
Printing Transactions and Proceedings, Paper, Binding, Engraving, and Lithography				1681	4	8
General Expenses (as per Table subjoined)				358	10	0
Rumford Medal Fund	136	9	4			
Davy Medal Fund, Investment, &c.	728	17	3			
Donation Fund	17	15	0			
Wintringham Fund	35	2	0	932	16	2
Copley Medal Fund	4	15	7			
Dr. Andrews, Bakerian Lecture	4	0	0			
Rev. T. S. Evans, Fairchild Lecture	2	18	6			
Croonian Lecture, Poor of St. James's Parish	2	18	6			
Law Expenses				65	12	2
Legacy Duty, Oliveira Bequest				150	13	9
				6429	1	8
Balance at Bank				298	18	5
Balance of Catalogue Account				22	12	3
„ Petty Cash Account				2	13	11
				£6753	6	3

WILLIAM ALLEN MILLER,
Treasurer.

Estates and Property of the Royal Society, including Trust Funds.

Estate at Mablethorpe, Lincolnshire (55 A. 2 R. 2 P.), £126 per annum.
Estate at Acton, Middlesex (34 A. 2 R. 27¼ P.), £109 10s. per annum.
Fee Farm near Lewes, Sussex, rent £19 4s. per annum.
One-fifth of the clear rent of an estate at Lambeth Hill, from the College of Physicians, £3 per annum.
£14,000 Reduced 3 per Cent. Annuities.
£29,569 15s. 7d. Consolidated Bank Annuities.
£513 9s. 8d. New 2½ per Cent. Stock—Bakerian and Copley Medal Fund.
£660 Madras Guaranteed 5 per Cent. Railway Stock—Davy Medal Fund.

[illegible] Fund.

Investments up to July 1865, New 3 per Cent. Annuities £6052 17 8

£6052 17 8

Dr.	£	s.	d.
Balance	379	7	0
Dividends	177	8	7
	£556	15	7

Cr.	£	s.	d.
By Grants	175	0	0
Balance	381	15	7
	£556	15	7

Statement of Income and Expenditure (apart from Trust Funds) during the Year ending November 30, 1869.

	£	s.	d.
Annual Subscriptions	1072	4	0
Admission Fees	170	0	0
Compositions	376	0	0
Rents	252	5	7
Dividends on Stock (exclusive of Trust Funds)	1008	9	2
" on Stevenson Bequest	468	1	3
Sale of Transactions, Proceedings, &c.	383	19	9
Oliveira Bequest	1506	17	1
Repayments	4	2	3
Income available for the Year ending Nov. 30, 1869	5241	19	1
Expenditure in the Year ending Nov. 30, 1869	5496	5	6
Excess of Expenditure over Income in the Year ending Nov. 30, 1869	£254	6	5

	£	s.	d.	£	s.	d.
Salaries, Wages, and Pension				1084	13	0
The Scientific Catalogue				406	7	6
Oliveira Bequest, Deposit				1506	17	1
Books for the Library				152	4	10
Binding ditto				90	2	6
Printing Transactions, Part II. 1868, and Part I. 1869	467	16	0			
Ditto Proceedings, Nos. 105–114	430	17	7			
Ditto Miscellaneous	62	4	9	1681	4	8
Paper for Transactions and Proceedings	208	6	6			
Binding and Stitching ditto	70	9	0			
Engraving and Lithography	441	10	10			
Law Expenses				65	12	2
Legacy duty, Oliveira Bequest				150	13	9
Fittings, Cleaning, and Repairs	82	18	4			
Miscellaneous Expenses	34	7	3			
Coal, Lighting, &c.	104	16	4			
Tea Expenses	28	11	2			
Fire Insurance	28	11	6	358	10	0
Stationery, &c.	15	15	4			
Taxes	13	4	8			
Advertising	14	7	6			
Postage, Parcels, and Petty Charges	33	15	11			
Mablethorpe Schools, Donation	2	2	0			
				£5496	5	6

WILLIAM ALLEN MILLER,
Treasurer.

December 9, 1869.

Dr. WILLIAM ALLEN MILLER, Treasurer and Vice-President, in the Chair.

It was announced from the Chair that the President had appointed the following Members of Council to be Vice-Presidents:—

The Treasurer.
Mr. De la Rue.
Sir Philip Egerton.
Capt. Richards.
Mr. Archibald Smith.

Dr. W. W. Gull was admitted into the Society.

The Presents received were laid on the Table, and thanks ordered for them, as follows:—

Transactions.

Albany:—State of New York. Fiftieth and Fifty-first Annual Reports of the Trustees of the State Library. Twentieth Annual Report of the Regents of the University on the Condition of the State Cabinet of Natural History. Report of Dr. Peters on the Longitude of the Western Boundary Line of the State of New York. 8vo. *New York* 1868–69. The Regents of the University.

Boston:—American Academy of Arts and Sciences. Proceedings. Vol. VII. Sheets 44–66. 8vo. *Boston* 1868. The Academy.

Boston Society of Natural History. Proceedings. Vol. XII. Sheets 1–17. Occasional Papers, I. (Entomological Correspondence of T. W. Harris.) 8vo. *Boston* 1868–69. The Society.

Breslau:—Schlesische Gesellschaft für vaterländische Cultur. Abhandlungen. Phil.-Hist. Abth. 1868. Heft II. 1869. Abth. für Naturwissenschaften und Medicin, 1868–69. Sechsundvierzigster Jahresbericht. 8vo. *Breslau* 1869. The Society.

Cambridge, Mass.:—American Association for the Advancement of Science. Proceedings. Sixteenth Meeting held at Burlington, Vermont, August 1867. 8vo. *Cambridge* 1868. The Association.

Harvard College. Forty-Second Annual Report of the President for the year 1867–68. Treasurer's Statement, 1868. Report of the Board of Overseers, 1869. A Catalogue of the Officers and Students for the year 1868–69. The New Catalogue of Harvard College Library, and other papers. 8vo. The College.

Museum of Comparative Zoology. Annual Report of the Trustees, 1868. Bulletin, pp. 121–142. 8vo. *Boston and Cambridge* 1868–69. The Museum.

Carácas:—Sociedad de Ciencias Fisicas y Naturales. Vargasia: Boletin

Transactions (*continued*).

de la Sociedad. Num. 1–4. El Lago de Asfalto en la Isla de Trinidad, por A. Rojas. La Sumergida Isla de Atlantis, por el Dr. F. Unger, traducido por G. A. Ernst. Rede gehalten am Abend der Vorfeier des Humboldt-Festes 13. Sept. 1869 in der Ruine von Saboura Grande, von A. Ernst. 8vo. *Carácas* 1867–69. The Society.

London:—Geological Survey of Great Britain. Memoirs. Reports on the Geology of Jamaica, by J. S. Sawkins. The Geology of the Carboniferous Limestone, Yoredale Rocks, and Millstone Grit of North Derbyshire and the adjoining parts of Yorkshire. The Triassic and Permian Rocks of the Midland Counties of England, by E. Hull. The Geology of part of the Yorkshire Coal-field. Mineral Statistics for 1868, by R. Hunt. Catalogue of the published Maps, Sections, Memoirs, and other Publications. 8vo. *London* 1869. The Survey.

Royal Medical and Chirurgical Society. Medico Chirurgical Transactions. Vol. LII. 8vo. *London* 1869. The Society.

New York. Lyceum of Natural History. Annals. Vol. IX. Nos. 1–4. 8vo. *New York* 1868. The Lyceum.

United States Sanitary Commission. A Sketch of its purposes and its work. 12mo. *Boston* 1863. A Succinct Narrative of its works and purposes. 8vo. *New York* 1864. History of the Commission, by C. J. Stillé. 8vo. *New York* 1868. Memoirs, Statistical. 8vo. *New York* 1869. History of the Brooklyn and Long Island Fair, Feb. 22, 1864. 8vo. *Brooklyn* 1864. Memoir of the Great Central Fair held at Philadelphia, June 1864, by C. J. Stillé. 4to. *Philadelphia* 1864. A Record of the Metropolitan Fair held at New York in April 1864. 4to. *New York* 1867. The Commission.

Philadelphia:—Forty-Ninth Annual Report of the Board of Controllers of Public Schools of the First School District of Penn'a for the year ending Dec. 31, 1867. 8vo. *Philadelphia* 1868. The Board.

American Philosophical Society. Proceedings. Vol. X. Nos. 78–80. 8vo. *Philadelphia* 1867–68. The Society.

Pisa:—Memorie Valdarnesi. Vol. I.–IV. 8vo. *Pisa* 1835–55. C. Falconer, Esq.

San Francisco:—California Academy of Sciences. Proceedings. Vol. IV. Part 1. 8vo. *San Francisco* 1869. The Academy.

Toulouse:—Académie Impériale des Sciences, Inscriptions et Belles-Lettres. Mémoires. 7e série, Tome I. 8vo. *Toulouse* 1869. The Academy.

Washington:—National Academy of Sciences. Letters of the President and Vice-President, 1867–68. 8vo. *Washington.* The Academy.

Smithsonian Institution. Annual Report of the Board of Regents for the year 1867. 8vo. *Washington* 1868. The Institution.

Chase (P. E.) Some Remarks on the Fall of Rain, as affected by the Moon. On some General Connotations of Magnetism. 8vo. *Philadelphia* 1868. The Author.

Hugueny (F.) Le Coup de Foudre de l'Ile du Rhin près de Strasbourg (13 Juillet, 1869). 4to. *Strasbourg* 1869. The Author.

Lea (Isaac) Descriptions of Twelve New Species of *Unionidæ* from South America. Notes on some Members of the Feldspar Family &c. 8vo. *Philadelphia* 1868. The Author.

Rees (G. Owen, F.R.S.) The Harveian Oration delivered at the Royal College of Physicians, June 26, 1869. 12mo. *London* 1869. The Author.

Smiles (R.) Memoir of the late Henry Booth, of the Liverpool and Manchester, and afterwards of the London and North-Western Railway. 8vo. *London* 1869. Miss Booth.

Tuson (R. V.) A Pharmacopœia, including the Outlines of Materia Medica and Therapeutics, for the use of Practitioners and Students of Veterinary Medicine. 12mo. *London* 1869. The Author.

The following communications were read :—

I. "Spectroscopic Observations of the Sun."—No. V. By J. Norman Lockyer, F.R.S. Received July 8, 1869. (See p. 74.)

II. "Researches on Gaseous Spectra in relation to the Physical Constitution of the Sun, Stars, and Nebulæ."—Third Note. By E. Frankland, F.R.S., and J. Norman Lockyer, F.R.S. Received July 14, 1869. (See p. 79.)

III. "On the successive Action of Sòdium and Iodide of Ethyl on Acetic Ether." By J. Alfred Wanklyn, F.C.S. &c. Communicated by Professor Williamson. Received July 16, 1869. (See p. 91.)

IV. "On Linear Differential Equations." By W. H. L. Russell, F.R.S. Received November 13, 1869.

The condition that the linear differential equation

$$(\alpha+\beta x+\gamma x^2)\frac{d^2u}{dx^2}+(\alpha'+\beta' x+\gamma' x^2)\frac{du}{dx}+(\alpha''+\beta'' x+\gamma'' x^2)u=0$$

admits of an integral $u=\epsilon^{\int\phi dx}$, where ϕ is a rational function of (x), is given by the system of equations

$$\left\| \begin{array}{cccccccc} \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot \\ 0\ldots P_{r-1} & Q_{r-1} & R_{r-1} & S_{r-1} & 0 & 0 & \ldots & 0 \\ 0\ldots 0 & P_r & Q_r & R_r & S_r & 0 & \ldots & 0 \\ 0\ldots 0 & 0 & P_{r+1} & Q_{r+1} & R_{r+1} & S_{r+1} & \ldots & 0 \\ \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot \end{array} \right\| =0$$

where P_r, Q_r, R_r, S_r are given as follows.

When α, β, γ are none of them equal to zero, and

$$\rho^2\gamma-\rho\gamma'+\gamma''=0,$$

$$P_r=\rho^2\beta-\{2\gamma(r-1)+\beta'\}\rho+(r-1)\gamma'+\beta',$$

$$Q_r=\rho^2\alpha-(2\beta r+\alpha')\rho+\gamma r(r-1)+\beta' r+\alpha'',$$

$$R_r=(r+1)(-2\rho\alpha+\beta r+\alpha'),$$

$$S_r=\alpha(r+1)(r+2).$$

There will be $(n+2)$ horizontal and $(n+1)$ vertical rows, where n is the index of the highest power of x in the denominator of ϕ.

When α and β are not zero but $\gamma=0$, and we put

$$\mu=\frac{\beta'}{\beta}-\frac{\gamma''}{\gamma'}-\frac{\alpha\gamma'}{\beta^2},\quad \nu=\frac{\gamma'}{\beta},$$

then

$$P_r=\beta\mu^2+2\alpha\mu\nu-\beta'\mu-(r\beta+\alpha')\nu+\beta'',$$

$$Q_r=\alpha\mu^2-(2\beta r+\alpha')\mu-(2r+1)\alpha\nu+\alpha''+r\beta',$$

$$R_r=(r+1)(\beta r-2\alpha\mu+\alpha'),$$

$$S_r=\alpha(r+1)(r+2).$$

When α is not zero, but $\beta=\gamma=0$, and we put

$$\mu=\frac{\alpha'}{\alpha}\quad\frac{\gamma''}{\gamma'},\quad \nu=\frac{\beta'}{\alpha},\quad \rho=\frac{\gamma'}{\alpha},$$

$$P_r=\alpha\mu\nu-\alpha'\nu-\alpha(r+1)\rho+\beta'',$$

$$Q_r=\alpha\mu^2-\alpha'\mu-\nu\alpha(r+1)+\alpha'',$$

$$R_r=(r+1)(\alpha'-2\alpha\mu),$$

$$S_r=\alpha(r+1)(r+2).$$

Similar methods will apply to the linear differential equation

$$(\alpha+\beta x+\gamma x^2)\frac{d^3u}{dx^3}+(\alpha'+\beta' x+\gamma' x^2)\frac{d^2u}{dx^2}+(\alpha''+\beta'' x+\gamma'' x^2)\frac{du}{dx}$$
$$+(\alpha'''+\beta''' x+\gamma''' x^2)\alpha=0,$$

and the process admits of a very remarkable simplification. All linear differential equations of the second and third orders may be treated in the same way, and, I believe, all linear differential equations of every degree*.

V. "Spectroscopic Observations of the Solar Prominences, being Extracts from a Letter addressed to Sir J. F. W. Herschel, Bart., F.R.S., by Captain Herschel, R.E., dated 'Bangalore, June 12th and 15th, 1869.'" Communicated by Sir J. Herschel. Received July 19, 1869. (See p. 62.)

* This investigation assumes that $\alpha+\beta x+\gamma x^2$ and the denominator of ϕ have no common factor.—W. H. L. R., Jan. 13, 1869.

December 16, 1869.

Lieut.-General Sir EDWARD SABINE, K.C.B., President, in the Chair.

Mr. Charles Chambers was admitted into the Society.

Pursuant to notice given at the last Meeting, General Sir Andrew Waugh proposed, and General Boileau seconded the Right Honourable Lord Napier of Magdala for election and immediate ballot.

The ballot having been taken, Lord Napier was declared duly elected.

The Presents received were laid on the Table, and thanks ordered for them, as follows:—

Transactions.

Bordeaux:—Société Médico-Chirurgicale des Hopitaux et Hospices. Mémoires et Bulletins. Tome III. fasc. 2. 8vo. *Bordeaux* 1868. The Society.

Calcutta:—Asiatic Society of Bengal. Journal, 1869. Part 2. No. 2. Proceedings, 1869. Nos. 2, 3, 5, 8. 8vo. *Calcutta* 1869. The Society.

Cambridge:—Philosophical Society. Proceedings. Nos. 1–2. 8vo. *Cambridge* 1866. The Society.

Innsbruck:—Ferdinandeum für Tirol und Vorarlberg. Zeitschrift. Dritte Folge. Heft 14. 8vo. *Innsbruck* 1869. The Institution.

Jena:—Medicinisch-Naturwissenschaftliche Gesellschaft. Jenaische Zeitschrift für Medicin und Naturwissenschaft. Band V. Heft 1–2. 8vo. *Leipzig* 1869. The Society.

Leipzig:—Resultate aus den Meteorologischen Beobachtungen angestellt an den fünfundwanzig Königl. Sächsischen Stationen im Jahre 1867 ... von C. Bruhns. Jahrgang IV. 4to. *Leipzig* 1869. The Observatory.

London:—Mathematical Society. Proceedings. Nos. 16–19. 8vo. *London* 1868–69. The Society.

Royal Institution of Great Britain. Proceedings. Vol. V. Part 6. No. 50. List of the Members, with the Report of the Visitors. 8vo. *London* 1869. The Institution.

Royal Geographical Society. Proceedings. Vol. XIII. No. 5. 8vo. *London* 1869. The Society.

Modena:—Società dei Naturalisti. Annuario. Anno 4. 8vo. *Modena* 1869. The Society.

Prag:—K. K. Sternwarte. Magnetische und Meteorologische Beobachtungen ... im Jahre 1868 von K. Hornstein und A. Murmann. Jahrgang 29. 4to. *Prag* 1869. The Observatory.

Vienna:—K. K. Central-Anstalt für Meteorologie und Erdmagnetismus.

Transactions (*continued*).

Jahrbücher von C. Jelinek und C. Fritsch. Neue Folge. Band III. Jahrgang 1866. 4to. *Wien* 1868. The Institution.

K. K. Geographische Gesellschaft. Mittheilungen. Jahrgang 10. Neue Folge. Band II. 8vo. *Wien* 1868–69. The Society.

Baratta (G.) Studio Geometrico sulla Variazione e paragone degli Angoli fondato su nuovi teoremi e problemi che ne derivano. 8vo. *Napoli* 1869. The Author.

Battye (R. F.) Upon certain important Matters in relation to the Areas of Circles and Squares, a Geometrical Brochure. 8vo. *London* 1869. The Author.

Dircks (H.) The Policy of a Patent Law. 8vo. *London* 1869. The Author.

Dove (H. W., For. Mem. R. S.) Nicht periodische Veränderungen der Verbreitung der Wärme auf der Erdoberfläche. 8vo. *Berlin* 1869. The Author.

Favre (E.) Description des Mollusques Fossiles de la Craie de Lemberg en Galicie. 4to. *Genève* 1869. The Author.

Francesco (B.) Catalogo dei Fossili Miocenici e Pliocenici del Modenese. 8vo. *Modena* 1869. L'Uomo fatto ad imagine di Dio fu anche fatto ad imagine della Scimia. 8vo. *Cagliari* 1869. The Author.

Günther (R.) Die Indische Cholera im Regierungsbezirke Zwickau im Jahre 1866. 4to. *Leipzig* 1869. The Author.

Huguet (H. A. B.) Exposé de Médecine Homœodynamique basée sur la loi de similitude fonctionnelle et appliquée au traitement des Affections aiguës et Chroniques. 12mo. *Paris* 1869. The Author.

Lawes (J. B., F.R.S.) and Gilbert (J. H., F.R.S.) On the Home Produce, Imports, and Consumption of Wheat. 8vo. *London* 1868. The Authors.

Linder (M.) Note sur les Variations Séculaires du Magnétisme Terrestre. 8vo. *Bordeaux* 1869. The Author.

Mühry (A.) Ueber die Lehre von den Meeresströmungen. 8vo. *Göttingen* 1869. Untersuchungen über die Theorie und das allgemeine geographische System der Winde. 8vo. *Göttingen* 1869. The Author.

Pictet (F. J.) Mémoire sur les Animaux Vertébrés trouvés dans le Terrain Sidérolithique du Canton de Vaud; Supplément. 4to. *Genève* 1869. The Author.

Reade (Rev. J. B., F.R.S.) The Diatom Prism and the true form of Diatom Markings, and the Microscope Prism and the Structure of the Podura Scale. 8vo. *London* 1869. The Author.

Sylvester (J. J., F.R.S.) Outline Trace of the Theory of Reducible Cyclodes. 8vo. *London* 1869. The Author.

Williamson (A. W., F.R.S.) On the Atomic Theory. 8vo. *London* 1869. The Author.

Du Mouvement Politique en France depuis 1789 jusqu'à nos jours. 8vo. *Toulon* 1869. The Author.

The Mortality Experience of Life Assurance Companies, collected by the Institute of Actuaries. 8vo. *London* 1869. The Institute.

The New System of Astronomy; or, is the Earth a Fixed Star or Planet? 8vo. *London* 1869. The Author.

The True Theory of the Earth, and Philosophy of the Predicted End. 8vo. *Edinburgh* 1869. The Author.

The following communications were read:—

I. "Researches into the Constitution of the Opium Bases.—Part III. On the Action of Hydrochloric Acid on Codeia." By AUGUSTUS MATTHIESSEN, F.R.S., Lecturer on Chemistry in St. Bartholomew's Hospital, and C. R. A. WRIGHT, B.Sc. Received July 23, 1869. (See p. 83.)

II. "On the Thermodynamic Theory of Waves of Finite Longitudinal Disturbance:" and Supplement. By W. J. MACQUORN RANKINE, C.E., LL.D., F.R.SS. Lond. & Edinb. Received August 13, 1869. (See p. 80.)

III. "On Abstract Geometry." By Professor CAYLEY. Received October 14, 1869.

(Abstract.)

I submit to the Society the present exposition of some of the elementary principles of an Abstract m-dimensional geometry. The science presents itself in two ways,—as a legitimate extension of the ordinary two- and three-dimensional geometries; and as a need in these geometries and in analysis generally. In fact whenever we are concerned with quantities connected together in any manner, and which are, or are considered as variable or determinable, then the nature of the relation between the quantities is frequently rendered more intelligible by regarding them (if only two or three in number) as the coordinates of a point in a plane or in space; for more than three quantities there is, from the greater complexity of the case, the greater need of such a representation; but this can only be obtained by means of the notion of a space of the proper dimensionality; and to use such representation, we require the geometry of such space. An important instance in plane geometry has actually presented itself in the question of the determination of the curves which satisfy given conditions: the conditions imply relations between the coefficients in the equation of the curve; and for the better understanding of these relations it was expedient to consider the coefficients as the coordinates of a point in a space of the proper dimensionality.

A fundamental notion in the general theory presents itself, slightly in

plane geometry, but already very prominently in solid geometry; viz. we have here the difficulty as to the form of the equations of a curve in space, or (to speak more accurately) as to the expression by means of equations of the twofold relation between the coordinates of a point of such curve. The notion in question is that of a k-fold relation,—as distinguished from any system of equations (or onefold relations) serving for the expression of it,—and giving rise to the problem how to express such relation by means of a system of equations (or onefold relations). Applying to the case of solid geometry my conclusion in the general theory, it may be mentioned that I regard the twofold relation of a curve in space as being completely and precisely expressed by means of a system of equations ($P=0,\ Q=0,\ ..\ T=0$), when no one of the func ions $P,\ Q,\ \ldots\ T$, as a linear function, with constant or variable *integral* coefficients, of the others of them, and when *every surface whatever* which passes through the curve has its equation expressible in the form $U\doteq AP+BQ\ldots+KT$, with constant or variable integral coefficients, $A,\ B\ \ldots\ K$. It is hardly necessary to remark that all the functions and coefficients are taken to be rational functions of the coordinates, and that the word integral has reference to the coordinates.

IV. "On the Action of Bromine upon Ethylbenzol." By T. E. Thorpe, Ph.D. Communicated by H. E. Roscoe, Ph.D. Received November 11, 1869.

In the course of an investigation upon ethylbenzoic acid which Prof. Kekulé and I recently published in conjunction, we had occasion to prepare a quantity of monobromethylbenzol, $C_6H_4Br\{C_2H_5$. Our object in this research was to prove experimentally the identity of the ethylbenzoic acid made synthetically by acting upon the monobromethylbenzol by means of carbonic anhydride and sodium,

$$C_6H_4Br\{C_2H_5+Na_2+CO_2=C_6H_4\left\{\begin{matrix}C_2H_5\\ CO\,NaO\end{matrix}\right.+Br\,Na,$$

with the acid subsequently obtained by Fittig by oxidizing diethylbenzol, $C_6H_4\left\{\begin{matrix}C_2H_5\\ C_2H_5\end{matrix}\right.$, by means of nitric acid.

In the preparation of the bromide for the purposes of our experiments, we followed the direction given by Fittig and König, by whom this substance was first described. Bromine was added drop by drop to well-cooled ethylbenzol in the proportion of 1 mol. bromine to 1 mol. ethylbenzol, and the mixture was allowed to stand one or two days before distillation. The action of bromine upon ethylbenzol is extremely energetic, each drop of the bromine disappears almost immediately on coming in contact with the hydrocarbon, the mixture, unless carefully cooled, becomes very hot, and large quantities of hydrobromic acid are evolved. It is easy to perceive when the proper point in the substitution is reached, since after the addition of the theoretical quantity of bromine in order to form $C_6H_4Br\{C_2H_5$,

the succeeding drops of bromine disappear with far less rapidity, a fact which evidently indicates that the substitution of the first atom of bromine is more easily effected than that of the second. After standing for about forty-eight hours, the liquid was shaken with a dilute solution of caustic soda, and then repeatedly washed with water, dried over calcium chloride, and submitted to fractional distillation.

The liquid commenced to boil at about 145°, and a comparatively large quantity passed over between 150° and 160°; a still larger fraction distilled over between 170° and 180°; but the greater portion came over between 180° and 190°, after which the temperature rapidly rose, and the residue in the flask became nearly solid, owing to the formation of a mixture of metastyrol and styrolbromide. Throughout the process of distillation large quantities of hydrobromic acid were evolved, and the portion boiling between 150° and 160° was found by the characteristic bromine reaction to consist mainly of styrol,

$$C_8H_9Br = C_8H_8 + HBr.$$

An analysis of the portion boiling between 180°–190° showed it to contain very nearly the theoretical amount of bromine calculated for

$$C_8H_9Br.$$

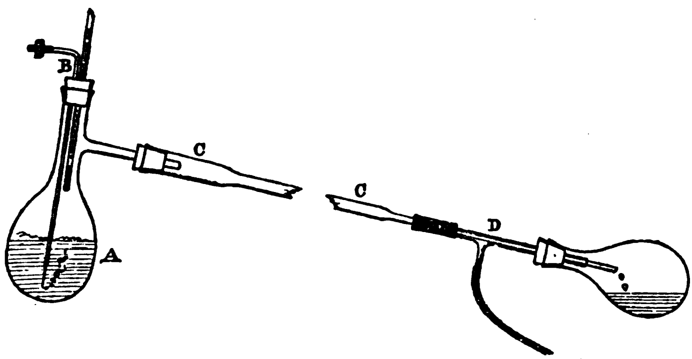
0·9742 grm. substance gave 0·9510 grm. silver bromide, and 0·0082 grm. silver

	Found.	Calculated.
Bromine	42·1 %.	43·2 %.

This compound is very unstable; on renewed distillation it invariably commences to boil at about 140°, and unless the distillation is very rapidly conducted, a large proportion is transformed into styrol and hydrobromic acid. So easily is this decomposition effected, that on exposing 4 or 5 grms. of the liquid in a sealed tube to a temperature of about 200° for a few minutes, it is almost entirely converted into metastyrol, and on opening the tube torrents of hydrobromic acid are evolved. The formation of styrol may, however, be almost entirely avoided by conducting the distillation in a partial vacuum; and for this purpose the water-pump of Bunsen may be very conveniently applied. The accompanying figure shows the disposition of the apparatus employed for this purpose; as it may hereafter be found useful in operations of a like nature, the following description of its arrangement may not be superfluous:—The apparatus may easily be adapted to the process of fractional distillation *in vacuo*; by a slight modification it is possible to change the receiver without disturbing the partial vacuum in the remaining parts of the apparatus.

A represents the flask from which the liquid is distilled; it is fitted with a good cork pierced with two holes, into one of which fits a thermometer, and into the other a piece of thermometer tubing (B), one end of which nearly reaches to the bottom of the flask, and is drawn out into a fine capillary tube; to the other end a screw-clamp is fixed by means of a short piece of caoutchouc tubing. The object of this capillary tube is to

deliver a minute stream of air-bubbles, and thus to prevent the violent "bumping" which almost invariably occurs during ebullition *in vacuo*; the supply of air may be regulated at will by increasing or diminishing the



pressure of the screw-clamp on the caoutchouc tubing. This little device succeeds admirably; it is due to my friend Mr. W. Dittmar, who has already applied it in the distillation of sulphuric acid: so rapidly and effectually does the "water-pump" of Bunsen exhaust, that the minute amount of air passing through the liquid, and serving to maintain it in regular ebullition, is without appreciable effect upon the manometer. The flask is attached, as represented in the figure, to a long tube C, made preferably of thin glass, so as to allow the condensation and cooling to take place as rapidly as possible: over the end of the condennser slides a short length of wider glass tubing, which is fastened air-tight on to the condenser by an inch or two of caoutchouc tubing; the other end fits into a caoutchouc cork adapted to the receivers; on this short piece of wider tubing another piece of glass tubing is fixed at right angles, in order to connect the entire apparatus with the caoutchouc tubing leading to the "water-pump." The mode of using the apparatus hardly requires description; it will be self-evident to anyone familiar to the working of the Bunsen pump.

Distilled in a partial vacuum (about 0·5 m.) in the apparatus above described, the bromide boiled nearly constantly at 148°–152°, and left scarcely any residue of styrolbromide or metastyrol. The bromide thus obtained is a heavy colourless liquid, possessing the characteristic penetrating odour peculiar to all the aromatic substitution products in which the substitution has occurred in the lateral group; its vapour is extremely irritating, and excites a copious flow of tears when incautiously inhaled. When heated with solution of ammonia or potash in alcohol, it gives up its bromine with the greatest facility.

The monobromethylbenzol, $C_6H_4Br\{C_2H_5$, described by Fittig, is a colourless aromatic-smelling liquid, possessing the sp. gr. 1·34, boiling constantly at 199° C., and capable of being distilled without decomposition. It may be boiled or heated in sealed tubes for any length of time with al-

coholic solution of potash or ammonia without giving up the least trace of bromine. It is evident, therefore, that the two bromides, although prepared under circumstances apparently exactly similar, are not identical. In all probability the bromide we obtained is identical with that recently prepared by Berthelot by acting upon boiling ethylbenzol with the vapour of bromine. To this compound the formula $C_6H_5\{C_2H_4Br$ is assigned; it cannot be distilled without a considerable portion undergoing decomposition into styrol and hydrobromic acid, and loses easily its bromine by double decomposition.

It remained now to discover the cause of the variation in the position of the bromine atom. In the preparation of the two products the conditions were apparently identical; why, then, should the substitution have occurred in the phenyl group in the bromide obtained by Fittig, and in the ethyl group in our own? The cause of the difference was soon found to reside in the bromine employed. The bromine used by Fittig doubtless contained iodine. By digesting a few grains of the bromine employed in our experiments with water and granulated zinc, and, on the complete disappearance of all colour, filtering the solution, adding a small quantity of chlorine-water, and then shaking the mixture with a few drops of benzol, the absence of even a trace of iodine was shown by the benzol remaining perfectly colourless. By adding about 0·5 % iodine to the bromine before allowing it to act upon the ethylbenzol, I easily succeeded in obtaining monobromethylbenzol with the properties described by Fittig. It distilled constantly without decomposition at 203° C., and completely resisted the action of boiling alcoholic potash. A similar series of comparative experiments on cymol obtained from camphor was attended with like results. We have thus a ready method of effecting at will the kind of substitution required without the employment of heat, the presence or absence of iodine determines the position of the substituting bromine; in the one case substitution occurs in the phenyl group, in the other in the lateral group. The bromine obtained by Berthelot is described as boiling between 200° and 210°, whilst the bromide which I obtained distilled at 190°. I am disposed, however, to consider the higher number to be a nearer approximation to the truth. The difference observed in my case is probably due to the fact that in distilling the substance I operated on a larger scale than did Berthelot, and kept the liquid exposed to the influence of the high temperature for a comparatively longer time, thus working under conditions more favourable to the production of styrol and hydrobromic acid, the formation of which would necessarily tend to lower the boiling-point.

Nearly seven years ago Dr. Hugo Müller drew attention to the remarkable effect of iodine in facilitating the action of chlorine upon organic compounds. He showed that many substances which were acted upon with great difficulty by chlorine alone, and some of them only with the aid of direct sunlight, yield chlorine products with great ease when acted upon by chlorine in the presence of iodine. He failed, however, to point out

any difference in the *kind* of substitution effected by the halogen in the presence or absence of iodine.

Beilstein also has recently shown that the action upon toluol varies materially with the conditions under which the experiment is performed. When a stream of chlorine is passed through carefully cooled toluol, chlortoluol, $C_6H_4Cl\{CH_3$, only is formed; this body is characterized by a high degree of stability, resisting completely the action of potassium cyanide, potassium sulphide, and silver-salts. On the other hand, if the toluol is previously heated, or if, through the energy of the action, its temperature be allowed to rise, the relative position of the substituting chlorine atom is essentially changed, and under these circumstances chlorbenzyl, $C_6H_5\{CH_2Cl$, is found to be the main product of the reaction: this substance differs from the isomeric chlortoluol by the facility with which it yields up its chlorine by double decomposition. But if a small quantity of iodine be added to the hydrocarbon before treatment with chlorine, chlortoluol only is produced, no matter whether the chlorine acts upon boiling or upon cold toluol.

To the bromide obtained by the action of bromine free from iodine on cold ethylbenzol I assign the formula $C_6H_5\{C_2H_4Br$, on the assumption that it is identical with that prepared by Berthelot. From the ease with which the compound yielded its bromine to alcoholic ammonia, I was induced to attempt the preparation of the corresponding amines. A quantity of the bromide was sealed up in wide glass tubes with about four times its volume of absolute alcohol saturated with ammoniacal gas, and the mixture exposed to a temperature of 100° C. for about three hours. When all action had apparently ended, the tubes were reopened and the liquid portion drained from the bulky precipitate of ammonium bromide. On treating the liquid with water, a light mobile agreeablysmelling liquid separated out; this was washed, dried by means of calcium chloride, and distilled; by far the greater portion boiled at 185°–187° C. This liquid was found to be free from nitrogen and bromine, and yielded on analysis the following numbers:—

I. 0·1552 grm. substance gave 0·4540 grm. carbonic anhydride and 0·1333 grm. water.

II. 0·2011 grm. substance gave 0·5896 grm. carbonic anhydride and 0·1078 grm. water.

	Calculated.		Found. I.	Found. II.
C_{10}	120	80·00	79·79	79·98
H_{14}	14	9·36	9·53	9·44
O	16	——	——	——

The constitution of this compound may be expressed by the formula

$$C_6H_5\{C_2H_4-O-C_2H_5,$$

and its formation from the bromide by the action of ammonium alcoholate may be thus represented :—

$$C_6H_5\{C_2H_4Br+\left.\begin{matrix}C_2H_5\\NH_4\end{matrix}\right\}O=C_6H_5\{C_2H_4-O-C_2H_5+NH_4Br.$$

For this substance I propose the name styrolylethyl ether. It is a colourless, mobile, fragrant-smelling liquid, boiling constantly at 187°, of specific gravity 0·9310 at 21°·9, slightly soluble in water, and burning with a strongly luminous flame.

When heated for a few hours with a concentrated solution of hydriodic acid in a sealed tube to about 120°, it was completely decomposed, and on distilling the liquid from a water-bath, a quantity of ethyl iodide, boiling at 73°–75°, and easily recognizable by its characteristic alliaceous odour, distilled over; the presence of the alcohol rest in the new ether was thus demonstrated. The remainder of the liquid, containing the greater portion of the hydriodic acid and possibly the alcohol or its corresponding iodide, was treated with dilute caustic soda, when a heavy oily liquid separated out. This liquid was repeatedly washed with water and dried over calcium chloride. On distilling it, the greater portion of the liquid boiled between 300° and 310°, but with evident decomposition, iodine being evolved. The compound in all probability was the iodide corresponding to the bromide originally taken, already described by Berthelot.

$$C_6H_5\{C_2H_4-O-C_2H_5+2HI=C_6H_5\{C_2H_4I+C_2H_5I+H_2O.$$

The very small quantity of substance at my disposal prevented me from more accurately investigating the nature of this reaction, or the properties of the iodine compound formed.

I next sought to obtain the alcohol, $C_8H_5\{C_2H_5O$, already described by Berthelot as a colourless liquid of an agreeable aromatic odour, heavier than water, and boiling at about 225°. I attempted to prepare the acetate, intending to decompose the compound with caustic potash. Fifty grms. of the bromide diluted with double its volume of absolute alcohol were heated with about 40 grms. potassium acetate to 100° in a flask placed in a water-bath. The liquid was then filtered from the mass of potassium bromide, and again sealed up in tubes with a further addition of acetate, and heated to 120°–130° for an hour or two. On cooling, the tubes were reopened and the contents treated with water, and the non-miscible portion separated and dried over calcium chloride. On standing over the calcium chloride, a crystalline precipitate was slowly formed, which was afterwards proved to be the compound of ethyl acetate and calcium chloride. On submitting the dehydrated liquid to distillation, a further quantity of ethyl acetate came over at 72°–74°. The next fraction, boiling between 140° and 150°, was found to consist chiefly of styrol; the quantity, however, was so small that in all probability it was not a product of the reaction, but existed already formed in the bromide used for the experiment. The next and main fraction of the distillate came over between 180° and 190°, and on repeated rectification a constant boiling-point of 185°–186° was obtained.

This substance, from its characteristic aromatic odour and from the following analyses, was identified as the styrolyl-ether already described.

0·2523 grm. substance gave 0·7358 grm. carbonic anhydride and 0·2119 water.

0·1993 grm. substance gave 0·5829 grm. carbonic anhydride and 0·1666 grm. water.

		Calculated.	Found. I.	Found. II.
C_{10}	120	80·00	79·54	79·75
H_{14}	14	9·36	9·33	9·30
O..........	16	——	——	——

The simultaneous production of this body and of the ethyl acetate may be thus represented :—

$$C_6H_5\{C_2H_4Br + \left.\begin{matrix}C_2H_3O\\K\end{matrix}\right\}O + 2\left(\left.\begin{matrix}C_2H_5\\H\end{matrix}\right\}O\right)$$

$$= C_6H_5\{C_2H_4{-}O{-}C_2H_5 + \left.\begin{matrix}C_2H_3O\\C_2H_5\end{matrix}\right\}O + KBr + H_2O.$$

The remainder of the distillate consisted principally of the acetic ether, $C_6H_5\{C_2H_4{-}O{-}C_2H_3O$. This body is a sweet-smelling liquid, possessing the characteristic fragrant odour of the acetic ethers and boiling at 217°–220°. The quantity produced, however, was so small as to preclude further investigation, or any attempt to prepare the alcohol.

Fittig has recently obtained phenylpropionic acid (hydrocinnamic acid), $C_6H_5\{CH_2{-}CH_2{-}COHO$, by acting upon chlorinated ethylbenzol by means of potassium cyanide and treating the resultant cyanide with potash.

$$C_6H_5\{CH_2{-}CH_2CN + 2H_2O = C_6H_5\{CH_2{-}CH_2{-}COHO + NH_3.$$

I have attempted to repeat this reaction with the bromide obtained by treating ethylbenzol with bromine free from iodine, but without success, although the experiment has been frequently made under the exact conditions described by Fittig. So easily, according to Fittig, is the transformation effected, that this chemist has recommended the reaction as affording the best method of obtaining phenylpropionic acid. I am unable to discover any reason for the discrepancy in the results of our observations. It is certainly very remarkable that the reaction should occur in the case of the chloride and not in that of the bromide.

The experiments which led to the discovery of the above method of effecting the substitution of hydrogen in the phenyl or in the lateral group at will had for their object the preparation by synthesis of ethylbenzoic acid from the bromide, $C_6H_4Br\{C_2H_5$. It therefore became interesting to trace the reaction which occurs on submitting the new bromide to a similar treatment. A small quantity (about 7 grms.) of the bromide was mixed with about five times its volume of anhydrous ether, and a large excess of sodium cut into slices as thin as possible added, and a slow continuous

[redacted]

current of carbonic anhydride sent through the mixture. Not the slightest reaction was perceptible in the cold. The sodium remained perfectly metallic-looking, even after the expiration of twenty-four hours. On gently heating the liquid for a few minutes, a vigorous reaction at once set in; the mass became suddenly very hot, and gave off abundant fumes of hydrobromic acid and a small quantity of a thick oily liquid boiling at a high temperature, and possessing the character of the distyrolyl,

$$\left.\begin{array}{l} C_6H_5 \,.\, C_2H_4 \\ | \\ C_6H_5 \,.\, C_2H_4 \end{array}\right\},$$

described by Berthelot was produced,

$$2C_6H_5\{C_2H_4Br + Na_2 = \begin{array}{l} C_6H_5 \\ | \\ C_6H_5 \end{array}\left\{\begin{array}{l} C_2H_4 \\ \\ C_2H_4 \end{array}\right. + 2BrNa.$$

The Society then adjourned over the Christmas Recess to Thursday, January 6th, 1870.

January 6, 1870.

Lieut.-General Sir EDWARD SABINE, K.C.B., President, in the Chair.

The Presents received were laid on the Table, and thanks ordered for them, as follows:—

Transactions.

Birmingham:—Institution of Mechanical Engineers. Proceedings, 1868, 28, 29 July. Leeds Meeting, Part 3, Nov. 5; 1869, April 29. 8vo. *Birmingham.* The Institution.

Cadiz:—Observatorio de Marina de la Ciudad de San Fernando. Almanaque Nautico para 1871. 8vo. *Cadiz,* 1869. The Observatory.

Cherbourg:—Société Impériale des Sciences Naturelles. Mémoires. Tome XIII. 8vo. *Paris* 1868. The Society.

Coimbra:—Observatorio da Universidade. Ephemeris Astronomicas para o anno de 1871. 8vo. *Coimbra* 1869. The Observatory.

Danzig:—Naturforschende Gesellschaft. Schriften, neue Folge. Band II. Heft II. 8vo. *Danzig* 1869. Two copies (one with Photographs). The Society.

Habana:—Colegio de Belen. Observaciones Magneticas y Meteorológicas. Año Meteorológico de 30 de Noviembre de 1867, a 30 de Noviembre de 1868. 8vo. *Habana* 1869. The College.

London:—British Museum. Hand-List of Genera and Species of Birds, by G. R. Gray. Part I. 8vo. *London* 1869. Catalogue of the Specimens of *Dermaptera Saltatoria,* by F. Walker. Part 2. 8vo. *London* 1869. Guides to the First and Second Vase Rooms. 12mo. *London*

Transactions (*continued*).

1869. Guide to the Printed Books. 12mo. *London* 1869. A Guide to the Slade Collection of Prints. 12mo. *London* 1869. The Museum.

The Nautical Almanac and Astronomical Ephemeris for 1873. 8vo. *London* 1869. The Admiralty.

Melbourne :—Williamstown Observatory. Astronomical Observations made in the years 1861, 1862, and 1863, under the direction of R. L. J. Ellery. 8vo. *Melbourne* 1869. The Observatory.

Moscow :—Société Impériale des Naturalistes. Bulletin, Année 1868, Nos. 3, 4. 8vo. *Moscou* 1869. The Society.

Paris :—Observatoire Impérial. Atlas Météorologique. Années 1867, 1868. fol. *Paris* 1869. Atlas des Mouvements généraux de l'Atmosphère. Année 1865, Janv.–Juin 1869. fol. *Paris* 1869. The Observatory.

Société Météorologique de France. Nouvelles Météorologiques. 1869, Nos. 7–12. roy. 8vo. *Paris* 1869. The Society.

Pulkowa :—Observations de Poulkova, publiées par Otto Struve. Vol. I., II. 4to. *St. Pétersbourg* 1869. Jahresbericht am 5. Juni 1869 dem Comité der Nicolai-Hauptsternwarte. 8vo. *St. Petersburg* 1869. Tabulæ Quantitatum Besselianarum pro annis 1750 ad 1840 computatæ. 8vo. *Petropoli* 1869. The Observatory.

St. Petersburg :—Observatoire Physique Central de Russie. Annales, publiées par H. Wild. 4to. *St. Pétersbourg* 1869. The Observatory.

Bertin (E.) Étude sur la Houle et le Roulis. 8vo. *Cherbourg* 1869. The Author.

Durdik (J.) Leibnitz und Newton. Ein Versuch über die Ursachen der Welt auf Grundlage der positiven Ergebnisse der Philosophie und der Naturforschung. 8vo. *Halle* 1869. The Author.

Kronecker (L.) Ueber Systeme von Functionen mehrer Variabeln. 8vo. *Berlin* 1869. The Author

Kudelka (J.) Die Gesetze der Lichtbrechung. 8vo. *Greifswald*. The Author.

Mueller (F. de, F.R.S.) Fragmenta Phytographiæ Australiæ. Vol. VI. 8vo. *Melbourne* 1867–68. The Author.

Plantamour (E.) Résumé Météorologique de l'Année 1868 pour Genève et le Grand St.-Bernard. 8vo. *Genève* 1869. The Author.

Turbiglio (S.) L'Empire de la Logique, Essai d'un nouveau système de Philosophie. 12mo. *Turin* 1870. The Author.

L'Album de la Fabrique. Troisième Année, 1869. fol. *Paris*. Messrs. L. Berger & Co.

The following communication was read :—

"Some Account of the Suez Canal, in a Letter to the President." By J. F. BATEMAN, Esq., F.R.S. Received January 3, 1870.

16 Great George Street, Westminster,
27th December, 1869.

MY DEAR SIR EDWARD,—On my return from the opening of the Suez Canal, where, by your kind selection, I had the honour of representing the Royal Society as the guest of the Viceroy, I think it incumbent on me to give a short account of my journey and my impressions of the great and important undertaking which was so magnificently inaugurated.

Nothing could exceed the splendid hospitality of the Viceroy, who had in every possible way provided for the accommodation and the comfort of his guests. The crowd of visitors, however, was so great, and his own personal attendance was so constantly given to the Empress of the French, the Emperor of Austria, and other Royal personages who honoured him with their company, that it was almost impossible for him to bestow any special attention on other individual guests; but few or none could complain of any want of attention or of any material inconvenience.

I was fortunate in being present at every important point and on every important occasion, and in receiving all the civilities which might be considered due to the representative of the Royal Society.

Of the work itself I have no hesitation in pronouncing it a decided success—not all that could be desired, nor all that was promised, and very far from being finished, even on the contracted scale on which it has been executed. A reef of rocks at Serapeum, extending for about 85 metres in length, at a depth of 16 or 17 feet below the surface of the canal, and which was not discovered till just before the period fixed for the opening, at present limits the draft of vessels which can use the navigation. There are some objectionable curves and narrow places, and many miles of unprotected slopes, all of which must be improved or remedied before the canal can be placed in a satisfactory condition. Still, in its present state, vessels drawing not more than 16 feet can pass from end to end with facility and safety; and when the rocks alluded to are removed, the depth will be increased to 21 or 22 feet.

For years before the commencement of the project which has been, so far, happily concluded, the practicability of forming and maintaining a maritime canal between the Red Sea and the Mediterranean was a much-disputed point among modern engineers. It was known that a water communication between the two seas had existed and been maintained for 600 years before, and for about 800 years after, the commencement of the Christian era; subsequent to which time it was allowed to fall into decay, and for a thousand years has so remained.

The first idea, in modern times, of restoring this ancient water commu-

nication, or of forming another more suitable to existing circumstances, seems to be due to the Emperor Napoleon I. (then General Bonaparte), who, at the close of the last century, during his occupation of Egypt, directed that a complete survey of the ancient canal should be made under the direction of M. Lepère, a French engineer of reputation. This survey was completed, and a project for a canal was designed in accordance with the apparent facts resulting from M. Lepère's survey. The evacuation of the country by the French put an end to further investigation, and arrested all progress in this direction for many years.

The conclusion at which M. Lepère arrived was, that the level of the Red Sea at high water at Suez was $30\frac{1}{2}$ feet higher than low water in the Mediterranean in Pelusium Bay; and his scheme was projected in accordance with the existence of such a difference in the level of the two seas. He also ascertained that the rise and fall of the tide in the Red Sea was $5\frac{1}{2}$ or 6 feet, and in the Mediterranean about 1 foot, leaving still a difference of 25 feet between the respective low waters of the two seas.

Doubts of the accuracy of the statement as to the difference of these levels were entertained by those who carefully considered the subject; but it was not till the year 1847 that these doubts were set at rest. In that year the late Mr. Robert Stephenson, in conjunction with M. Talabot, a French engineer, M. de Negrelli, an Austrian engineer, and Linant Bey, a French engineer in the Egyptian service, directed a series of independent levellings across the Isthmus, which determined beyond all doubt the important fact that "at low water there was no essential difference in the level of the two seas, and that at high water it was not more than 4 feet, the rise of tide being about 1 foot in the Mediterranean and about 6 feet in the Red Sea." Up to that time Mr. Stephenson seems to have been in favour of the proposal to form a canal across the Isthmus, in accordance with the views of Linant Bey, who "proposed to carry a canal from the Red Sea through the Bitter Lakes to Lake Timsah, and thence through the lagoons of Menzaleh to Tineh (Pelusium) on the Mediterranean:" it was "thus expected to create a current through the canal of three or four miles an hour;" and "the project appeared very feasible, and was calculated to excite high hopes of success." When, however, it was ascertained that the level of the two seas was practically the same, Mr. Stephenson remarked "it became evident that it would not be practicable to keep open a level cut or canal without any current between the two seas, and the project was abandoned."

The fact of there being no difference in the level of the two seas led other men to very different conclusions; for shortly after the period here referred to, M. Ferdinand Lesseps conceived the idea which has since been so successfully realized. His project was to cut a great canal on the level of the two seas by the nearest and most practicable route, which lay

along the valley or depression containing Lake Menzaleh, Lake Ballah, Lake Timsah, and the Bitter Lakes. The character of this route was well described in 1830 by General (then Captain) Chesney, R.A., who examined and drew up a report on the country between the Mediterranean and the Red Sea. At that time a difference of 30 feet between the two seas was still assumed, and all proposals for canals were laid out on that assumption. Allowance must, of course, be made for this error, in so far as it affected any particular project of canal; but it would not affect the accuracy of any general description of the district to be traversed. General Chesney summed up his report by stating, "as to the executive part, there is but one opinion: there are no serious difficulties; not a single mountain intervenes, scarcely what deserves to be called a hillock; and in a country where labour can be had without limit, and at a rate infinitely below that of any other part of the world, the expense would be a moderate one for a single nation, and scarcely worth dividing among the great kingdoms of Europe, who would all be benefited by the measure."

M. Lesseps was well advised therefore in the route he selected, and (assuming the possibility of keeping open the canal) in the character of the project he proposed.

From 1849 to 1854 he was occupied in maturing his project for a direct canalization of the Isthmus. In the latter year Mahomet Saïd Pasha became Viceroy of Egypt, and sent at once for M. Lesseps to consider with him the propriety of carrying out the work he had in view. The result of this interview was, that on the 30th of November in the same year a Commission was signed at Cairo charging M. Lesseps to constitute and direct a Company named "The Universal Suez Canal Company." In the following year, 1855, M. Lesseps, acting for the Viceroy, invited a number of gentlemen eminent as directors of public works, as engineers, and distinguished in other ways, to form an International Commission for the purpose of considering and reporting on the practicability of forming a ship canal between the Mediterranean and the Red Sea. This Commission, which included some of the ablest civil and military engineers of Europe, was honorary, and its members were considered as guests of the Viceroy.

The Commission met in Egypt in December 1855 and January 1856, and, accompanied by M. Lesseps, and by Mougel Bey and Linant Bey, engineers, and other gentlemen in the service of the Viceroy, they made a careful examination of the harbours in the two seas and of the intervening Desert, and arrived at the conclusion that a ship canal was practicable between the Gulf of Pelusium in the Mediterranean and the Red Sea near Suez. They differed, however, as to the mode in which such a canal should be constructed. The three English engineering members of the Commision were of opinion that a ship canal raised 25 feet *above* the sea-level, and communicating with the Bay of Pelusium at one end and

the Red Sea at the other, by means of locks, and supplied with water from the Nile, was the best mode of construction. The foreign members, on the contrary, held that a canal 27 feet *below* sea-level, from sea to sea, without any lock, and with harbours at each end, was the best system: the harbours to be formed by piers and dredging out to deep water.

The whole of the Members of the Commission, with the exception of Mr. Rendel, met at Paris in June 1856, when the views of the English engineers were, after full discussion, rejected, and the report to the Viceroy recommended the system which has since been carried out. The Commission estimated the work to cost £8,000,000.

Two years from the date of this report were spent in conferences and preliminary steps before M. Lesseps obtained the necessary funds for carrying out the works. About half the capital required was subscribed on the Continent, by far the larger portion being taken in France, and the other half was found by the Viceroy. Further time was necessarily lost in preparation, and it was not till near the close of 1860 that the work was actually commenced.

In this interval two "Reports on the subject of the Deposits of the Delta of the Nile" were made by Admiral (then Captain) T. Spratt, R.N., C.B., F.R.S., extracts from which were printed by order of the House of Commons in 1860. They embraced "An Enquiry into the Soundness of M. Lesseps's Reasonings and Arguments on the practicability of the Suez Canal," and "An Investigation of the effect of the prevailing Wave-influence on the Nile's Deposits, and upon the Littoral of its Delta." These documents were dated respectively 30th January and 9th July 1858.

The conclusion to which Captain Spratt arrived was adverse to M. Lesseps's project. He was of opinion that it would be next to impossible to keep open any harbour to the eastward of the mouths of the Nile; and he warned "the commercial interest against risking their millions in the undertaking." He contended that the material brought to the sea by the Nile, and which is carried eastwards by the prevailing winds and currents, would accumulate against the piers or jetties proposed to be carried out to deep water at Port Saïd, so rapidly and to such an extent as to prevent the maintenance of a sufficient harbour. He thought "the sands of the Nile would mount over the piers of Saïd," and he did not believe that any amount of dredging would overcome the difficulties.

It was against such opinions from high authority that M. Lesseps had to contend; but his confidence in his project and his courage and perseverance never failed him. As time went on, he had other difficulties ahead.

The original concession granted extraordinary privileges to the Company. It included or contemplated the formation of a "sweet-water" canal for the use of the workmen engaged; and the Company were to become proprietors of all the land which could be irrigated by means of this

canal. One of the conditions of the concession also was, that the Viceroy should procure forced labour for the execution of the work; and soon after the commencement of operations, and for some time, the number of workmen so engaged amounted to from 25,000 to 30,000. The work, thus commenced, steadily proceeded until 1862, when the late Viceroy, during his visit to this country at the time of the International Exhibition, requested Mr. Hawkshaw, F.R.S., to visit the canal and report on the condition of the works and the practicability of its being successfully completed and maintained. His Highness's instructions were, that Mr. Hawkshaw should make an examination of the works quite independently of the French Company and their engineers, and report, from his own personal examination and consideration, the result at which he arrived. If his report were favourable, the work would be proceeded with; if unfavourable, it would at once be stopped.

Mr. Hawkshaw proceeded to Egypt upon this important commission in November of the same year; and in February 1863 he wrote a well-considered report which may be said to have in great measure contributed to the rapid and successful completion of the work. Mr. Hawkshaw described the works of the canal which had been already executed, and those which remained at that time unfinished. He examined and discussed the dimensions of the various parts then in progress, recommending various alterations; and having carefully gone into all the details of construction, he proceeded to investigate the question of maintenance, with reference to which it had been urged by opponents:—

"1st. That the canal will become a stagnant ditch.

"2nd. That the canal will silt up, or that the moving sands of the Desert will fill it up.

"3rd. That the Bitter Lakes through which the canal is to pass will be filled up with salt.

"4th. That the navigation of the Red Sea is dangerous and difficult.

"5th. That shipping will not approach Port Saïd, because of the difficulties that will be met with, and the danger of that port on a lee-shore.

"6th. That it will be difficult, if not impracticable, to keep open the Mediterranean entrance to the canal."

Having analyzed each of these objections, and fully weighed the arguments on which they were based, he came to the following conclusions as to the practicability of construction and of maintenance:—

"1st. As regards the engineering construction, there are no works on the canal presenting on their face any unusual difficulty of execution, and there are no contingencies, that I can conceive, likely to arise that would introduce difficulties insurmountable by engineering skill.

"2ndly. As regards the maintenance of the canal, I am of opinion that no obstacles would be met with that would prevent the work, when completed,

being maintained with ease and efficiency, and without the necessity of incurring any extraordinary or unusual yearly expenditure."

The whole of Mr. Hawkshaw's report is well worthy of perusal; and I must congratulate him on the sound conclusions at which he arrived, and on the foresight by which he was enabled to point out difficulties and contingencies which have since arisen. Could he at that time have seen the full realization of the work, he would scarcely have altered the report he wrote.

Saïd Pasha died between the period of Mr. Hawkshaw's examination of the country and the date of his report. He was succeeded by his brother Ismaïl, the present Viceroy or Khedive, who, alarmed at the largeness and uncertainty of the grants to the Canal Company, of the proprietorship of land which could be irrigated by the Sweet-water Canal, and anxious to retire from the obligation of finding forced labour for the construction of the works, refused to ratify or agree to the concessions granted by his brother. The whole question was referred to the arbitration of the present Emperor of the French, who kindly undertook the task, and awarded the sum of £3,800,000 to be paid by the Viceroy to the Canal Company as indemnification for the loss they would sustain by the withdrawal of forced or native labour, for the retrocession of large grants of land, and for the abandonment of other privileges attached to the original Act of Concession. This money was applied to the prosecution of the works.

The withdrawal of native labour involved very important changes in the mode of conducting the works, and occasioned at the time considerable delay. Mechanical appliances for the removal of the material, and European skilled labour, had to be substituted; these had to be recruited from different parts of Europe, and great difficulty was experienced in procuring them. The accessory canals had to be widened for the conveyance of larger dredging-machines, and additional dwellings had to be provided for the accommodation of European labourers. All these difficulties were overcome, and the work proceeded.

Since the date of Mr. Hawkshaw's Report, viz. February 1863, much has been said and written upon the operations of the canal as they were going on, and upon its prospects of success. Sir William Denison, K.C.B., R.E., presented the Institution of Civil Engineers, in April 1867, with a paper on the condition of the works as he found them at the end of 1866, which led to an animated discussion upon the whole subject. The conclusions at which Sir William Denison himself arrived were:—

"1st. That (subject, of course, to the condition that the relative levels of the Red Sea and the Mediterranean are as stated by the French authorities) there will be no extraordinary difficulty in carrying an open salt-water channel from the Mediterranean to the Red Sea of the depth proposed, namely 8 metres.

"2nd. That no special difficulty in maintaining this channel need be anticipated.

"3rd. That it will be necessary to modify the section proposed by the French engineers, making the side slopes much more gradual.

"4th. That the cost of maintaining the above-mentioned depth of water will be found at first to be largely in excess of the amount estimated. Eventually, it is by no means impossible that means may be found to fix or check the drift of sand, or to shut it out from the canal. But for some years it must be expected that the ordinary action of the atmosphere, which has filled up former excavations made in this dry desert, will have the same effect on the new canal.

"Looking at the work as an engineer, there does not appear to be any difficulty which a skilful application of capital may not overcome."

In the discussion which followed, while on the one hand Sir William Denison's views were well supported, much was said, on the other hand, of the difficulties which would attend the construction, and the impossibility of keeping open the harbours and the canal. The old questions of silting up and stagnation were discussed; and quotations from the correspondence of Mr. R. Stephenson with M. de Negrelli were read, with the object of showing the absurdity of the whole scheme. In one of these quotations Mr. Stephenson thus expresses himself:—

"In conclusion, Sir, I will only say that I have—indeed I can have—no hostility to a maritime canal through the Isthmus of Suez. If I could regard such a canal as commercially advantageous, I have already shown that I should be the first to give it the advantage of my time, my money, and my experience. It was because, after elaborate investigation, and in conjunction with such men as M. Talabot, I arrived at a clear conclusion that the project was not one which deserved serious attention, that I refused to give it support. I should be delighted to see a channel like the Dardanelles or the Bosphorus penetrating the Isthmus that divides the Red Sea from the Mediterranean. But I know that such a channel is impracticable—that nothing can be effected even by the most unlimited expenditure of time and life and money beyond the formation of a stagnant ditch, between two almost tideless seas, unapproachable by large ships under any circumstances, and only capable of being used by small vessels when the prevalent winds permit their exit and their entrance. I believe that the project will prove abortive in itself and ruinous to its constructors; and entertaining that view, I will no longer permit it to be said that, by abstaining from expressing myself fully on the subject, I am tacitly allowing capitalists to throw away their money on what my knowledge assures me to be an unwise and unremunerative speculation."

It was shown also by calculations that the evaporation from the Bitter Lakes alone, without taking into consideration the long length of canal, was such that the channel from the Red Sea to the lakes was much too

small to supply the loss, and that the result would be that the water in these Lakes must settle to a level below the low water of a spring tide in the Red Sea. It was urged too that there would be great difficulty in maintaining the entrances to the harbours and the harbours themselves, and that bars would inevitably form at each end of the canal.

It will be seen therefore that, so recently as 1867, opinions were strongly against the success of the canal, those persons who entertained contrary views being in a considerable minority.

In the commencement of this year Mr. John Fowler, C.E., wrote an excellent letter to 'The Times' on the condition in which he then found the canal, and upon its prospects. The observations which he made, and the conclusions at which he arrived, seem to have been carefully formed and well grounded. He stated that the cost would greatly exceed the original estimate, although the works were carried out of much less than the originally proposed dimensions—that the works were in truth simple in character, and in a soil favourable to execution, but of such vast magnitude, and in a country presenting such peculiar difficulties in climate, and in the absence of fresh water, that special organization and adoption of means of no ordinary kind were required for their realization. He was of opinion that large quantities of alluvium would find their way into the harbour at Port Saïd, and that it would be necessary to make the western breakwater solid to prevent the deposit being carried through, as at present—nevertheless that no apprehension need be entertained as to the channel and harbour being silted up and destroyed, but that considerable expense in dredging would be constantly required. He agreed with Mr. Hawkshaw that the amount of drifting sand would not be such as materially to interfere with maintenance, that various means might be adopted for limiting the amount, but that, after every precaution, it would be necessary to employ one or two powerful dredges to keep the canal clear from the sand blown in. He was further of opinion that the protection of the slopes by stone would be necessary. With reference to the evaporation from the Bitter Lakes, and the current from the Red Sea to those Lakes, he believed that it would not be strong enough to affect injuriously the bottom or sides of the channel, after they had been properly protected by stone pitching. Mr. Fowler then entered into a consideration of the mode in which the traffic should be carried on and the probable use to be made of the canal, and concluded his letter with a well-deserved compliment to the remarkable energy and perseverance of M. Lesseps, to the skill and resources of M. Voisin, the Engineer-in-chief, and the district engineers acting under him, and to the great powers of organization and high qualities of M. Levalley, the contractor.

The total length of the canal from Port Saïd to Suez is 99 miles; it varies in width from 196 feet to 327 feet, having, however, in each case a width of 72 feet in the centre, the slopes on each side of this centre-

width varying with the character of the material cut through. Near Port Saïd, and through the shallow lake of Menzaleh, the material is very sandy; and here and elsewhere, under similar circumstances, the slopes must be protected by stone pitching or facing, or they will wash down by the action of passing vessels, and the material thus deposited in the bottom of the canal must be removed by dredging. Further south, the material generally becomes more argillaceous and stony; and here the slopes will be much more easily maintained, though nearly throughout the whole length of the canal some stone protection at the level of the water will be required.

Before reaching Lake Timsah, which lies about midway between Port Saïd and Suez, the canal passes through the deep cutting of El Guisr, which at its greatest depth is 85 feet to the bottom of the canal. The lower part of this excavation, at and a little above the level of the water, consists of soft clay, above which is a concreted mass of shells and sand; and this is covered by loose sand liable to be acted on by the wind. The canal here is curved and narrow, and ought to be improved in both respects. It is again restricted in width through the deep cutting at Serapeum; but here, the material being argillaceous and strong, the slope will be easily maintained in shape. From the Bitter Lakes to Suez it is a wide, noble, and well-finished canal.

Out of the whole length, nearly 30 miles are through Lake Timsah and the Bitter Lakes, 5½ miles in the first, and 23½ in the latter. In these lakes a deep channel has been dredged out, which is marked by buoys and stakes. These vast sheets of water in the midst of the Desert, on which so many noble vessels were floating, had been but a few months before mere dry depressions, covered by a stratum of salt. The filling them with water commenced in February from the Mediterranean, and in July from the Red Sea. They were filled by the beginning of October, thus belying one of the many unfavourable prophecies, that the absorption and evaporation would be so great that they would never fill at all, or, if they did, the current inwards in both directions would be so great as to be destructive of the canal.

On our voyage from Port Saïd to near Lake Timsah there was a current setting against us towards the Mediterranean. We anchored about ½ mile from the end of this portion of the canal, and at daylight the next morning there was a current in the same direction of nearly 1½ mile an [illegible] Our time of starting from Lake Timsah was purposely delayed till midday, that we might have the tide from the Red Sea against us, [illegible] deep water over the rocks at Serapeum. The current towards Lake Timsah was strong; and on the following morning, between the Bitter Lakes and Suez, it ran at 3½ miles per hour, but a strong southerly wind accompanied the tide. We had no opportunity of making observations ourselves, or of obtaining information; but my impression is that at this season

of the year there is on the average of the day a regular current from the Red Sea to the Mediterranean.

This is an interesting as well as important question; and it is to be hoped that regular observations will be taken at all points along the canal, and at each end, which may show accurately the rise and fall of tide, the velocity and duration of the currents in each direction, and the relative height of the various portions of the canal and the Lakes it traverses.

The range of the tide in the Mediterranean is, as already stated, about 12 inches, while in the Red Sea at Suez it varies from 4 to 6 feet.

On the day of the opening thirty-two vessels reached Lake Timsah without let or hindrance; one Egyptian vessel, the 'Garbia,' coming after this number, stuck fast for some hours about 12 miles from the Lake, and retarded a number of vessels in its rear; but eventually all came forward, and the mighty fleet assembled on Lake Timsah the following day.

At Port Saïd I counted on the day of the inauguration more than ninety vessels, chiefly of the largest class (many being upwards of 2000 tons register), and including a fleet of British "iron-clads," which anchored within the western pier. Here, however, a good deal requires to be done. The harbour is formed by two jetties built of concrete blocks, the western one being run out to sea, at right angles to the shore, for a distance of 2400 metres, and then turned eastwards for 300 metres more. The eastern jetty starts from shore at a distance of 1400 metres from the western pier, is continued out to sea for a length of about 1700 metres, gradually approaching to within about 700 metres of the western jetty at its termination.

The western jetty has been erected for protection, and for the purpose of intercepting the sand and alluvial matter which are undoubtedly drifted from the mouth of the Nile eastwards. This work is too light and too open effectually to answer its purpose, and requires improvement. Close in shore a considerable amount of the drifting sand has been arrested, and where the sea recently flowed there is already an accumulation of dry land. On the land thus formed were erected the temple in which the Viceroy received his principal guests at the inauguration, and the temples for the worship of the Mahometan and Christian churches, where all the religions of the world were supposed to be present and to ask a blessing on the great undertaking the opening of which they were assembled to celebrate. In its present condition, the jetty favours the deposit of material within the harbour; and not until the passage of the sand through the interstices of the concrete blocks of which it is built has been checked, will there be any effectual protection against the silting up which is taking place. Perhaps by degrees an inner bank or shoal may be formed, which would answer the purpose of a breakwater; but this would create a crooked and inconvenient channel, and would be ineffective

towards the seaward end of the jetty. The Company will no doubt see the necessity of completing the necessary works here and elsewhere. The harbour at Port Saïd and portions of the canal will require pretty constant dredging for some time; but in my opinion neither this nor any other work will entail any very serious expense in maintenance.

The cost of the whole undertaking is stated to have been about £16,000,000 sterling; and it may require from £2,000,000 to £4,000,000 more to complete the work satisfactorily on its present scale of dimensions; but interest has to be paid at present on about half only of the capital hitherto raised.

Many persons who are competent to form sound opinions on this point believe that the traffic will be quite sufficient to pay all cost of maintenance and handsome dividends; but I am not sufficiently well informed to hazard any conjecture on the purely financial part of the question. In an engineering point of view I consider the canal a great and most important undertaking—great, however, only as respects its magnitude and the country in which it has been executed. There is not a work of art or of difficulty from one end to the other; but there have been about 80,000,000 cubic yards of material excavated, and at one time nearly 30,000 labourers were employed in the works. For their sustenance, and before operations could be carried on with any vigour, sweet water had to be brought from the Nile at Cairo, and distributed along the whole length of the canal. This work was in itself one of considerable magnitude. It is a navigable canal from Cairo to Ismaïlia, and thence to Suez. From Ismaïlia to Port Saïd and intervening places, the fresh water is conveyed in pipes. The surplus water has been applied to irrigation, the fertilizing results of which are already visible, and may be expected to perform an important part in the improvement of the country.

The canal must be regarded as a great work, more from its relation to the national and commercial interests of the world than from its engineering features. In this light it is impossible to overestimate its importance. It will effect a total revolution in the mode of conducting the great traffic between the East and the West, the beneficial effects of which I believe it is difficult to realize. It is in this sense that the undertaking must be regarded as a great one; and its accomplishment is due mainly to the rare courage and indomitable perseverance of M. Ferdinand Lesseps, who well deserves the respect he has created and the praises which have been bestowed. By cutting across the sandy ligaments which have hitherto united Asia and Africa, a channel of water-communication has been opened between the East and the West which will never again be closed so long as mercantile prosperity lasts or civilization exists.

I cannot close this letter without expressing my obligations to Mr. Pender, Chairman of the Eastern Telegraphic Companies, who courteously entertained me, with other friends, on our passage through the canal on

board the 'Hawk,' a steam corvette belonging to the Electric Telegraph Construction Company, which had been placed at his disposal. On board this vessel were assembled a small body of distinguished and intelligent gentlemen, who had more than usual opportunities of obtaining such information as time and circumstances afforded.

I have the honour to remain,

Very truly and faithfully yours,

JOHN FRED. BATEMAN.

Lieut.-General Sir Edward Sabine, P.R.S., K.C.B.

Memorandum as to the Dimensions of the Canal.

The following, it is believed, are the dimensions on which the canal has been constructed. They are principally extracted from Mr. Fowler's letter.

	Miles, in length.	Width at top water, in feet.
1. From Port Saïd, through Lakes Menzaleh and Ballah, to near El Ferdane..........	37	327
2. From near El Ferdane, through the great excavation of Seuil de Guisr, to Lake Timsah .	9½	196
3. Through Lake Timsah.................	5½	327
4. From Lake Timsah, through the excavation of Seuil de Serapeum, to the Bitter Lakes ..	7½	196
5. Through the Bitter Lakes	23½	327
6. Through the deep portion of Chalouf cutting .	5	196
7. Thence to Suez and the end of the canal....	11	327
Total length	99	

The canal is intended throughout to be 8 metres, or 26 ft. 4 in. in depth. In every case this depth is to be maintained for a width at the bottom in the centre of 72 feet, with slopes on each side of 2 horizontal to 1 vertical to within a few feet of the surface. In the wider portions of the canal the sides above this level are formed with flat slopes of 5 horizontal to 1 vertical, with a horizontal bench between the two slopes of 58 feet in width. A narrower bench is left where the canal is of the smaller width.

On board the 'Hawk' soundings were taken along the whole length of the canal. Between Port Saïd and Lake Timsah the soundings near the centre of the canal, on both sides of the vessel, showed a depth varying from 21 ft. to 29½ ft., the greater number being from 24 to 29. In Lake Timsah the depth, according to soundings, was from 19 to 23 ft. Between Lake Timsah and the Bitter Lakes there were no soundings less than 21 ft., except over the rocks at Serapeum, where vessels drawing

16 ft. only could pass. In the Bitter Lakes the depth was seldom below 28 ft., often above 30 ft.; and the same may be said of the canal between the Bitter Lakes and the Red Sea at Suez. On each side of the deep part in the centre the depth was generally about 12 or 13 ft.

Where the slopes are unprotected by stone, and the natural soil is sandy, the sides, notwithstanding the flat slope, were a good deal washed when a paddle-wheel steamer (the 'Delta,' P. & O. 1600 tons) advanced at seven or eight miles an hour; but comparatively little effect was produced when the speed did not exceed five or six miles an hour.

Two large vessels will find it difficult to pass each other; but "lie-by" or passing-places are being constructed to remedy this inconvenience.

J. F. B.

January 13, 1870.

WARREN DE LA RUE, Vice-President, in the Chair.

The Presents received were laid on the Table, and thanks ordered for them, as follows:—

Transactions.

Basel:—Naturforschende Gesellschaft. Verhandlungen. Theil V. Heft 2. 8vo. *Basel* 1869. The Society.

Bern:—Naturforschende Gesellschaft. Mittheilungen, aus dem Jahre 1868. No. 654–683. 8vo. *Bern* 1869. The Society.

Brussels:—Académie Royale de Médecine. Mémoires. Tome V. Fasc. 1. 4to. *Bruxelles* 1869. Bulletin. 3ᵉ série, Tome II. No. 10; Tome III. Nos. 5–10. 8vo. *Bruxelles* 1868–69. The Academy.

Buenos Aires:—Museo Publico. Anales, por German Burmeister. Entrega 6. 4to. *Buenos Aires* 1869. The Museum

Einsiedeln:—Schweizerische Naturforschende Gesellschaft. Verhandlungen. Jahresbericht 1868. 8vo. *Einsiedeln.* The Society.

Geneva:—Société de Physique et d'Histoire Naturelle. Mémoires. Tome XX. Partie 1. 4to. *Genève* 1869. The Society.

Göttingen:—Königl. Sternwarte. Astronomische Mittheilungen. Theil I. 4to, *Göttingen* 1869. The Royal Society of Göttingen.

Graz:—Naturwissenschaftlicher Verein für Steiermark. Mittheilungen. Band II. Heft 1. 8vo. *Graz* 1869. The Society.

Haarlem:—Musée Teyler. Archives. Vol. II. Fasc. 3. roy. 8vo. *Haarlem* 1869. The Museum.

London:—Quekett Microscopical Club. Journal. Nos. 7–9. Fourth Report. 8vo. *London* 1869–70. The Club.

Utrecht:—Provinciaal Utrechtsch Genootschap van Kunsten en Wetenschappen. Natuurkundige Verhandelingen. Nieuwe Reeks. Deel I.

Transactions (*continued*).

Stuk 6. 4to. *Utrecht* 1869. Verslag van het Verhandelde in de algemeene Vergadering ... gehouden den 29. Juni 1869. 8vo. *Utrecht* 1869. Aanteekeningen van het Verhandelde in de Sectie-Vergaderingen. 8vo. *Utrecht* 1869. The Society.

Vienna:—K. Akademie der Wissenschaften. Sitzungsberichte. Math.-Naturw. Classe: Erste Abth. Band LVIII. Hefte 1–5; Band LIX. Hefte 1, 2. Zweite Abth. Band LVIII. Hefte 2–5; Band LIX. Hefte 1–3. Phil.-Hist. Classe. Band LX. Hefte 1–3; Band LXI. Heft 1. Register zu den Bänden 51–60. 8vo. *Wien* 1868–69. Tabulæ Codicum Manu Scriptorum præter Græcos et Orientales in Bibliotheca Palatina Vindobonensi asservatorum. Vol. III. 8vo. *Vindobonæ* 1869. Die Porphyrgesteine Österreichs aus der mittleren geologischen Epoche, von Dr. G. Tschermak. 8vo. *Wien* 1869. The Academy

---

Bellucci (G.) Sull' Ozono, Note e Riflessioni. 8vo. *Prato* 1869. The Author, by Dr. Tyndall, F.R.S.

Duncan (P. M., F.R.S.) First Report on the British Fossil Corals. 8vo *London* 1868. The Author.

Hanbury (D., F.R.S.) Historical Notes on Manna. 8vo. *London* 1869. The Author

Hill (M.D.) Is Allegiance by Birth affected in England by Naturalization in France? Opinions of M. Cremieux, M. Bonneville de Marsangy, of Lord Campbell, and of Lord Brougham. 8vo. *Bristol* 1869. The Author.

Lankester (E., F.R.S.) Sixth Annual Report of the Coroner for the Central District of Middlesex. 8vo. *London* 1869. The Author.

Leymerie (A.) Catalogue des Travaux Géologiques et Minéralogiques publiés jusqu'en 1870. 8vo. *Paris* 1869. The Author.

Littrow (C. von.) Ueber das Zurückbleiben der Alten in den Naturwissenschaften. 8vo. *Wien* 1869. The Author.

Newmarch (W., F.R.S.) Inaugural Address. Statistical Society, Nov. 16, 1869. 8vo. *London* 1869. The Author.

Roussillon (Duc du) Origines, Migrations, Philologie et Monuments Antiques. Vol. I. Partie 1, 2. 8vo. *Londres* 1867. The Author.

Zöllner (J. C. F.) Ueber ein neues Spectroskop nebst Beiträgen zur Spectralanalyse der Gesteine. Beobachtungen von Protuberanzen der Sonne. Astrophysik. 8vo. *Leipzig* 1868. The Author.

The following communications were read:—

I. "On the Mineral Constituents of Meteorites." By NEVIL STORY-MASKELYNE, M.A., Professor of Mineralogy in the University of Oxford, and Keeper of the Mineral Department, British Museum. Communicated by Prof. H. J. STEPHEN SMITH. Received October 9, 1869.

(Abstract.)

I. *The Application of the Microscope to the Investigation of Meteorites.*

The difficulties in the way of the complete investigation of a meteorite resemble those we meet with in terrestrial rocks. In both the ingredient minerals are minute, and are often, especially in the case of the aërolitic rock, very imperfectly crystallized. Moreover the methods for separating them, whether mechanically or chemically, are very incomplete. With a view to obtain some more satisfactory means of dealing with these aggregates of mixed and minute minerals, I sought the aid of the microscope, by having in the first place sections of small fragments cut from the meteorites so as to be transparent.

One may learn, by a study and comparison of such sections, something concerning the changes that a meteorite has passed through; for one soon discovers that it has had a history, of which some of the facts are written in legible characters on the meteorite itself; and one finds that it is not difficult roughly to classify meteorites according to the varieties of their structure. In this way one recognizes constantly recurring minerals; but the method affords no means of determining what they are. Even the employment of polarized light, so invaluable where a crystal is examined by it of which the crystallographic orientation is at all known, fails, except in rare cases, to be a certain guide to even the system to which such minute crystals belong. It was found that the only satisfactory way of dealing with the problem was by employing the microscope chiefly as a means of selecting and assorting out of the bruised débris of a part of the meteorite the various minerals that compose it, and then investigating each separately by means of the goniometer and by analysis, and finally recurring to the microscopic sections to identify and recognize the minerals so investigated. The present memoir deals with the former part of this inquiry. Obviously the amount of each mineral thus determined, after great care and search, can only be extremely small, as only very small amounts of a meteorite can be spared for the purpose, notwithstanding that as large a surface as possible of its material requires to be searched over for instances of any one of the minerals occurring in a less than usually incomplete form. On this account one has to operate with the greatest caution in performing the analysis of such minerals; and the desirability of determining the silica with more precision than is usually the case in operations on such minute quantities of a silicate suggested to me the process, which, after several experiments in perfecting it, assumed the following form.

II. *On the Method of Analyzing Silicates that do not gelatinize with Hydrogen Chloride.*

The process is conducted in an apparatus of the following construction. A platinum retort, 30 cub. centims. in capacity, is fitted with a tubulated stopper of the same material, which reaches nearly to the bottom; a small tube entering the vertical tube of the stopper at an angle, above the neck of the retort, conveys hydrogen to its interior. The vertical tube can be closed either by a stopper of platinum or by a funnel of that metal, stopped in like manner at the top, and having a fine orifice at its lower extremity.

To the side of the retort, just below its neck, a straight delivery-tube is fixed, which in its turn fits into another platinum tube that, after taking a curve into a vertical position, is enlarged into a cylinder, which passes a considerable distance down a test-tube. The latter, into which the delivery-tube is fitted with a cork, holds 7·5 cub. centims., or 6·6 grammes of strong ammonia of the spec. gravity 0·88.

The gas-delivery tube inserted in the side of this receiver dips into some more ammonia in a second test-tube.

The pounded mineral, from 0·2 to 0·5 gramme in quantity, and a small platinum ball, are placed in the retort, and the stopper luted to it with gutta percha, and cemented air-tight in its place with caoutchouc and gutta-percha varnish. The funnel, filled with perfectly pure hydrogen fluoride, is now introduced into the tubulure of the stopper, the tap opened, and the acid allowed to run down into the retort. This acid contains about 32 per cent. of absolute hydrogen fluoride—that is to say, a funnel of this reagent contains 1·12 gramme of acid, capable of rendering gaseous 0·84 gramme of silica, and of neutralizing 0·95 gramme of ammonia. The funnel is now replaced by a little platinum stopper, and the orifice secured air-tight with gutta-percha varnish. Pure hydrogen is then allowed slowly to traverse the entire apparatus, the retort is placed in a water-bath at 100°C. for two hours, and occasionally slightly shaken to set the ball rotating. During the operation a trace only of silicium difluoride passes over.

The retort is next transferred to a paraffin-bath, and the temperature is cautiously raised. At first hydrogen fluoride passes over, and at this point of the process the flow of hydrogen requires some attention to prevent regurgitation of the ammonia. At about 132° C., in the case of the silicates mentioned in this memoir, the silica first becomes visible in fine flocks in the ammonia of the receiver, and in another minute the whole is cloudy.

In eight minutes the rise of the thermometer to 145° C. has brought over so much difluoride that the contents of the tube are semisolid, and nearly the whole of it has passed over. The temperature is then raised to 150° C., and the retort allowed to cool. The process is next repeated with a fresh charge of acid and ammonia. If no more than 0·2 gramme of silicate be taken, twice charging of the retort is sufficient; but with 0·5 gramme three or four repetitions of the process are required. In short, the operation is continued with fresh reagents till no flock of silica forms

in the receiver. Finally, 0·75 cub. centim. of sulphuric acid is introduced into the retort, and the temperature again raised to 160°C., the stream of hydrogen being continued as before.

The several ammoniacal charges are poured into a platinum dish, together with the washings of the delivery-tube and the two test-tubes, and slowly evaporated in a water-bath, with continued stirring.

At a point in the evaporation just before the solution becomes neutral and the ammonium fluoride begins to turn acid, the entire silica in the dish will have been dissolved by the fluoride. The process is gradual, but the moment when the solution is complete is easily determined. Then, the dish being removed, potassic chloride is added in slight excess, together with absolute alcohol equal in volume to the contents of the platinum vessel. Potassium fluosilicate precipitates, which, after the lapse of twenty-four hours, is filtered, washed with a mixture of equal volumes of absolute alcohol and water, dried, and weighed. The results are accurate. In the retort are the bases in the form of sulphates, the treatment of which calls for no further remark.

III. *The Busti Aërolite of* 1852.

This meteorite fell on the 2nd of December, 1852, about six miles south of Busti, a station halfway between Goruckpoor and Fyzabad in India, and nearly in lat. 26° 45′ N. and long. 82° 42′ E. For an account of the circumstances attending its fall I am indebted to Mr. George Osborne, at that time resident at Busti, and who presented this stone (the only specimen of the fall that he was able to procure) to the East India Company. Mr. Osborne states that the fall took place at ten minutes past ten in the morning, and was attended by an explosion louder than a thunderclap, and lasting from three to five minutes. At Goruckpoor the report appeared to approach in a direction from N.N.W.; at Busti the sound seemed to come from the zenith, and proceed in a somewhat easterly course.

The explosion that shattered the meteorite must have occurred soon after its passing the longitude of Goruckpoor. There was no cloud in the sky at the time. The stone, which weighed about 3 lbs., was presented to the collection at the British Museum by the Secretary of State for India.

The Busti aërolite bears a great resemblance to the stone that fell on the 25th of March, 1843, at Bishopsville, South Carolina, U.S. A crust, coating the larger part of the stone, was of a dark yellowish brown, with a few yellowish-white porphyritic-looking patches at it flat end, whilst a yellowish enamel, mingled with dark grey, covered a hollow portion on one side of the stone.

It is difficult to refer these markings to the minerals underlying them, a similar crust covering both the augite and enstatite of the meteorite. They are probably due to the alterative action of the oxidized products of the nickeliferous iron on the silicates in a state of fusion during the rapid passage of the stone through the atmosphere.

The meteorite consists for the most part of the mineral enstatite; at one end, however, was imbedded a number of small chestnut-brown spherules, in which again a lens enabled me to detect minute octahedral crystals, having the lustre and colour of gold.

These two minerals seem scarcely to have been affected by the heat that fused the silicates which surround and encrust them.

IV. *Sulphide of Calcium* (Oldhamite).

This mineral occurs in the Busti aërolite, and sparsely in that which fell at Bishopsville, imbedded in augite, or enstatite, or both of them. It has a pale chestnut-brown colour, and forms small, nearly round spherules, whose outer surface is generally coated with calcium sulphate. It cleaves with equal facility in three directions, which give normal angles, averaging 89° 57′, and are no doubt really 90°. Its system, therefore, is cubic; indeed in polarized light it is seen to be devoid of double refraction. Its specific gravity is 2·58, and its hardness 3·5 to 4. With boiling water it yields calcium polysulphides, and in acids it easily dissolves with evolution of hydrogen sulphide. Chemical analyses indicated the following as its composition:—

		I.	II.
Oldhamite	Calcium monosulphide	89·369	90·244
	Magnesium monosulphide	3·246	3·264
Gypsum		3·951	4·189
Calcium carbonate		3·434	——
Troilite		——	2·303
		100·000	100·000

The presence of such a sulphide in a meteorite shows that the conditions under which the ingredients of the rock took their present form are unlike those met with in our globe. Water and oxygen must have alike been absent. The existence of iron in a state of minute division, as often found in meteorites, leads to a similar conclusion. But if we bear in mind the conditions necessary for the formation of pure calcium sulphide, the evidence imported into this inquiry by the Busti aërolite seems further to point to the presence of a reducing agent during the formation of its constituent minerals; whilst the crystalline structure of the Oldhamite and of the Osbornite must certainly have been the result of fusion at an enormous temperature. The detection of hydrogen in meteoric iron by Professor Graham tends to confirm the probability of the presence of such a reducing agent.

V. *Osbornite.*

The golden-yellow microscopic octahedra imbedded in the Oldhamite were furnished by the analyses of that mineral to the amount of only 0·0028 gramme, and though upwards of 150 in number, were capable of being measured by the goniometer.

This microscopic mineral I wish to name Osbornite, in honour of Mr.

Osborne and in commemoration of the important service that gentleman rendered to science in preserving and transmitting to London in its entirety the stone which his zeal saved at the time of its fall.

That the octahedra of Osbornite are regular was proved by angles of even such microscopic crystals giving measurements over the edges and solid angles that accorded within 3′ with those of the regular octahedron.

The crystals are brittle, and their powder retains the beautiful yellow colour of the surface, which is therefore intrinsic, and not a tarnish. The amount of them available for analysis being so minute, their chemical examination was attended with much difficulty. Boiled for a long time in the strongest hydrogen chloride, they were unchanged, and hydrogen fluoride was apparently without action on them. They passed unscathed through a fusion with potassio-sodium carbonate. When heated on a splinter of porcelain in a current of dry chlorine, the crystals glowed for a few seconds, lost their metallic lustre, and became of a honey-yellow colour, while a white sublimate formed on the walls of the tube. Exposed to the air, the altered crystals deliquesced, and assumed a pasty consistence; in water they dissolved partially, forming an alkaline solution, in which ammonium oxalate produced a precipitate. The insoluble portion was taken up for the most part by hydrogen chloride, and its solution gave a decided precipitate with the above reagent. The water through which the chlorine was allowed to escape, and the sublimate in the tube, after treatment with hydrogen chloride, were taken together, and found, on examination, to give a white precipitate with barium chloride, the filtrate from which, after the excess of barium had been removed, furnished with ammonia a precipitate resembling alumina, which, however, was insoluble in potash, and was thrown down from slightly acid solutions with sodium hyposulphite, and potassium sulphate. It was examined for titanic acid by means of magnesium wire in a slightly acid solution, but with a negative result. The only alternative left was to conclude that the substance which exhibited this deportment was either titanium or zirconium, and that the gold-like crystals were a combination of this element with calcium (perhaps a little iron) and sulphur in some remarkably stable form. That this mineral should be a compound of the sulphides of these metals merely is scarcely conceivable when its power to withstand the action of acids is considered; possibly its composition, if it could be quantitatively analyzed, would be found to be that of a compound of titanium or zirconium and calcium of the obscure kind that is known as an oxysulphide.

Mr. Sorby, who has made the zirconium and titanium group of metals the subject of special study, formed a microscopic borax bead, into which he introduced some of the oxide obtained from the Osbornite. He found it to behave as titanic acid.

The occurrence of Osbornite occasionally in the augite presently to be described, and the fact of the latter mineral lying chiefly in that part of the meteroite where the Osbornite is found, suggested the possibility of the

presence of this metal of the zirconium group in the augite itself,—an assumption confirmed by experiment. The dichroism of this augite is strongly marked, especially through the face 0 1 0, which in one position exhibits a tint resembling that of the blue anatase of Brazil, due apparently to minute scales permeating the crystal, and visible only in the microscope. These scales may possibly be the Osbornite sufficiently thin to be transparent, and may be the cause of the beautiful golden metallic reflection which characterizes the face 1 0 0 of the augite.

VI. *The Augitic Constituent of the Busti Aërolite.*

Associated with the spherules of Oldhamite that have been described as occurring in a nodule of this aërolite, and less plentifully distributed through the rest of its mass, is the silicate already alluded to as a variety of augite, and as containing traces of titanium or zirconium oxide. This silicate occurs in crystalline grains of a pale violet-grey colour, intimately mixed with another silicate presently to be described. When isolated, these grains present a few crystal faces, among which one as a cleavage-plane is prominent. So imperfect are the rest, that they furnished reliable measurements only with extreme difficulty. These determinations, however, together with its optical characters, proved that the mineral belongs to the oblique system. The measurements gave the following approximate values:—

		Angles found.	Angles of diopside.
001	100	About 75° 30′	73° 59′
001	110	About 81°	79° 29′
110	100	45° 54′ to 47° 26′	46° 27′
$11\bar{0}$	110	85° 8′ to 86° 20′	87° 5′
100	111 (?)	53° 25′ to 54° 15′	53° 50′
001	$\bar{1}10$	100° 8′	100° 57′

The plane containing the optic axis is perpendicular to the edge 1 0 0, 1 0 0, and the optical character in the centre of the field is negative on looking down the second mean line, which makes angles about 22° 45′ and 52° 30′ with the normals to the faces 0 0 1 and 1 0 0 respectively.

Two analyses of this mineral by the method described gave the following results:—

	I.	II.	Mean oxygen ratios.
Silicic acid	55·389	55·594	29·928
Magnesia	23·621	23·036	9·331
Lime	20·02	19·942	5·709
Iron oxide	0·78	0·309	
Soda	0·554	[0·554]	
Lithia............	trace	[trace]	
	100·364	99·435	

Viewed as a magnesium calcium silicate, the percentage composition becomes—

Silicic acid	56·165	56·604
Magnesia	23·612	23·585
Lime	20·223	19·811
	100·000	100·000

The second column gives the percentage composition according with the formula

$$(\tfrac{5}{8}\ Mg,\ \tfrac{3}{8}\ Ca)\ O,\ SiO_2.$$

Such a formula does not accord with those of the ordinary varieties of augite, in which calcium is usually present in at least as high a ratio in equivalents as the magnesium. A deduction, however, of a certain amount of purely magnesian enstatite corresponding in chemical type to the augite has to be made by reason of the presence of the white mineral intercalated in layers along a direction parallel to the plane 0 0 1, and sometimes to a second plane. This white mineral is, there can be no doubt, the mineral next to be described, and its presence would modify the apparent formula of the augite as derived from analysis, increasing the magnesia.

The trace of the titanoid element in this mineral is included with the iron oxide in the above analyses.

VII. *On the Occurrence of Enstatite in the Busti Aërolite.*

Besides the augite already described there occurs in this meteorite another silicate which constitutes its most important ingredient. The augite is chiefly found in the nodule with the calcium sulphide, and is found more sparsely in the remaining parts. Associated with it throughout, and otherwise forming the chief mass of the stone, is a mineral which, in microscopic sections, presents the appearance of a number of more or less fissured crystals of varying transparency, some clear, some nearly opaque, and usually presenting a not very unsymmetrical polygonal outline. Those crystals are imbedded in a magma of fine-grained silicate, itself often entangled in an irregular meshwork of opaque white mineral. Amongst these ingredients, when mechanically separated, what seems to be three different minerals can be distinguished. The rarest of them is transparent and colourless, and very irregular in the form of its fragments; a second is of a greyish-white colour, translucent, and offering an even less hopeful problem to the crystallographer than that presented by the first. The third is an opaque mineral with a distinct cleavage following the faces of a prism of about $\frac{88·35}{91·27}$, and with a second imperfect cleavage perpendicular to the former. From a few fragments of the two former kinds some measurements were obtained, which conduct to the conclusion that, like the last-mentioned silicate, these minerals are enstatite. The angles 1 0 0, 1 1 0 are 46° 25′, and 1 0 0, 1 0 1, 41° 34′.

Chemical analysis confirmed the identity of these three minerals by

showing them to be enstatite under different aspects. When lime is absent it presents itself as a simply prismatic mineral, the dark-grey tabular variety. When lime is present, though to an amount less than two per cent., the crystalline structure becomes more complex. The augite may perhaps be tessellated, as it were, in the enstatite, somewhat as this latter mineral has been shown to occur intercalated to a small amount in layers of augite. I did not succeed in establishing this point, however, by an examination of microscopic sections of this mineral.

The crystalline fragments frequently show, when examined by polarized light, a composite structure, the principal sections of the different parts of the mineral being disposed at every angle of mutual inclination.

The analysis of these minerals yielded the following numbers :—

	Dark Grey Tabular Variety.		Transparent White Variety.	
	Percentages.	Oxygen ratios.	Percentages.	Oxygen ratios.
Silicic acid....	57·597	30·718	58·437	31·166
Magnesia	40·64	16·238	38·942	15·564
Lime........	——		1·677	0·479
Iron oxide....	1·438		1·177	
Potash	0·394		0·332	
Soda........	0·906		0·357	
	100·975		100·922	

Semitransparent Grey Variety.

	Percentages. I.	Oxygen ratios.	Percentages. II.	Oxygen ratios.	Percentages. III.	Oxygen ratios.
Silicic acid..	57·037	30·419	57·961	30·912	57·754	30·802
Magnesia ...	40·574	16·117	39·026	15·598	38·397	15·247
Lime......	2·294	0·655	1·524	0·435	2·376	0·678
Iron oxide ..	0·867		0·154		0·423	
Potash	——		0·569		0·569	
Soda	——		0·68		0·657	
Lithia......	——		——		0·016	
	100·772		99·914		100·192	

As in the case of the augite, the soda is probably derived from the hydrogen chloride; the iron occurs partly as metal, minutely subdivided, partly as oxide combined with the magnesium silicate. In each case the bases slightly exceed the amount required by the formula of enstatite. On comparing these with known analyses, and those which I shall shortly submit to the Society, it seems highly probable that, where the conditions under which a meteoric silicate has been formed were such that silicic acid was present in excess of that required by the formula of enstatite, this acid

remains uncombined in the form of crystallized silica with the specific gravity of a fused quartz, and that where magnesia and other bases are in excess, a basic silicate with the formula of olivine absorbs the supplementary portion of these bases. Calcium, when present, would convert into augite its equivalent ratio of what would otherwise constitute enstatite, and it is possible that this is true even when this element is distributed in small quantities throughout the mass.

No alumina, and consequently no feldspathic ingredient, has been detected in this meteorite.

VIII. *Composition of the entire Meteorite.*

With the view of determining the different ingredient minerals present in the Busti meteorite, fragments and dust from the neighbourhood of the nodule of sulphide and augite were analyzed. The mineral was treated with hydrogen chloride, carbon disulphide, and potash, which removed 16·873 per cent., leaving a residue of 83·127 per cent.; the composition of these two portions, soluble and insoluble, is given below:—

	Soluble portion.		Insoluble portion.	
	Percentages.	Oxygen ratios.	Percentages.	Oxygen ratios.
Calcium sulphate....	0·442			
Calcium sulphide....	4·133			
Iron oxide	0·194	....	0·891	
Silicic acid	6·514	3·474	46·357	24·727
Lime..	0·022	0·006	12·375	3·535
Magnesia..........	5·055	2·02	23·266	9·299
Potash............	0·099		0·14	
Soda	0·118		0·455	
Lithia............	——		0·019	
	16·577		83·503	

The ratio of the silicic acid to the magnesia and lime in the latter analysis corresponds with the composition $(\frac{3}{4}\mathrm{Mg}\,\frac{1}{4}\mathrm{Ca})\mathrm{O}, \mathrm{SiO_2}$. Regarding the calcium of the white and grey varieties of enstatite to be present as augite intercalated with the enstatite, we may assert that while all the silicates of this meteorite present the typical formula $\mathrm{MO}, \mathrm{SiO_2}$, three equivalents of the rock near the nodule may be treated as composed of two equivalents of augite and one of enstatite; in other parts of the stone the latter mineral predominates.

A formula for the augite with magnesium and calcium in equal proportions would no doubt more truly represent its composition; it is, however, as impossible to separate the enstatite intercalated with it as it is to remove this mineral when blended with the enstatite.

IX. *Solubility of the Minerals of the Busti Meteorite.*

As it appears of importance to determine the degree to which these

meteoric minerals were soluble in acid, the augite and enstatite were submitted to this solvent action. Digested for several hours at 100° C. in hydrogen chloride diluted with half its volume of water, and subsequently in potash for some hours to remove the free silica, the augite and each of the three forms of enstatite proved to be acted upon, the results in all cases showing that the acid simply exercises a solvent action on the mineral, without separating it into two or more distinct silicates.

The subjoined Table gives the results of the experiments. The degree in which the acid dissolved the mineral was due to the more or less complete trituration of the material before treatment. In one case, for which the transparent variety was selected, a repetition of the process three times gave results that left no doubt as to the nature of the action of the acid.

Of the greyish-white variety of enstatite, after treatment for 20 hours with acid and 12 hours with potash, 9·414 per cent. dissolved, an analysis of which is given in column I.

Of the grey tubular variety of enstatite, after treatment with acid for 16 hours and with potash for a similar time, 7·779 per cent. dissolved, that gave on analysis numbers the approximate value of which is found in column II.

Of the white variety, after the first treatment for 20 hours with acid and subsequently with potash, 12·68 per cent. dissolved, the composition of which is given in column III. By a second treatment of the residual enstatite from this experiment, after 2 hours' trituration with acid for 30 hours and potash for 12 hours, 67·84 per cent. dissolved; and on subjecting the mineral to a third treatment in a similar way, 51·18 per cent. were dissolved in acid and potash. In the last of these experiments the ratio of the silica to the bases, neglecting the small amount of the former dissolved in the acid, is as 58·4 to 42·0, that of an analysis of an enstatite being as 58·4 to 41·6.

The solubility of the augite was determined by subjecting it to similar treatment with acid during 18 hours, and with potash for a like time, these reagents removing 7·384 per cent. of the mineral.

	I.	II.	III.
Silicic acid	5·408	5·141	6·724
Magnesia	2·367	1·353	4·61
Lime	1·048	0·27	0·432
Iron oxide, &c.	0·187	0·676	0·576
Potash	0·121	0·528	0·504
Lithia	——	trace	trace
	9·131	7·968	12·846
Soda found	[0·126]	[1·217]	[1·042]

X. *The Iron of the Busti Meteorite.*

A small pepita of the iron contained in the meteorite was analyzed.

Omitting the silicate attached to the iron, the results of the analysis were as follow:—

Iron-nickel alloy		98·798
Iron	94·949	
Nickel	3·849	
Schreibersite		1·202
Iron	0·884	
Nickel	0·234	
Phosphorus	0·084	
	100·000	

The quantity was far too small to encourage a search for cobalt and other metals.

Besides the nickeliferous iron, which is disseminated very sparsely, and in particles singularly unequal in size and distribution, and with which troilite is associated in very small quantity, chromite is present as a constituent of small but appreciable amount. The crystals of this mineral are distinct and brilliant, and sometimes present good angles for measurement. One gave the solid angle of a regular octahedron.

The Manegaum Meteorite of 1843.

This meteorite fell at Manegaum in Khandeish, India, on the 26th July, 1843. Only a small fragment was preserved, and of this a portion was given by the Asiatic Society of Bengal to the British Museum in 1862. In 1863 I described its appearance as seen in section in the microscope, and gave the particulars of its fall (Phil. Mag. August 1863).

From the minuteness of the specimen I had very little material to work upon. One mineral is conspicuous in the stone, namely, a primrose-coloured transparent crystalline silicate in small grains, loosely cemented by a white flocculent mineral. This greenish-yellow mineral (I.) and a fragment of the entire meteorite (II.) were analyzed, and crystalline grains of the former were measured on the goniometer. The prism angles (1) for the prism {1 1 0} were about $\frac{88^\circ}{92^\circ}$, and (2) for the prism {1 0 1} were $\frac{81^\circ 52'}{98^\circ 8'}$ for {1 0 0, 1 1 0} about 46°; for {1 0 0, 1 0 1}, 49° 4'; and for {1 1 0, 1 0 1} 58° 39'.

The analyses gave the following numbers:—

	I.	Oxygen ratios.	II.	Oxygen ratios.
Silicic acid	55·699	29·706	53·629	28·602
Magnesia	22·799		23·32	
Iron oxide	20·541	14·059	20·476	14·305
Lime	1·316		1·495	
Chromite	——		1·029	
	100·355		99·949	

The specific gravity of the granular mineral is 3·198, and its hardness 5·5.

The result of the above analyses is to show that, except for a little chromite and a little augite, with possibly in the crystallized mineral a little free silica, both that mineral and the collective silicate of the stone consist of a ferriferous enstatite.

The formula most in accordance with the analysis would be

$$(\tfrac{2}{3}\,\mathrm{Mg}\,\tfrac{1}{3}\,\mathrm{Fe})\mathrm{O},\ \mathrm{SiO_2};$$

that of the enstatite in the Breitenbach meteorite is $(\frac{4}{5}\,\mathrm{Mg}\,\frac{1}{5}\,\mathrm{Fe})\mathrm{O},\ \mathrm{SiO_2}$.

The bulk of the Busti meteorite consists of a purely magnesian enstatite; this of Manegaum is almost entirely an enstatite richer in iron than any yet examined. Both bear evidence to the white flocculent mineral which characterizes the microscopic sections of many meteorites, being composed of this now important mineral enstatite.

In publishing the results I have obtained in the attempt, so far as this memoir goes, to treat exhaustively of the mineralogy of two important meteorites, I wish to record the obligations I am under to Dr. Flight, Assistant in my Department at the British Museum, for his valuable aid in the chemical portion of the inquiry.

II. "On Fluoride of Silver.—Part I. By George Gore, F.R.S.

Received October 5, 1869.

(Abstract.)

This communication treats of the formation, preparation, analysis, composition, common physical properties, and chemical behaviour of fluoride of silver.

The salt was prepared by treating pure silver carbonate with an excess of pure aqueous hydrofluoric acid in a platinum dish, and evaporating to dryness, with certain precautions. The salt thus obtained invariably contains a small amount of free metallic silver, and generally also traces of water and of hydrofluoric acid, unless special precautions mentioned are observed. It was analyzed by various methods: the best method of determining the amount of fluorine in it consisted in evaporating to dryness a mixture of a known weight of the salt dissolved in water, with a slight excess of pure and perfectly caustic lime in a platinum bottle, and gently igniting the residue at an incipient red heat until it ceased to lose weight. By taking proper care, the results obtained are accurate. The reaction in this method of analysis takes place according to the following equation, $2\mathrm{AgF}+\mathrm{CaO}=\mathrm{CaF_2}+2\mathrm{Ag}+\mathrm{O}$. Sixteen parts of oxygen expelled equal thirty-eight parts of fluorine present. One of the methods employed for determining the amount of silver consisted in passing dry ammonia over the salt in a platinum boat and tube at a low red heat. The results ob-

tained in the various analyses establish the fact that pure fluoride of silver consists of 19 parts of fluorine and 108 of silver.

Argentic fluoride is usually in the form of yellowish brown earthy fragments; but when rendered perfectly anhydrous by fusion, it is a black horny mass, with a superficial satin lustre, due to particles of free silver. It is extremely deliquescent and soluble in water; one part of the salt dissolves in ·55 part by weight of water at 15°·5 C.; it evolves heat in dissolving, and forms a strongly alkaline solution. It is nearly insoluble in absolute alcohol. The specific gravity of the earthy-brown salt is 5·852 at 15°·5 C.; the specific gravity of its aqueous solution, at 15°·5 C., saturated at that temperature, is 2·61. By chilling the saturated solution, it exhibited the phenomenon of supersaturation and suddenly solidified, with evolution of heat, on immersing a platinum plate in it. The solution is capable of being crystallized, and yields crystals of a hydrated salt; the act of crystallization is attended by the singular phenomenon of the remainder of the salt separating in the anhydrous and apparently non-crystalline state, the hydrated salt taking to itself the whole of the water. The fused salt, after slow and undisturbed cooling, exhibits crystalline markings upon its surface.

The dry salt is not decomposed by sunlight; it melts below a visible red heat, and forms a highly lustrous, mobile, and jet-black liquid. It is not decomposed by a red heat alone; but in the state of semifusion, or of complete fusion, it is rapidly decomposed by the moisture of the air with separation of metallic silver; dry air does not decompose it. In the fused state it slightly corrodes vessels of platinum, and much more freely those of silver.

The salt in a state of fusion with platinum electrodes conducts electricity very freely, apparently with the facility of a metal, and without visible evolution of gas or corrosion of the anode; a silver anode was rapidly dissolved by it, and one of lignum-vitæ charcoal was gradually corroded. A saturated aqueous solution of the salt conducted freely with electrolysis, crystals of silver being deposited upon the cathode, and a black crust of peroxide of silver upon the anode; no gas was evolved; with *dilute* solutions gas was evolved from the anode. By electrolysis of anhydrous hydrofluoric acid with silver electrodes, the anode was rapidly corroded.

The electrical order of substances in the fused salt was as follows, the first-named being the most positive: silver, platinum, charcoal of lignum-vitæ, palladium, gold. In a dilute aqueous solution of the salt, the order found was: aluminium, magnesium, silicon, iridium, rhodium, and carbon of lignum-vitæ, platinum, silver, palladium, tellurium, gold.

The chemical behaviour of the salt was also investigated. In many cases considerable destruction of the platinum vessels occurred, either in the experiments themselves, or in the processes of cleaning the vessels from the products of the reactions.

Hydrogen does not decompose the dry salt, even with the aid of sunlight, nor does a stream of that gas decompose an aqueous solution of the salt, but the dry salt is rapidly and perfectly decomposed by that gas at an incipient red heat, its metal being liberated.

Nitrogen has no chemical effect upon the salt, even at a red heat, nor upon its aqueous solution. Dry ammonia gas is copiously absorbed by the dry salt. In one experiment the salt absorbed about 844 times its volume of the gas. The salt in a fused state is rapidly and perfectly decomposed by dry ammonia gas, and its silver set free. A saturated solution of the salt is also instantly and violently decomposed by strong aqueous ammonia.

Oxygen has no effect either upon the dry salt at 15° C., or at a red heat, nor upon its aqueous solution. Steam perfectly and rapidly decomposes the salt at an incipient red heat, setting free all its silver. No chemical change took place on passing either of the oxides of nitrogen over the salt in a state of fusion.

By passing anhydrous hydrofluoric acid vapour over perfectly anhydrous and previously fused fluoride of silver, at about 60° Fahr., distinct evidence of the existence of an acid salt was obtained. This acid salt is decomposed by a slight elevation of temperature.

Numerous experiments were made to ascertain the behaviour of argentic fluoride in a state of fusion with chlorine, and great difficulties were encountered in consequence of the extremely corrosive action of the substances when brought together in a heated state. Vessels of glass, platinum, gold, charcoal, gas carbon, and purified graphite were employed*. By heating the salt in chlorine, contained in closed vessels, formed partly of glass and partly of platinum, more or less corrosion of the glass took place, the chlorine united with the platinum and fluoride of silver to form a double salt, and a vacuum was produced. By similarly heating it in vessels composed wholly of platinum, the same disappearance of chlorine, the same double salt, and a similar vacuum resulted. Also, by heating it in vessels composed partly of gold, an analogous double salt, the same absorption of chlorine and production of rarefaction were produced. And by employing vessels partly composed of purified graphite, a new compound of fluorine and carbon was obtained.

III. "Approximate determinations of the Heating-Powers of Arcturus and *α* Lyræ. By E. J. STONE, F.R.S., First Assistant at the Royal Observatory, Greenwich. Received October 13, 1869.

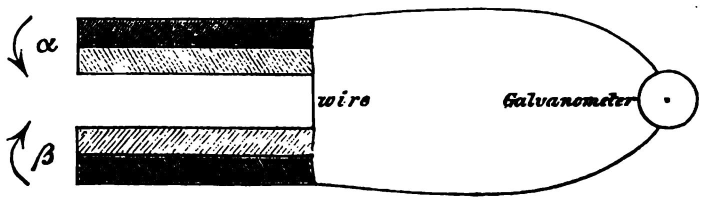
About twelve months ago I began to make observations upon the heating-power of the stars. My first arrangements were simply these: I made

* In the next communication will be described the results obtained with vessels formed of other materials.

use of a delicate reflecting astatic galvanometer, and a thermo-electric pile of nine elements. The pile was screwed into the tube of a negative eyepiece of the Greenwich Great Equatoreal, from which the eye-lenses had been removed.

I soon convinced myself that the heat, condensed by the object-glass of twelve and three-quarters inches upon my pile, was appreciable in the case of several of the brighter stars; but the endless changes in the zero-point of the galvanometer-needle, and the magnitude of these changes, compared with those arising from the heating-power of the stars, prevented me from making any attempts to estimate the absolute magnitude of the effects produced. Every change in the state of the sky, every formation or dissipation of cloud, completely drove the needle to the stops.

At the February Meeting of the Royal Astronomical Society I first became aware of what Mr. Huggins had done upon this question. His arrangements, however, did not appear to me to meet the difficulties which I had encountered. After some trials, I arranged my apparatus as follows, and with its present form I am satisfied.



α and β are two pairs of plates of antimony and bismuth. The areas are about $(0\cdot075)^2$ inches, and their distance is about 0·25 inch.

The poles are joined over in opposite directions to the terminals of the pile and galvanometer. The whole pile is screwed into a tube of one of the negative eyepieces of the great equatoreal. This completely shuts the pile up in the telescope-tube. A thick flannel bag is then wrapped over the eyepiece and terminals. The bag is prevented from actually touching the case of the pile, and is useful in preventing the irregular action of draughts upon the case of the pile and terminals. The wires are led from the terminals of the pile to the observatory library, where I have placed the reflecting galvanometer. This separation of the galvanometer from the telescope is most inconvenient, but it was absolutely necessary on account of the large moving masses of iron in the observing-room.

The two faces α and β of the pile are so nearly alike, that the resultant current generated by any equal heating of them is exceedingly feeble.

The telescope is first directed so that the star falls between the faces α and β, and allowed to remain thus until the needle is nearly steady at the zero.

The star is then placed alternately upon the faces α and β, and the corresponding readings of the galvanometer taken as soon as the needle appears to have taken up its position, which usually takes place in about

ten minutes. In order to avoid changes of zero, I have always reduced those readings by comparing a reading with star on face α with the mean of two readings with star on β, taken before and after the reading with star on α, or *vice versâ*.

With this precaution I have never met with any anomalous results, although in making the observations I have usually joined over the terminals, without knowing the direction for heat, and have left this undetermined until the completion of the observations. I mention this because the differences in the readings for star on α and star on β in the state in which I use my galvanometer are small.

On many nights, when very slight appearances of cloud prevailed, I have not been able to make any satisfactory observations at all.

The number of divisions over which the spot of light travels on the galvanometer-scale for a given difference of temperature of the faces α and β is of course dependent upon many circumstances, and especially upon the position of the sensitiveness-regulation magnet of the galvanometer.

I have thought it useless, therefore, to publish any results unless obtained upon nights when the state of the galvanometer was eliminated by referring to an independent source of heat. The way in which this has been attempted is as follows:—

After obtaining the differences in the position of the spot of light on galvanometer-scale for star on α and star on β, I remove the pile from the telescope, leaving all its galvanic connexions untouched, and mount the pile so that of the two halves of the face of a Leslie's cube, containing boiling water, each radiates heat upon one face, α or β of the pile, placed at a known distance of about twenty inches from the cube. After some time the deflection of the needle will fall nearly to zero, and become steady enough for observation. A piece of glass, G, is then placed to intercept from β a portion of the heat radiating from one half of the face of the cube, and when the needle has taken up its position, the reading is taken. Next the glass G is placed to intercept a portion of the heat from the face α, and the galvanometer-reading taken, as before, as soon as the needle has assumed its position of rest.

If, then, θ is the mean difference of readings for star on face α and face β, ϕ the mean difference for glass before β and α, C the heating-power of each half of the cube at its distance from the faces of the pile, and p the measure of the absorption of the piece of glass G, then the heating-power of star

$$=\frac{\theta}{\phi}\times \mathrm{C}\times p.$$

The quantity p has been determined by merely comparing the readings of the galvanometer, obtained by cutting off the whole heat from one-half of the cube, with that obtained by intercepting a portion of this heat by the glass G. A considerable number of accordant results gave $p=0{\cdot}725$.

To determine the quantity C, I have proceeded as follows:—

1st. I have placed two very delicate thermometers, one in contact with each face α and β of the pile, along the lines of junction of the plates. The thermometers were separated from each other, and the direct radiation of one on the other prevented by the interposition of a piece of blackened card. The two thermometers, with faces of pile in contact, were then exposed to the radiation of the halves of the face of the cube containing the boiling water. A third delicate thermometer was read for registration of any change in the temperature of the surrounding air. This thermometer was protected from the direct radiation from the cube. The pile, with thermometers in contact, was then placed at different distances from the cube and the thermometer-readings taken. I have usually taken readings at three distances, one at about 23·5 inches, another at 11·9 inches, another at 2·5 inches. From a comparison of these readings with those taken before the heat from the cube fell upon the thermometers, I infer the heating-power of each half of the cube upon the thermometers, with faces of pile in contact. Calling this quantity for one inch of distance H', I find for my cube in its present state, with slightly laquered face, $H'=130°$ F.

2nd. If H denote the corresponding heating-power of each half of the cube upon the faces of the pile α and β, I have found the ratio $H : H'$ as follows:—

The thermometers being placed in contact with the faces of the pile, and the galvanic connexions made, we may be certain that the temperature of the thermometers has been imparted to the faces of the pile when the needle is steady, provided that the current be carried from the thermometers without loss in the nature of increased resistance. I have therefore compared the deviations produced by glass G before the faces β and α with the thermometers in contact and without thermometers in contact with two different amounts of resistance in circuit. Such observations have been considered satisfactory only when the two resistances for thermometers in contact and without thermometers are sensibly equal. This condition can be obtained by making the thermometers touch along the lines of junction of the antimony and bismuth; but the connexion being one of mere contact, there is always danger of failure.

The following observations were made on 1869, Aug. 19:—

1. Without thermometers:
   Resistance $=R+0·003$ B.A. units.
   Mean difference, G before β—G before $\alpha=735$ div.
2. With thermometers in contact:
   Resistance $=R_1+0·003$ B.A. units.
   Mean difference $=698$ div.
3. With thermometers in contact:
   Resistance $=R_1+1·437$ B.A. units.
   Mean differences$=324$ div.

4. **Without thermometers:**

Resistance $R+1{\cdot}437$ B.A. unit.

From (1) and (4) $R=1{\cdot}251$ B.A. unit.

From (2) and (3) $R_1=1{\cdot}239$ B.A. unit.

The resistances are therefore each sensibly equal to $1{\cdot}245$ B.A. unit.

From (1) (2) and (3) (4) we find $\frac{H}{H_1}=1{\cdot}056$.

From the mean of such determinations I find

$$\frac{H}{H_1}=1{\cdot}087.$$

If, therefore, c is the distance of the pile from the cube in inches, we have

$$C=\frac{130^\circ}{c^2}\times 1{\cdot}087.$$

And the heating-power of the star

$$=\frac{130^\circ}{c^2}\times 1{\cdot}087\times 0{\cdot}725\times\frac{\theta}{\phi}.$$

I may mention that the whole area of a face of the small pile may be considered as effective in the focus of the equatoreal.

The following observations have been made and reduced as above:—

1869. Aug. 2.

Observations of Arcturus, altitude about 25°.

$\theta=23$ div.
$\phi=160$ div.
$c=17{\cdot}6$ inches.

Heating-power of star

$$=\frac{130}{(17{\cdot}6)^2}\times 1{\cdot}087\times 0{\cdot}725\,\frac{23}{160}\left(\frac{17}{37{\cdot}5}\right)$$
$$=0^\circ{\cdot}0216 \text{ F.}$$

For the observations ϕ the scale was removed nearer the galvanometer so that the effective radius for these readings was 2×17 inches against $2\times 37{\cdot}5$ inches for the observations of the star.

1869. August 11.

Observations of Arcturus.

$\theta=27$ div.
$\phi=114$ div.
$c=24$ inches.

Effective radius for observations, 32 inches.

Heating-power of Arcturus

$$=\frac{130}{(24)^2}\times 1{\cdot}087\times 0{\cdot}725\times\frac{27}{114}\times\frac{32}{75}$$
$$=0^\circ{\cdot}0180 \text{ F.}$$

The mean result of the observations on these two nights is

$$0^\circ{\cdot}0198 \text{ F.}$$

as a measure of the heating-effect of Arcturus in raising the temperature of the plate of antimony and bismuth when the heat is condensed by the object-glass of 12·75 inches.

If the absorption by the object-glass be considered insensible, the direct effect upon the pile would be

$$0^\circ{\cdot}000000685 \text{ F.}$$

I have not yet determined the coefficient of absorption for the object-glass, but if it be provisionally taken at $\frac{1}{2}$, the direct heating-effect of Arcturus

$$=0^\circ{\cdot}00000137 \text{ F.}$$

The result may be otherwise stated as follows:—That the heat received from Arcturus is sensibly the same as that from the whole face of the cube containing boiling water at 400 yards.

1869. August 14.

Observations of β Lyræ at 8^h 38^m G.M.T.

$$\theta=15 \text{ div.}$$
$$\phi=686 \text{ div.}$$

Heating-power for β Lyræ

$$=\frac{130}{(24)^2}1{\cdot}087\times 0{\cdot}725\ \frac{15}{686}=0{\cdot}0039 \text{ F.}$$

Observations were subsequently made of α Lyræ, but the zero was unsteady; and as the night advanced clouds appeared, and ultimately interrupted the observations.

1869. August 14.

α Lyræ. Star on α — star on $\beta=11$ div.

1869. August 15.

The night was very clear, and the air steady, but completely saturated with moisture, at a temperature of about 52°. The mean of fourteen observations of the difference of reading for α Lyræ on α and β gave only 11 divs. I have no doubt but that the small effect here obtained was due principally to the amount of moisture in the air.

1869. August 25.

Observations of α Lyræ. Night fine.

Mean value of the difference from nine observations was

$$\theta=33 \text{ div.}$$
$$\phi=669 \text{ div.}$$
$$c=24 \text{ inches.}$$

$$\therefore \text{ heating-power of } \alpha \text{ Lyræ}=0^\circ{\cdot}0088 \text{ F.}$$

This result is again so much smaller than those obtained from Arcturus, although the observations of Arcturus were made under more unfavourable circumstances with respect to altitude, that I cannot but regard it as a fact

that the star Arcturus does give us more heat than α Lyræ,—a result probably due to the same cause which gives rise to the difference in colour between these stars, viz. the greater absorption of the red end of the spectrum in the case of α Lyræ than in the case of Arcturus.

I may here mention that on June 25, 1869, I made a direct comparison between Arcturus and α Lyræ. The result gave for the heat received from Arcturus : that from α Lyræ : : 3 : 2; but on account of the observations of α Lyræ having been interrupted by cloud, they were not sufficiently numerous to eliminate mere errors of reading.

From the whole of these observations I think we may conclude that Arcturus gives to us considerably more heat than α Lyræ; that the amount of heat received is diminished very rapidly as the amount of moisture in the air increases; that nearly the whole heat is intercepted by the slightest cloud; that as first approximations, the heat from Arcturus, at an altitude of 25°, at Greenwich is about equal to that from a three-inch cube containing boiling water at a distance of 400 yards.

The heat from α Lyræ at an altitude of 60° is about equal to that from the same cube at a distance of about 600 yards. The form given to the pile appears likely to be useful in many inquiries respecting differences of heating-power.

January 20, 1870.

Dr. WILLIAM ALLEN MILLER, Treasurer and Vice-President, in the Chair.

The Presents received were laid on the Table, and thanks ordered for them, as follows:—

Transactions.

Christiania:—Kongelige Norske Frederiks Universitet. Aarsberetning for Aaret 1868. 8vo. *Christiania* 1869. Index Scholarum, 1869. 4to. *Christiania* 1869. Forhandlinger i Videnskabs-Selskabet i Christiania, Aar 1868. 8vo. *Christiania* 1869. Forhandlinger ved de Skandinaviske Naturforskeres, tiende Möde i Christiania fra den 4[de] til den 10[de] Juli 1868. 8vo. *Christiania* 1869. Nyt Magazin for Naturvidenskaberne. Bind I. Heft 1, 1835 (1869); Bind XVI. 8vo. *Christiania* 1869. Anton Rosing, Biographie, par P. Chr. Asbjörnsen. 8vo. *Christiania* 1869. La Norvège Littéraire, par P. Botten-Hansen. 8vo. *Christiania* 1868. Traité Elémentaire des Fonctions Elliptiques, par O. J. Broch. 8vo. *Christiania* 1867. Quellen zur Geschichte der Taufsymbols und der Glaubensregel von C. P. Caspari. II. 8vo. *Christiania* 1869. Micrometric Examination of Stellar Cluster in Perseus, by O. A. L. Phil. 4to. *Christiania* 1869. Skolevœsenets Ordning i Massachusetts, af H. Rissen. 8vo. *Christiania* 1868. Le Glacier de Boium en Juillet

Transactions (*continued*).

1868, par S. A. Sexe. 4to. *Christiania* 1869. Anatomisk Beskrivelse af Bursæ Mucosæ, af A. S. D. Synnestvedt. 4to. *Christiania* 1869. The University.

Norske Meteorologiske Institut. Norsk Meteorologisk Aarbog for 1868, 2den Aargang. 4to. *Christiania* 1869. The Institute.

Coimbra:—Universidade. Annuario 1869–70. 12mo. *Coimbra* 1869. The University.

Dublin:—Royal Dublin Society. Journal. No. 37. 8vo. *Dublin* 1868. The Society.

Royal Geological Society of Ireland. Journal. Vol. II. Part 1. 8vo. *Dublin* 1868. The Society.

Edinburgh:—Royal Scottish Society of Arts. Transactions. Vol. VIII. Part 1. 8vo. *Edinburgh* 1869. The Society.

Emden:—Naturforschende Gesellschaft. Jahresberichte 1858–68, 44–54. 8vo. *Emden* 1859–69. The Society.

Haarlem:—Société Hollandaise des Sciences. Archives Néerlandaises des Sciences Exactes et Naturelles. Tome IV. 8vo. *La Haye* 1869. The Society.

London:—Royal Asiatic Society. Journal. New series. Vol. IV. Part 1. 8vo. *London* 1869. The Society.

Throndhjem:—Kongelige Norske Videnskabers Selskabs-Skrifter i det 19de Aarhundrede. Bind V. Heft 2. 8vo. *Throndhjem* 1865–68. The Society.

Vienna:—K. K. Sternwarte. Annalen, von C. von Littrow. Dritter Folge. Band XV. Jahrgang 1865. 8vo. *Wien* 1869. The Observatory.

Galton (Douglas, F.R.S.) An Address on the general principles which should be observed in the Construction of Hospitals. 8vo. *London* 1869. The Author.

Palombo (E.) Della Proprietà e degli Ordinamenti Sociali, studi storico-economici. 8vo. *Napoli* 1869. The Author.

Townsend (R., F.R.S.) On the Nodal Cones of Quadrinodal Cubics, and the Zomal Conics of Tetrazomal Quartics. 8vo. *London* 1869. The Author.

The following communications were read:—

I. "On the Mechanical Performance of Logical Inference." By W. Stanley Jevons, M.A. (Lond.), Professor of Logic &c. in Owens College. Communicated by Professor E. Roscoe. Received October 16, 1869.

(Abstract.)

It is remarkable that from the earliest times mechanical assistance has been employed in mathematical computation. The use of pebbles, of the

fingers, and of the *abacus* of the Greeks and Romans may be adduced as examples. Mathematicians have constantly delighted in devising mechanical modes of calculations, as in the case of Napier's bones, mechanical globes, slide rules, &c. Actual machines for performing difficult calculations have been designed or constructed at various times since the early part of the 17th century, by Pascal, Morland, Leibnitz, Gersten, Babbage, and Scheutz.

In logic, on the contrary, we meet with a total absence of any actual mechanism, although logical works abound with expressions implying the need of such aid. The name of Aristotle's logical treatises, the 'Organon,' or *Instrument*, and many definitions of logic, clearly express this idea, which is also distinctly stated by Bacon in the second aphorism of his 'New Organon.'

This inability of logicians to realize their notions of a mechanical logic in a material form, analogous to the many kinds of calculating machines, can only be explained by the extreme incompleteness of their doctrines. It is the advance of logical science, chiefly due to the late Dr. Boole, Prof. De Morgan, and George Bentham, which now enables us to produce a truly mechanical logic.

Boole, in his celebrated work on the 'Laws of Thought,' first put forth the problem of logical science in its complete generality:—*Given certain logical premises or conditions, to determine the description of any class of objects under those conditions.* The ancient forms of logical deductions are but a few isolated cases of this general problem, which Boole solved in a complete but exceedingly obscure manner. In my 'Pure Logic' (London, 1864, Stanford) and my 'Substitution of Similars' (London, 1869, Macmillan), I have endeavoured to show that the mysterious mathematical form of Boole's logical system is altogether superfluous, and that in one point of great importance he was deeply mistaken. His logical views, when simplified and corrected, give us a method of indirect deduction of extreme generality and power, founded directly upon this most fundamental Law of Thought. A proof of the truthfulness and power of this system is to be found in the fact that it can be embodied in a machine just as the Calculus of Differences is embodied in Mr. Babbage's calculating machine.

To explain the nature of the logical machine alluded to, it may be pointed out that the third of the fundamental Laws of Thought allow us to affirm of any object one or the other of two contradictory attributes, and that we are thus enabled to develope a series of alternatives which must contain the description of a given class or object. Thus, if we are considering the propositions—

Iron is metal,
Metal is element,

we can at once affirm of iron that it is included among the four alternatives:—

Metal, element,
Metal, not element.
Not metal, element.
Not metal, not element.

But according to the second Law of Thought, nothing can combine contradictory attributes, and this law prevents us from supposing that *iron* can be *not metal*, while the first premise affirms that it is *metal*. The second premise again prevents our supposing that the combination *metal, not element* can exist. Hence the only combination of properties which the premises allow us to affirm of *iron* is *metal, element*. In a similar manner a complete solution of any logical problem may be effected by forming the complete list of combination, in which the terms of the problem can manifest themselves, and then striking out such of the combinations as cannot exist in consistency with the conditions of the problem.

The logical machine actually constructed represents the combination, 16 in number, of four positive terms, denoted by A, B, C, D, and their corresponding negatives, *a, b, c, d*. The instrument is provided with eight keys, representing these terms when appearing in the subject of a proposition, with eight keys, placed to the right hand of the former, representing the terms when occurring in the predicate of a proposition, and with the certain *operation* keys denoting the *copular* of the proposition, the *full stop* at the end of it, and the conjunction *or*, according as it occurs in the eulycet or tredicate. There is also a key denoting the *finis* or end of an argument, which has the effect of obliterating any previous impressions, and making the machine a *tabula nasa*. If, now, each of the letter terms A, B, C, D be taken to represent some logical term or noun, and propositions concerning them be, as it were, played upon the machine, as upon a telegraphic instrument, the machine effects thereby such a classification and selection of certain rods representing the 16 possible combinations of the terms, that only those combinations consistent with the propositions remain indicated by the machine at the end of the operations. When once a series of propositions is thus impressed upon the machine, it is capable of exhibiting an answer to any question which may be put to it concerning the possible combinations which form any class.

The machine thus embodies almost all the powers of Boole's logical system up to problems involving four distinct terms, and to represent problems of any complexity involving any number of terms only requires the multiplication of the parts of the machine. The construction involves no mechanical difficulties, and depends upon a peculiar arrangement of pins and levers, which it would not be easy to explain without drawings. In this arrangement of the parts the conditions of correct thinking are observed; the representative rods are just as numerous as the laws of thought require, and no rod represents inconsistent attributes. The representative rods are classified, selected, or rejected by the reading of a proposition in a manner exactly answering to that in which a reasoning

mind should treat its ideas, and at every step in the progress of a problem the machine indicates the proper condition of a mind exempt from mistake.

It is believed that this logical machine may be usefully employed in the logical class-room to exhibit the complete analysis of any argument or logical problem; and it is superior for this purpose to a more rudimentary contrivance, the logical abacus, constructed by me for the same purpose and previously described. But by far the chief importance of the machine is in a theoretical point of view as demonstrating, in the simplest and most evident manner, the character and powers of a universal system of logical deduction, of which the first, although obscure solution, was given by Dr. Boole.

II. "Preliminary Paper on certain Drifting Motions of the Stars." By Richard A. Proctor, B.A., F.R.A.S. Communicated by Warren De La Rue, V.P.R.S. Received October 26, 1869.

A careful examination of the proper motions of all the fixed stars in the catalogues published by Messrs. Main and Stone (Memoirs of the Royal Astronomical Society, vols. xxviii. and xxxiii.) has led me to a somewhat interesting result. I find that in parts of the heavens the stars exhibit a well-marked tendency to drift in a definite direction. In the catalogues of proper motions, owing to the way in which the stars are arranged, this tendency is masked; but when the proper motions are indicated in maps, by affixing to each star a small arrow whose length and direction indicate the magnitude and direction of the star's proper motion, the star-drift (as the phenomenon may be termed) becomes very evident.

It is worthy of notice that Mädler, having been led by certain considerations to examine the neighbourhood of the Pleiades for traces of a community of proper motion, founded on the drift he actually found in Taurus his well-known theory that Alcyone (the *lucida* of the Pleiades) is the common centre around which the sidereal system is moving. But in reality the community of motion in Taurus is only a single instance, and not the most striking that might be pointed out, of a characteristic which may be recognized in many regions of the heavens. In Gemini and Cancer there is a much more striking drift towards the south-east, the drift in Taurus being towards the south-west. In the constellation Leo there is also a well-marked drift, in this case towards Cancer.

These particular instances of star-drift are not the less remarkable, that they (the stars) are drifting almost exactly in the direction due to the proper motion which has been assigned to the sun, because the recent researches of the Astronomer Royal have abundantly proved that the apparent proper motions of the stars are not to be recognized as principally due to the sun's motion. Mr. Stone has shown even that we must assign to the stars a larger proper motion, on the average, than that which the sun possesses.

Looking, therefore, on the stars as severally in motion, with velocities exceeding the sun's on the average, it cannot but be looked upon as highly significant that in any large region of the heavens there should be a community of motion such as I have described. We seem compelled to look upon the stars which exhibit such community of motion as forming a distinct system, the members of which are associated indeed with the galactic system, but are much more intimately related to each other.

In other parts of the heavens, however, there are instances of a star-drift opposed to the direction due to the solar motion. A remarkable instance may be recognized among the seven bright stars of Ursa Major. Of these, the stars β, γ, δ, ϵ, and ζ are all drifting in the same direction, and almost exactly at the same rate, towards the "apex of the solar motion," that is, the point from which all the motions due to the sun's translation in space should be directed. If these five stars, indeed, form a system (and I can see no other reasonable explanation of so singular a community of motion), the mind is lost in contemplating the immensity of the periods which the revolutions of the components of the system must occupy. Mädler had already assigned to the revolution of Alcor around Mizar (ζ Ursæ) a period of more than 7000 years. But if these stars, which appear so clear to the naked eye, have a period of such length, what must be the cyclic periods of stars which cover a range of several degrees upon the heavens?

In like manner the stars α, β, and γ of Arietis appear to form a single system, though the motion of α is not absolutely coincident either in magnitude or direction with that of β and γ, which are moving on absolutely parallel lines with equal velocity.

There are many other interesting cases of the same kind. I hope soon to be able to lay before the Society a pair of maps in which all the well-recognized proper motions in both hemispheres are exhibited on the stereographic projection. In the same maps also the effects due to the solar motion are exhibited by means of great circles through the apex of the solar motion, and small circles or parallels having that apex for a pole.

It appears to me that the star-drift I have described serves to explain several phenomena which had hitherto been thought very perplexing. In the first place, it accounts for the small effect which the correction due to the solar motion has been found to have in diminishing the sums of the squares of the stellar proper motions. Again, it explains the fact that many double stars which have a common proper motion appear to have no motion of revolution around each other; for clearly two members of a drifting-system might appear to form a close double, and yet be in reality far apart and travelling not around each other, but more closely around the centre of gravity of the much larger system they form part of.

I may add that, while mapping the proper motions of the stars, I have been led to notice that the rich cluster around χ Persei falls almost exactly on the intersection of the Milky Way with the great circle which

may be termed the equator of the solar motion; that is, the great circle having the apex of the sun's motion as a pole. This circumstance points to that remarkable cluster, rather than to the Pleiades, as the centre of the sidereal system, if, indeed, that system have a centre cognizable by us. When we remember that for every fixed star in the Pleiades there are hundreds in the great cluster in Perseus, the latter will seem the worthier region to be the centre of motion. I should be disposed, however, to regard the cluster in Perseus as the centre of a portion of the sidereal system, rather than as the common centre of the Galaxy.

The peculiarities of the apparent proper motions of the stars seem to me to lend a new interest to the researches which Mr. Huggins is preparing to make into the stellar proper motions of recess or approach.

III. "On Jacobi's Theorem respecting the relative Equilibrium of a Revolving Ellipsoid of Fluid; and on Ivory's Discussion of the Theorem." By I. TODHUNTER, M.A., F.R.S., late Fellow of St. John's College, Cambridge. Received November 23, 1869.

(Abstract.)

Jacobi discovered the theorem that a fluid ellipsoid revolving with uniform angular velocity round its least axis might be in equilibrium. Ivory discussed the theorem, and made several statements regarding the limitations of the proportions of the axis. Ivory's statements contain various errors and truths based on erroneous reasoning. The object of the present memoir is to correct Ivory's errors, to supply his imperfections, and to add something to what is already known respecting the theorem.

January 27, 1870.

ARCHIBALD SMITH, M.A., Vice-President, in the Chair.

Professor Wyville Thomson was admitted into the Society.

The Presents received were laid on the Table, and thanks ordered for them, as follows:—

Transactions:—

Cambridge, Mass.:—Museum of Comparative Zoology. Bulletin. Nos. 8–13. 8vo. *Cambridge* 1869. The Museum.

Copenhagen:—Kongelige Danske Videnskabernes Selskab. Skrifter, Nat. og Math. Afd. Bd. VIII. 1, 3, 4, 5. 4to. *Kjöbenhavn* 1868–69. Oversigt, 1868, No. 5; 1869, No. 2. 8vo. *Kjöbenhavn* 1868–69. The Society.

Transactions (*continued*).

Frankfurt a. M.:—Senckenbergische Naturforschende Gesellschaft. Abhandlungen. Band VII. Hefte 1–2. 4to. *Frankfurt* 1869. Bericht von Juni 1868 bis Juni 1869. 8vo. The Society.

Leipzig:—Astronomische Gesellschaft. Vierteljahrsschrift. IV. Jahrgang. Hefte 2–3. 8vo. *Leipzig* 1869. Publication IX. (Tafeln der Pomona.) 4to. *Leipzig* 1869. The Society.

Neuchâtel:—Société des Sciences Naturelles. Bulletin. Tome VIII. cahier 1. 8vo. *Neuchâtel* 1868. The Society.

Philadelphia:—Franklin Institute. Journal. Third Series. Vol. LVIII. Nos. 1–6. 8vo. *Philadelphia* 1869. The Institute.

Agassiz (L., For. Mem. R.S.) Report upon Deep-Sea Dredgings. 8vo. *Cambridge* [*Mass.*]. Address delivered on the Centennial Anniversary of the birth of Alexander von Humboldt, under the auspices of the Boston Society of Natural History. 8vo. *Boston* 1869. The Author.

Campani (G.) Azione del Permanganato di Potassio sull Asparagina. 8vo. *Siena* 1869. The Author.

Clark (F. Le Gros) Lectures on the Principles of Surgical Diagnosis, especially in relation to Shock and Visceral Lesions. 8vo. *London* 1870. The Author.

Joly (N.) Haute Antiquité du Genre Humain. 8vo. *Toulouse* 1869. The Author.

Pole (Dr., F.R.S.) On Probabilities as illustrated by events occurring in Games with Cards. 12mo. *London* 1869. The Author.

Realis (S.) Note sur le Nombre *e*. 8vo. *Paris* 1869. The Author.

Rütimeyer (L.) Ueber Thal- und See-Bildung. 4to. *Basel* 1869. The Author.

Stainton (H. T., F.R.S.) The Tineina of Southern Europe. 8vo. *London* 1869. The Entomologist's Annual for 1870. 12mo. *London* 1870. The Author.

Sundby (Thor) Brunetto Latinos Levnet og Skrifter. 8vo. *Kjöbenhavn* 1869. The Author.

Catalogue of Books added to the Library of Congress, from Dec. 1, 1867 to Dec. 1, 1868. roy. 8vo. *Washington* 1869. The Library.

Statistics of New Zealand for 1868. fol. *Wellington* 1869. The Registrar-General of New Zealand.

Archivio per la Zoologia, l'Anatomia e la Fisiologia, pubblicato per cura dei Proff. S. Richiardi e G. Canestrini. Serie 2, Vol. I. 8vo. *Torino* 1869. The Editors.

Der Zoologische Garten. X. Jahrgang, No. 1–12. 8vo. *Frankfurt a. M.* 1869. The Zoological Society of Frankfort.

Melbourne:—Flagstaff Observatory. Meteorological Observations, 1857–58. 2 vols. sm. fol. Daily Meteorological Register, 1859–1863. 5 vols. fol. Original Observations on Atmospheric Electricity, 1858. sm. fol. Daily Electrical Register, 1860–63. 3 vols. 4to. Magnetical Observations, 1st May to 31st Dec. 1858. sm. fol. Remarks and Disturbance Observations during the year 1859. Daily Magnetical Register, 1860–62, and Jan. to Feb. 1863. 3 vols. sm. fol. MSS. Presented by the Observer, Dr. Neumayer.

The following communications were read:—

I. "Observations on the Temperature of the Strata taken during the sinking of the Rose Bridge Colliery, Wigan, Lancashire, 1868–69." By Edward Hull, M.A., F.R.S., Director of the Geological Survey of Ireland. Received November 27, 1869.

In an elaborate paper by Mr. W. Hopkins, F.R.S., entitled "Experimental Researches on the Conductive Powers of various Substances," published in the Philosophical Transactions for 1857, an account is given of a series of experiments made under the general supervision of Mr. Hopkins himself and Mr. W. Fairbairn, F.R.S., during the sinking of the Astley Pit of Dukenfield Colliery in Cheshire*. At the time this paper was written the depth attained was only a little more than 1400 feet; and the rate of increase between the depths of 700 feet and 1330 feet was found to be 1° F. for about 65 feet. These observations were subsequently continued until the pits had attained their full depth of 717 yards from the surface. The last observation made was in the shale overlying the coal-seam, known as the "Black Mine," which it was the object of the proprietor, Mr. Astley, to reach, and the temperature was found to be 75° F. Assuming the "stratum of constant temperature," or, as it is also called by Humboldt, "the invariable stratum," to be that which was reached at 16·5 feet with a temperature of 51° F., the total increase of temperature would amount to 24° F., giving as the rate of increase 1° F. for every 88·925 feet. This is much below the average rate of increase.

During a part of the period above referred to (from 1854–56) another coal-pit was being sunk at Wigan, which reached the depth of 600 yards, down to the celebrated "Cannel Mine." At this pit similar observations on the temperature of the strata were made very carefully by the manager, Mr. Bryham, which were kindly communicated to myself for publication, and will be found in my work on the 'Coalfields of Great Britain.' The ultimate temperature attained in this pit at the depth from the surface of 600 yards was found to be 72° F.; and assuming the invariable stratum to be the same as that at Dukenfield Colliery, the resulting rate of increase would be 1° F. for every 61·5 feet, which accords very closely with the

* The entire series of these interesting observations were kindly supplied to me by Mr. W. Fairbairn, and are published in 'The Coalfields of Great Britain,' 2nd edit. p. 226.

result obtained by Professor Phillips, F.R.S., at the Monkwearmouth Colliery.

Since the time above referred to, the proprietor of the Rose Bridge Colliery, Mr. J. Grant Morris, determined to carry down the shafts from the "Cannel" seam to the "Arley" seam of coal, which was known to lie more than 200 yards below it; and consequently in the spring of 1868 preparations were commenced for carrying out this project. In the incredibly short time of one year and two months the Arley coal was struck, and was found to be of good thickness and quality. The total depth reached was 808 yards, and the ultimate temperature in the coal itself was found to be 93½° F. The manager of the colliery, Mr. Bryham, sensible of the value of observations on the temperature of the strata at such unusual depths (this being probably the deepest colliery in the world, certainly in Britain), made a series of observations with as much care as the circumstances would admit, and has entrusted them to me for publication.

The mode of taking the observations was as follows:—On a favourable stratum, such as shale, or even coal, having been reached, a hole was drilled with water in the solid strata to a depth of one yard from the bottom of the pit. A thermometer was then inserted, the hole having been sealed and made airtight with clay. At the expiration of half an hour the thermometer was taken up and the reading noted.

It might possibly be objected that the time allowed (thirty minutes) was insufficient for the imbedding of the thermometer, and that the readings are liable to error from this cause. I feel sure, however, that if any error has arisen it is inappreciable, and does not in the least invalidate the general result. In fact I am assured by Mr. Bryham that, from actual testing on several occasions, he found less than this time of thirty minutes sufficient for the purpose required.

While the temperatures of the strata were being measured, observations were also carried on *pari passu* on those of the open pit during the descent. These are given in the Table annexed. By a comparison of the results in the two columns, it will be observed that as the depth increased the differences between the corresponding temperatures in the pit and the strata tended to augment; in other words, the temperature of the strata was found to augment more rapidly than that of the open pit.

The effects of the high temperature and pressure on the strata at the depth of 2425 feet are, as I am informed by Mr. Bryham, making themselves felt, and cause an increase in the expense both of labour and timber for props. This colliery, in fact, will be in a position to put to the test our views and speculations on the effects of high temperature and pressure on mining operations.

In order to obtain the average rate of increase of heat, as shown by the experiments at Rose Bridge Colliery, we may assume (in the absence of direct observation) the position and temperature of the *invariable stratum* to be 50 feet from the surface and 50° F., which is probably nearly the

mean temperature of the place. With these data, the increase is 1° F. for every 54·57 feet, which approximates to that obtained by Professor Phillips at Monkwearmouth of 1° F. for about every 60 feet.

If, on the other hand, for the purpose of comparison, we adopt the measurements for the *invariable stratum* as obtained at Dukenfield, we find the rate of increase to be 1° F. for every 47·2 feet as against 1° F. for every 83·2 feet in the case of Dukenfield itself. So great a discordance in the results is remarkable, and is not, in my opinion, attributable to inaccuracy of observation in making the experiments. On the other hand, I may venture to suggest that it is due, at least in some measure, to dissimilarity in the position and inclination of the strata in each case. These I now proceed to point out.

Position of the Strata at Rose Bridge and Dukenfield Collieries.—Rose Bridge Colliery occupies a position in the centre of a gently sloping trough, where the beds are nearly horizontal; they are terminated both on the west and east by large parallel faults which throw up the strata on either side. The colliery is placed in what is known as "the deep belt."

Dukenfield Colliery, on the other hand, is planted upon strata which are highly inclined. The beds of sandstone, shale, and coal rise and crop out to the eastward at angles varying from 30° to 35°. Now I think we may assume that strata consisting of sandstones, shales, clays, and coal alternating with each other are capable of conducting heat more rapidly along the planes of bedding than across them, different kinds of rock having, as Mr. Hopkins's experiments show, different couducting-powers. If this be so, we have an evident reason for the dissimilar results in the two cases before us. Assuming a constant supply of heat from the interior of the earth, it could only escape, in the case of Rose Bridge, *across* the planes of bedding, meeting in its progress upwards the resistance offered by strata of, in each case, varying conducting-powers. On the other hand, in the case of Dukenfield the internal heat could travel along the steeply inclined strata themselves, and ultimately escape along the outcrop of the beds.

I merely offer this as a suggestion explanatory of the results before us, and may be allowed to add that the strata at Monkwearmouth Colliery, the thermometrical observations at which correspond so closely with those obtained at Rose Bridge, are also in a position not much removed from the horizontal, which is some evidence in corroboration of the views here offered.

Thermometrical Observations at Rose Bridge Colliery.

Date.	Depth, in yards.	Strata.	Temperature in open pit.	Temperature in solid strata.
			° F.	° F.
July 1854	161	Blue shale	......	64·5
August 1854	188	Warrant earth	......	66
May 1858	550	Blue shale	......	78
July 1858	600	Warrant earth	......	80
May 18, 1868	630	"Raven" coal	73	83
July 24, 1868	665	Linn and wool	75	85
April 19, 1869	673	"Yard Coal" mine	76	86
November 18, 1868	700	Strong blue metal	76	87
February 22, 1869	736	Do.	76	88½
March 12, 1869	748	Shale	77	89
April 17, 1869	762	Linn and wool, or strong shale	78	90·5
May 3, 1869	774	Strong shale	80	91·5
May 19, 1869	782	Blue metal	79	92
July 8, 1869	801	Strong blue shale	79	93
July 16, 1869	808	Coal (Arley mine)	79	93½

Remarks.

All holes vertical in solid at bottom of pit drilled with water 1 yard deep, and thermometer remained in hole thirty minutes and made airtight with clay.

II. "On the Action of Rays of high Refrangibility upon Gaseous Matter." By John Tyndall, LL.D., F.R.S., Professor of Natural Philosophy in the Royal Institution. Received December 4, 1869.

This paper is an expansion of the Researches already communicated to the Royal Society on the Chemical Action of Light on Gaseous Matter. (See Proceedings, vol. xvii. p. 92.)

III. "On the Theory of Continuous Beams." By John Mortimer Heppel, Mem. Inst. C.E. Communicated by Prof. W. J. Macquorn Rankine. Received December 9, 1869.

(Abstract.)

The chief object of the present communication is to remedy some acknowledged defects in the theory of the above-mentioned subject. The principal steps by which it has reached its present state of development are also noticed, and may be briefly recapitulated as follows:—

In 1825 M. Navier investigated the conditions of a straight continuous beam resting on any number of supports. His method, though perfectly correct for the assumed conditions (which embraced most cases occurring

in practice), was so exceedingly intricate when the number of openings became at all large, that in such instances it was of little practical use.

In 1849 M. Clapeyron, a distinguished engineer and savant, devised a much more direct and easy means of treating such cases, though he did not at first succeed in giving to his own method all the simplicity and elegance of which it was capable.

This was first done in 1856 by M. Bertot, civil engineer, who, by effecting an elimination which had escaped Clapeyron, arrived at a remarkable equation which has been the key to all subsequent treatment of the subject. This equation involves the bending moments over any three consecutive points of support, and is well known in France by the name of the "Theorem of the three Moments."

In 1857 M. Clapeyron himself and M. Bresse, Professeur de Mécanique appliquée à l'École Impériale des Ponts et Chaussées, appear to have discovered this theorem independently of M. Bertot, and M. Bresse shortly afterwards extended it to a much greater degree of generality.

M. Bresse's researches on this subject are published in the third volume of his 'Cours de Mécanique appliquée,' Paris, 1865; but they had been communicated by him to the Academy of Sciences in 1862, and fully completed in the previous year. M. Bresse not only contributed to the advancement of the theory, but entered largely into the best methods of its application to practice, and framed rules which have since, under an Imperial Commission, acquired the character of legislative enactments.

M. Bélanger, Professeur de Mécanique appliquée à l'École centrale, appears, about the same time as M. Bresse, to have made an independent investigation of this subject, and to have brought the theory of it to about the same stage of advancement.

Little has been since added to this theory in France, but valuable contributions to its development in reference to practice are to be found in the works of MM. Renaudot, Albaret, Molinos et Pronnier, Colignon, and Piarron de Mondesir.

In England Professor Moseley is the first writer on mechanics who appears to have occupied himself with this subject. In his work on 'The Mechanical Principles of Engineering and Architecture,' he gives several examples of the application of M. Navier's method to important practical cases. This work was published in 1843, and no doubt furnished the groundwork for Mr. Pole's more extended investigations.

In 1852 Mr. Pole had to examine the case of the bridge over the Trent at Torksey, involving some new conditions not treated by Moseley, but which he found the means of treating with perfect success. About the same time Mr. Pole had to deal with the much more complex and important case of the Britannia bridge, in which, besides variation of load from one span to another, variation of section also had to be considered, and imperfect continuity over the middle pier. These conditions were successfully imported into this method of Navier, which was, however,

only known to Mr. Pole through the examples of its application given in Moseley's work, and the results obtained were identical with those which would have followed from the application of the method of Clapeyron in its most improved and generalized form.

In 1858, the present writer, being then in India, had occasion to consider the condition of a continuous girder of five spans, and finding the method of Navier unmanageable, was forced to seek for some other. He first came upon the equation which he afterwards found had been for some years known in France as the "Theorem of the three Moments," and afterwards extended it, so as to take in all the conditions of the Britannia bridge and to verify all Mr. Pole's results. In this form it was absolutely identical with the equation given by M. Bélanger, and nearly so with that of M. Bresse.

The great defect in all this theory up to the present time has been that, in order to avoid an inextricable complexity, it has been necessary to consider the load in each span as uniformly distributed over it, and the moment of inertia of the section as uniform throughout each span.

In many cases these hypotheses are false, notably so in the case of the Britannia; and the conclusions are affected by their falsity, to what extent being a matter of uncertainty, though good grounds have been shown for believing that the errors cannot attain to importance.

The method now given treats these conditions, it is hoped, rigorously; and although the equations obtained are such as necessarily require some laborious computation to obtain numerical results, they are certainly by no means inextricable.

It is satisfactory to find that in the case of the Britannia, where these new conditions enter with much greater force than in most cases, their effect on the resulting stresses is very unimportant; so that the inference may legitimately be drawn that in all ordinary cases the method of Bresse may be confidently applied.

It is scarcely possible in a short abstract to give an idea of an analytical investigation. The equations obtained are of the same form as those of the previous methods, each containing, as unknown quantities, the bending moments over three consecutive supports; but the coefficients are somewhat involved functions of the varying loads and sections. An abbreviated functional notation has, wherever possible, been used, by means of which a certain degree of clearness and symmetry is preserved in expressions which would otherwise become inextricably complex.

IV. "Remarks on Mr. Heppel's Theory of Continuous Beams." By W. J. Macquorn Rankine, C.E., LL.D., F.R.S. Received December 22, 1869.

(Abstract.)

The author states that the advantages possessed by Mr. Heppel's method will probably cause it to be used both in practice and in scientific study.

With a view to the instruction of students in engineering science, he proposes an abridged way of stating the theoretical principles of Mr. Heppel's method, considering at the same time that Mr. Heppel's more detailed investigation forms the best model for numerical calculation.

He then uses Mr. Heppel's improved form of the "Theorem of the three Moments" to test the accuracy of the formulæ which he obtained in another way, and published in 'A Manual of Civil Engineering,' for the case of a uniform continuous beam with an indefinite number of equal spans, the successive spans being loaded alternately with a uniform fixed load only, and with a uniform travelling load in addition to the fixed load; and he finds the results of the two methods to agree in every respect.

V. "Remarks on the recent Eclipse of the Sun as observed in the United States." By J. N. Lockyer, F.R.S. Received December 7, 1869.

By the kindness of Professors Winlock, Morton, and Newton, I have been favoured with photographs, and as yet unpublished accounts, of the results of the recent total eclipse of the sun observed in America. I am anxious, therefore, to take the opportunity afforded by the subject being under discussion, to lay a few remarks thus early before the Royal Society.

The points which I hoped might be more especially elucidated by this eclipse were as follows:—

1. Is it possible to differentiate between the chromosphere and the corona?

2. What is the real photographic evidence of the structure of the base of the chromosphere in reference to Mr. W. De La Rue's enlarged photographs of the eclipse of 1860?

3. What is the amount of the obliterating effect of the illumination of our atmosphere on the spectrum of the chromosphere?

4. Is there any cooler hydrogen above the prominences?

5. Can the spectroscope settle the nature of the corona during eclipses?

With regard to 1, the evidence is conclusive. The chromosphere, including a "radiance," as it has been termed by Dr. Gould (the edge of the radiance as photographed being strangely like the edge of the chromosphere in places viewed with the open slit), is not to be confounded with the corona.

On this subject, in a letter to Professor Morton, Dr. B. A. Gould writes:—"An examination of the beautiful photographs made at Burlington and Ottumwa by the sections of your party in charge of Professors Mayer and Haines, and a comparison of them with my sketches of the corona, have led me to the conviction that the radiance around the moon in the pictures made during totality is not the corona at all, but is actually the image of what Lockyer has called the chromosphere.

"This interesting fact is indicated by many different considerations. The directions of maximum radiance do not coincide with those of the great beams of the corona; they remain constant, while the latter were variable.

There is a diameter approximately corresponding to the solar axis, near the extremities of which the radiance upon the photographs is a minimum, whereas the coronal beams in these directions were especially marked during a great part of the total obscuration. The coronal beams stood in no apparent relation to the protuberances, whereas the aureole seen upon the photographs is most marked in their immediate vicinity; indeed the great protuberance, at 230° to 245°, seems to have formed a southern limit to the radiance on the western side, while a sharp northern limit is seen on all the photographs at about 350°, the intermediate arc being thickly studded with protuberances which the moon displayed at the close of totality. The exquisite masses of flocculent light on the following limbs are upon the two sides of that curious prominence at 93°, which at first resembled an ear of corn, as you have said, but which, in the later pictures after it had been more occulted, and its southern branch thus rendered more conspicuous, was like a pair of antelope's horns, to which some observers compare it. Whatever of this aureole is shown upon the photographs was occulted or displayed by the lunar motion, precisely as the protuberances were. The variations in the form of the corona, on the other hand, did not seem to be dependent in any degree upon the moon's motion. The singular and elegant structural indication in the special aggregations of light on the eastern side may be of high value in guiding to a further knowledge of the chromosphere. They are manifest in all the photographs by your parties which I have seen, but are especially marked in those of shortest exposure, such as the first one at Ottumwa. In some of the later views they may be detected on the other side of the sun, though less distinct; but the very irregular and jagged outline of the chromosphere, as described by Janssen and Lockyer, is exhibited in perfection."

The second point is also referred to in the same letter. I think the American photographs afford evidence that certain appearances in parts of Mr. De La Rue's photographs, which represent the chromosphere as billowy on its under side, are really due to some action either of the moon's surface or of a possible rare lunar atmosphere; so that it is not desirable to confound these effects with others that might be due to a possible suspension of the chromosphere in a transparent atmosphere, if only a *section* of the chromosphere were photographed.

Dr. Gould writes:—"You will observe that some of the brighter, petal-like flocculi of light have produced apparent indentations in the moon's limb at their base, like those at the bases of the protuberances. These indentations are evidently due to specular reflection from the moon's surface, as I stated to the American Association at Salem last month. Had any doubt existed in my mind previously, it would have been removed by an inspection of the photographs."

Where the chromosphere is so uniform a light that the actinic effect on the plate is pretty nearly equal, the base of the chromosphere is absolutely

continuous in the American photographs; but in the case of some of the larger prominences, notably those at +146 (Young) and −130 (Young), there are strong apparent indents on the moon's limb.

I next come to the obliterating effect of the illumination of our atmosphere on the spectrum of the chromosphere.

This is considerable; in fact the evidences of it are very much stronger than one could have wished, but hardly more decided than I had anticipated. Professor Winlock's evidence on this point, in a letter to myself, is as follows:—"I examined the principal protuberance before, during, and after totality. I saw three lines (C, near D and F) before and after totality and eleven during totality; *eight were instantly extinguished on the first appearance of sunlight.*"

This effect was observed with two flint prisms and 7 inches aperture. Professor Young, with five prisms of 45° and 4 inches aperture, found the same result in the part of the spectrum he was examining at the end of the totality.

He writes: "I had just completed the measurements of 2602, when the totality ended. *This line disappeared instantly,* but 2796 [the hydrogen line near G] was nearly a minute in resuming its usual faintness." These observations I consider among the most important ones made during the eclipse; for they show most unmistakably that, as I have already reported to the Secretary of the Government-Grant Committee, the new method to be employed under the best conditions must be used with large apertures and large dispersion.

On the fourth point the evidence is but negative only, and therefore in favour of the view I have some time ago communicated to the Royal Society.

We next come to the question of the corona,—a question which has been made more difficult than ever (in appearance only I think) by the American observations.

I propose to discuss only the spectroscopic observations of Professors Young and Pickering in connexion with Dr. Gould's before quoted remarks.

Professor Pickering, with an ordinary chemical spectroscope merely directed to the sun's place during totality, obtained the combined spectrum of the protuberances and corona. He saw a continuous spectrum with two or three bright lines, one "near E," and a second "near C."

Professor Young, who used a spectroscope specially adapted for the work, in which only one part of the prominence at +146° was being examined, saw C, near D, a line at 1250 ± 20, and another at 1350 ± 20 of Kirchhoff's scale. The rest of the observations I give in his own words.

"Then came the 1474 K line, which was very bright, though by no means equal to C and D_3; but attention was immediately arrested by the fact that, unlike them, it extended clean across the spectrum; and on moving the slit away from the protuberances, it persisted, while D_3, visibly in the

edge of the field, disappeared. Thus it was evident that this line* belonged not to the spectrum of the protuberance, but to that of the corona. My impression, but I do not feel at all sure of it, is that the two faint lines between it and D_3 behaved in the same manner, and are also corona lines†.

"I am confirmed in this opinion by Professor Pickering's observation. He used a single-prism spectroscope, with the slit of the collimator simply directed to the sun, and having no lens in front of it. With this arrangement he saw only three or four bright lines, the brightest near E (1474). Now this is exactly what ought to occur if that line really belongs to the corona, which, from its great extent, furnished to his instrument a far greater quantity of light than the prominences.

"By this time the moon had advanced so far that it became necessary to shift the slit to the great prominence on the opposite side of the sun. While my assistant was doing this, I suppose I must, in the excitement of the moment, have run my eye-piece over the region of the magnesium lines (b), and thrown them out of the field before he had brought anything upon the slit. At any rate I saw nothing of these lines, which were evident enough to several other observers, and can think of no other way to account for their having escaped me. The F line in the spectrum of the great protuberance was absolutely glorious, broad at the base and tapering upwards, crookedly as Lockyer has before often observed. Next appeared a new line, about as bright as 1474 at 2602=2 of Kirchhoff's scale. Its position was carefully determined by micrometrical reference to the next line, 2796 K (hydrogen γ), which was very bright; h was also seen, very clear, but hardly brilliant. In all I saw nine bright lines.

* "On two or three occasions previously I had been very much surprised at not being able to detect this line in the spectrum of unusually bright prominences. On the other hand, I once found it very easy to see at a place on the sun's limb where the other chromosphere lines, usually far more brilliant, were almost invisible."

† "A careful examination of the photographs, especially No. 2 of the Burlington totality pictures, somewhat diminishes my confidence in the conclusion of the text as to the nature of these three lines (1250, 1350, and 1474). They certainly do not belong to the spectrum of the most brilliant portion of the prominences; but around the prominences of the eastern limb, on which the slit of the spectroscope was directed during the first half of the totality, the photograph shows a pretty extensive and well-defined nebulosity, evidently distinct from, though associated with, the brilliant nuclei. Now it is possible that these lines may belong to this nebulosity, and not to the corona proper; for I cannot recall with certainty whether 1474 retained its brilliance at any considerable distance from the prominences, or only in their immediate neighbourhood. My strong impression, however, is that the former was the case, and that the text is correct. I may as well confess that my uncertain memory here is due to the fact that just at this time, while my assistant was handing me the lantern with which to read the micrometer-head, I looked over my shoulder for an instant, and beheld the most beautiful and impressive spectacle upon which my eyes have ever rested. It could not have been for five seconds; but the effect was so overwhelming as to drive away all certain recollection of what I have just seen. What I have recorded I recall from my notes taken down by my assistant."

"A faint continuous spectrum, without any traces of dark lines in it, was also visible, evidently due to the corona. Its light, tested by a tourmaline applied next to the eye, proved to be very strongly polarized in a plane passing through the centre of the sun. I am not sure, however, but that this polarization, as suggested by Prof. Pickering, may have been produced by the successive refractions through the prisms. This explanation at once removes the difficulty otherwise arising from the absence of dark lines."

I have first to do with the continuous spectrum, deduced from Professor Pickering's observations.

I think in such a method of observation, even if the corona were terrestrial and gave a dark line spectrum, the lines visible with such a dim light would in great part be obliterated by the corresponding bright lines given out by the long arc of chromosphere visible, to say nothing of the prominences, in which it would be strange if C, D, E, *b*, F, and many other lines were not reversed. This suggestion, I think, is strengthened by the statement that two bright lines were seen "near C" and "near E;" should we not rather read (for the "near" shows that we are only dealing with approximations) C and F, which is exactly what we might expect.

But even this is not all that may be hazarded on the subject of the continuous spectrum, which was also seen by Professor Young under different conditions.

Assuming the corona to be an atmospheric effect merely, as I have before asserted it to be, it seems to me that its spectrum should be continuous, or nearly so; for is it not as much due to the light of the prominences as to the light of the photosphere, which, it may be said roughly, are complementary to each other?

With regard to the aurora theory, I gather from Professor Young's note that, if not already withdrawn, he is anxious to wait till the next eclipse for further facts. I consider that the fact that I often see the line at 1474, and often do not, is fatal to it, as it should be constantly visible on the proposed hypothesis. The observation of iron-vapour, as I hold it to be at this elevation, is of extreme value, coupled with its simple spectrum, *seen during an eclipse*, as it entirely confirms my observations made at a lower level in the case not only of iron but of magnesium.

February 3, 1870.

Lieut.-General Sir EDWARD SABINE, K.C.B., President, in the Chair.

Among the Presents received was a Thermometer, presented by Mr. Augustus De Morgan, which had been made in Florence in the seventeenth century. It was one of a collection discovered in the Museo Fisico of

Florence in 1829, which had belonged to the Accademia del Cimento, and corresponds with the small thermometer used for determining atmospheric temperatures, of which a figure and description are given in the Memoirs of the Academy.

The following communications were read:—

I. "Note on an Extension of the Comparison of Magnetic Disturbances with Magnetic Effects inferred from observed Terrestrial Galvanic Currents; and Discussion of the Magnetic Effects inferred from Galvanic Currents on days of tranquil magnetism." By GEORGE BIDDELL AIRY, Astronomer Royal. Received December 22, 1869.

(Abstract.)

The author, after referring to his paper in the Philosophical Transactions for 1868 on the comparison of Magnetic Disturbances inferred from Galvanic Currents recorded by the Self-registering Galvanometers of the Royal Observatory of Greenwich with the Magnetic Disturbances registered by the Magnetometers, on 17 days, states that he had now undertaken the examination of the whole of the Galvanic Currents recorded during the establishment of the Croydon and Dartford wires (from 1865 April 1 to 1867 October 24). The days of observation were divided into three groups,—No. 1 containing days of considerable magnetic disturbance, and therein including not only the 17 days above mentioned, but also 36 additional days, No. 2 containing days of moderate disturbance, of which no further use was made, and No. 3 containing the days of tranquil magnetism.

The comparisons of the additional 36 disturbed days were made in the same manner as those of the preceding 17 days, and the inferences were the same. The results were shown in the same manner, by comparison of curves, which were exhibited to the Society. The points most worthy of notice are, that the general agreement of the strong irregularities, Galvanic and Magnetic, is very close, that the galvanic irregularities usually precede the magnetic, in time, and that the northerly magnetic force appears to be increased. The author remarks that no records appeared open to doubt as regards instrumental error, except those of western declination; and to remove this he had compared the Greenwich Curves with the Kew Curves, and had found them absolutely identical.

The author then proceeds with the discussion of the Galvanic Current-Curves on days of tranquil magnetism, not by way of comparison with the magnetic curves, but for independent examination of the galvanic laws. The method was explained of measuring the ordinates and connecting the measures into expressions for magnetic action, at every hour, and group-

ing the measures at the same nominal hour by months, and taking their monthly means for each hour. As these exhibited sensible discordance, they were smoothed by taking the means of adjacent numbers, taking the means of the adjacent numbers of the new series, and so on, repeating the operation six times. The author explains the theory of this process, and the way in which it tends to degrade the periodical terms of higher orders. He then explains an easy method of resolving the numbers so smoothed into periodical terms recurring once in the day, twice in the day, thrice in the day, &c., and applies the method to the numbers for every month.

When these quantities (which from month to month are perfectly independent) are brought together in tables, they present such an agreement, with gradual change accompanying the change of seasons, as leaves no doubt on their representation of a real law of the diurnal changes of the galvanic currents. They also show the existence of a constant turn towards the north (which explains the apparent increase of force to the north observed in the results for days of great disturbance), and a still larger force towards the west (which also is well marked on the days of great disturbance). No light is obtained on the origin of these terms, but they appear to be probably pure galvanic accidents, depending on the nature of the earth-connexions.

The author then exhibits in curves the diurnal inequalities of magnetism which the galvanic currents must produce. The form generally consists of two parallel lobes, making with the magnetic meridian an angle of nearly 60° from the north towards the west. The greatest east-and-west difference of ordinates, in the month of April, is 0·00044 of Total Horizontal Magnetic Force; it corresponds, in the hours to which those ordinates relate, nearly with the Ordinary Diurnal Inequality. But it is much smaller than the ordinary diurnal inequality, and the daily law of the galvano-magnetic inequality differs greatly from that of diurnal inequality. For the greater part, therefore, of diurnal inequality the cause is yet to be found.

II. "Monthly Magnetic Determinations, from December 1866 to May 1869 inclusive, made at the University of Coimbra." By Professor J. A. de Souza, Director of the Observatory. Communicated by Balfour Stewart, F.R.S. Received December 16, 1869.

[Note.—These observations form the continuation of a series the results of which were communicated to the Royal Society on May 8, 1867, by the President. In both series the same instruments were used, and the method of observation was the same in both.—B. S.]

Year and month.	Observations of Deflection and Vibration for absolute measure of H. F.										Magnetic Dip.				Value.		
	Day, hour, and minutes.	Distances.	Temperature, Fahr.	Deflection.	Log $\frac{m}{X}$.	Day, hour, and minutes.	Temperature, Fahr.	Time of one vibration.	Log mX.	Value of m.	Day, hour, and minutes.	Azimuth.	Needle.	Dip.	X.	Y.	Total force.
	d h m	foot	°	° ′ ″		d h m	°	s			d h m			° ′ ″			
1866. Dec.	1 0 44	1·0	62·0	11 6 40·6	8·98635	1 1 15	65·3	4·23460	0·39466	0·4903	2 0 42	......	1	61 25 51	5·0606	9·2772	10·5677
		1·3		5 2 19·4	8·98616				0·39466				2	61 20 43			
	5 0 42	1·0	66·0	11 5 32·5	8·98592	5 0 28	66·8	4·23445	0·39492	0·4902	6 0 24	......	1	61 16 15	5·0646	9·2278	10·5263
		1·3		5 1 45·0	8·98573				0·39492				2	61 12 35			
	10 0 34	1·0	64·1	11 5 31·2	8·98576	10 0 49	63·6	4·23436	0·39485	0·4902	11 1 58	......	1	61 17 26	5·0638	9·2439	10·5400
		1·3		5 2 3·7	8·98603				0·39485				2	61 16 54			
	20 0 49	1·0	62·6	11 4 53·7	8·98525	20 1 6	64·8	4·23483	0·39467	0·4897	21 0 22	......	1	61 14 51	5·0670	9·2371	10·5356
		1·3		5 1 28·7	8·98509				0·39467				2	61 15 32			
1867. Jan.	1 0 36	1·0	54·3	11 5 21·2	8·98492	1 0 56	54·0	4·23434	0·39411	0·4892	2 1 29	......	1	61 12 28	5·0653	9·2114	10·5123
		1·3		5 1 45·0	8·98486				0·39411				2	61 10 49			
	15 0 53	1·0	51·5	11 6 8·7	8·98524	15 1 15	51·9	4·23434	0·39399	0·4892	16 1 18	......	1	61 20 10	5·0637	9·2576	10·5519
		1·3		5 1 52·5	8·98484				0·39399				2	61 18 30			
	25 1 23	1·0	62·0	11 4 35·0	8·98500	25 1 6	61·0	4·23498	0·39474	0·4895	26 1 6	......	1	61 15 17	5·0696	9·2402	10·5396
		1·3		5 1 10·0	8·98459				0·39474				2	61 14 36			
Feb.	1 0 18	1·0	56·8	11 4 5·0	8·98423	1 0 27	57·2	4·23491	0·39398	0·4889	2 1 13	......	1	61 13 4	5·0672	9·2130	10·5145
		1·3		5 1 27·5	8·98463				0·39398				2	61 9 37			
	10 0 36	1·0	65·4	11 3 23·7	8·98449	10 1 2	66·5	4·23545	0·39485	0·4894	11 0 30	......	1	61 16 21	5·0723	9·2366	10·5377
		1·3		5 0 50·0	8·98437				0·39485				2	61 10 49			
	20 0 45	1·0	65·7	11 2 50·0	8·98415	20 0 48	66·2	4·23519	0·39482	0·4892	21 1 20	......	1	61 15 7	5·0741	9·2116	10·5110
		1·3		5 0 36·2	8·98406				0·39483				2	61 9 52			
											Means			61 15 20	5·0668	9·2356	10·5337

Year and month.	Observations of Deflection and Vibration for absolute measure of H. F.										Magnetic Dip.				Values of		
	Day, hour, and minutes.	Distances.	Temperature, Fahr.	Deflection.	$\text{Log}\frac{m}{X}$.	Day, hour, and minutes.	Temperature, Fahr.	Time of one vibration.	Log mX.	Value of m.	Day, hour, and minutes.	Azimuth.	Needle.	Dip.	X.	Y.	Total force.
	d h m	foot	°	° ′ ″		d h m	°	s			d h m			° ′ ″			
1867. Mar.	1 0 42	1·0	63·0	11 3 3·7	8·98409	1 0 57	64·5	4·23514	0·39462	0·4891	2 1 36	......	1	61 13 59	5·0727	9·2213	10·5245
		1·3		5 0 50·0	8·98419				0·39463				2	61 8 10			
	10 0 45	1·0	64·6	11 2 49·4	8·98405	10 1 3	66·5	4·23555	0·39468	0·4890	11 1 30	......	1	61 15 0	5·0741	9·2511	10·5513
		1·3		5 0 31·9	8·98387				0·39468				2	61 15 42			
	20 1 29	1·0	60·8	11 3 31·2	8·98422	20 1 24	60·9	4·23533	0·39451	0·4892	21 1 44	......	1	61 14 34	5·0726	9·2347	10·5360
		1·3		5 0 47·5	8·98396				0·39451				2	61 11 49			
April	1 1 12	1·0	63·9	11 1 28·7	8·98313	1 1 16	64·9	4·23562	0·39454	0·4885	2 1 17	......	1	61 12 36	5·0773	9·2410	10·5439
		1·3		5 0 15·0	8·98341				0·39454				2	61 13 6			
	10 0 29	1·0	69·5	11 0 11·2	8·98272	10 0 35	70·6	4·23592	0·39483	0·4884	11 1 17	......	1	61 11 7	5·0762	9·2428	10·5450
		1·3		4 59 31·2	8·98278				0·39483				2	61 15 48			
	20 0 40	1·0	69·3	11 0 36·2	8·98297	20 0 47	68·9	4·23591	0·39488	0·4885	21 0 42	......	1	61 10 49	5·0819	9·2326	10·5388
		1·3		4 59 27·5	8·98268				0·39488				2	61 9 37			
May	1 0 40	1·0	69·2	10 59 46·2	8·98242	1 0 43	69·3	4·23577	0·39489	0·4883	2 1 55	......	1	61 11 34	5·0837	9·2431	10·5489
		1·3		4 59 26·2	8·98264				0·39489				2	61 11 9			
	10 1 3	1·0	66·5	11 0 27·5	8·98267	10 1 14	68·1	4·23601	0·39468	0·4883	11 1 27	......	1	61 10 15	5·0817	9·2078	10·5170
		1·3		4 59 35·0	8·98265				0·39468				2	61 2 30			
	20 1 14	1·0	70·1	10 59 43·7	8·98247	20 0 38	71·7	4·23636	0·39488	0·4883	21 1 55	......	1	61 15 54	5·0842	9·2509	10·5560
		1·3		4 59 13·7	8·98241				0·39489				2	61 8 58			
											Means			61 11 49	5·0783	9·2361	10·5401

Observations of Deflection and Vibration for absolute measure of H. F.										Declination.		Magnetic Dip.			Values of		
Day, hour, and minutes. 1867.	Distances.	Temperature, Fahr.	Deflection.	Log $\frac{m}{X}$.	Day, hour, and minutes.	Temperature, Fahr.	Time of one vibration.	Log *m*X.	Value of *m*.	Coimbra mean time.	West Declination.	Day, hour, and minutes.	Needle.	Dip.	X.	Y.	Total force.
June d h m	foot	°	° ′ ″		d h m	°				d h m	° ′ ″	d h m		° ′ ″			
1 1 15	1·0	76·3	10 59 7·5	8·98256	1 1 26	77·5	4·23658	0·39529	0·4885	......	......	2 2 2	1	61 13 52	5·0863	9·2948	10·5955
	1·3		4 58 53·7	8·98241				0·39529					2	61 23 32			
10 11 37	1·0	80·4	10 57 37·5	8·98189	10 11 14	82·4	4·23656	0·39548	0·4883	......	......	11 1 55	1	61 11 19	5·0907	9·2601	10·5671
	1·3		4 58 21·2	8·98194				0·39548					2	61 12 43			
20 0 52	1·0	73·3	10 58 50·0	8·98213	20 1 0	74·2	4·23669	0·39497	0·4881	......	......	21 2 12	1	61 12 26	5·0867	9·2548	10·5601
	1·3		4 58 48·7	8·98205				0·39497					2	61 12 13			
July 1 0 50	1·0	79·5	10 57 51·2	8·98198	1 1 8	80·2	4·23642	0·39541	0·4882	......	......	2 1 51	1	61 10 30	5·0908	9·2463	10·5551
	1·3		4 58 13·1	8·98168				0·39541					2	61 9 9			
10 1 15	1·0	79·4	10 57 48·7	8·98195	10 1 13	80·9	4·23693	0·39548	0·4883	17 1 12	20 53 7	11 1 14	1	61 11 10	5·0906	9·2530	10·5609
	1·3		4 58 23·1	8·98192				0·39548					2	61 10 42			
20 1 12	1·0	81·6	10 56 16·2	8·98111	20 1 27	83·4	4·23589	0·39579	0·4880	30 5 4	20 54 52	21 1 40	1	61 12 52	5·0979	9·2849	10·5923
	1·3		4 57 35·0	8·98092				0·39580					2	61 14 49			
Aug. 1 0 45	1·0	76·6	10 54 30·0	8·97956	1 1 5	77·7	4·23785	0·39488	0·4867	3 4 23	20 51 42	2 2 21	1	61 12 13	5·1007	9·2659	10·5771
	1·3		4 57 1·2	8·97968				0·39488					2	61 7 55			
10 1 9	1·0	79·5	10 50 53·7	8·97742	10 0 36	82·2	4·23883	0·39498	0·4855	4 1 2	20 54 48	11 1 45	1	61 9 54	5·1144	9·2882	10·6032
	1·3		4 55 16·2	8·97737				0·39498					2	61 9 28			
20 1 28	1·0	79·7	10 50 48·7	8·97738	20 2 0	81·7	4·23841	0·39515	0·4855	22 10 35	20 39 29	21 1 22	1	61 10 54	5·1158	9·3023	10·6162
	1·3		4 55 11·2	8·97727				0·39515					2	61 12 4			
										Means	20 50 48	......	...	61 12 6	5·0971	9·2722	10·5808

Observations of Deflection and Vibration for absolute measure of H. F.										Declination.		Magnetic Dip.			Values of		
Day, hour, and minutes. 1867.	Distances.	Temperature, Fahr.	Deflection.	$\log \frac{m}{X}$.	Day, hour, and minutes.	Temperature, Fahr.	Time of one vibration.	Log mX.	Value of m.	Coimbra mean time.	West declination.	Day, hour, and minutes.	Needle.	Dip.	X.	Y.	Total force.
Sept. d h m	foot	°	° ′ ″		d h m	°	s			d h m	° ′ ″	d h m		° ′ ″			
1 0 50	1·0	80·1	10 51 37·5	8·97795	1 0 50	81·4	4·23856	0·39499	0·4858	3 4 10	20 39 43	2 2 47	1	61 12 36	5·1117	9·2858	10·5998
	1·3		4 55 30·0	8·97776				0·39499					2	61 7 30			
10 1 15	1·0	78·1	10 49 55·6	8·97667	10 1 40	79·9	4·23975	0·39460	0·4848	......	......	11 1 7	1	61 13 26	5·1175	9·3092	10·6231
	1·3		4 54 36·9	8·97630				0·39460					2	61 10 43			
21 1 9	1·0	80·8	10 51 30·0	8·97791	21 1 19	82·2	4·23898	0·39497	0·4858	20 10 57	20 42 49	22 1 20	1	61 14 28	5·1107	9·3196	10·6289
	1·3		4 55 42·0	8·97810				0·39497					2	61 16 45			
Oct. 2 0 57	1·0	82·8	10 50 36·2	8·97748	2 0 58	83·8	4·23904	0·39495	0·4856	1 11 31	20 5 17	4 1 59	1	61 15 58	5·1124	9·3097	10·6211
	1·3		4 55 27·5	8·97790				0·39495					2	61 11 13			
11 0 52	1·0	73·9	10 51 16·2	8·97723	11 0 42	75·0	4·23867	0·39463	0·4852	10 0 39	20 47 8	12 0 53	1	61 11 51	5·1129	9·2939	10·6075
	1·3		4 55 32·5	8·97734				0·39463					2	61 10 6			
21 1 10	1·0	69·8	10 53 7·5	8·97813	21 1 27	71·4	4·23883	0·39437	0·4856	20 0 51	21 0 16	22 1 37	1	61 11 51	5·1063	9·2867	10·5980
	1·3		4 56 21·2	8·97820				0·39437					2	61 11 39			
Nov. 1 0 57	1·0	66·5	10 52 44·4	8·97763	1 1 0	66·9	4·23841	0·39429	0·4853	2 0 56	20 52 17	3 2 44	1	61 13 51	5·1087	9·2930	10·6047
	1·3		4 56 10·0	8·97768		66·9		0·39429					2	61 10 13			
11 2 17	1·0	71·8	10 53 30·0	8·97853	11 2 17	72·0	4·23729	0·39448	0·4859	10 0 42	20 53 44	12 1 34	1	61 10 7	5·1040	9·2681	10·5805
	1·3		4 56 38·7	8·97879				0·39449					2	61 8 51			
21 1 10	1·0	62·2	10 54 5·0	8·97818	21 1 27	61·6	4·23883	0·39417	0·4855	......	......	22 1 47	1	61 12 56	5·1054	9·2844	10·5956
	1·3		4 56 38·7	8·97806				0·39417					2	61 10 21			
										Means	20 43 2	......	...	61 11 55	5·1099	9·2945	10·6066

Observations of Deflection and Vibration for absolute measure of H. F.										Declination.		Magnetic Dip.			Values of		
Day, hour, and minutes. 1867.	Distances.	Temperature, Fahr.	Deflection.	$\text{Log}\,\frac{m}{X}$.	Day, hour, and minutes.	Temperature, Fahr.	Time of one vibration.	Log *m*X.	Value of *m*.	Coimbra mean time.	West declination.	Day, hour, and minutes.	Needle.	Dip.	X.	Y.	Total force.
Dec. d h m	foot	°	° ′ ″		d h m	°	s			d h m	° ′ ″	d h m		° ′ ″			
1 0 50	1·0	58·2	10 55 31·2	8·97883	1 0 35	58·2	4·23769	0·39393	0·4857	2 2 41	20 55 28	3 2 16	1	61 11 21	5·1001	9·2642	10·5752
	1·3		4 57 18·7	8·97873				0·39393					2	61 8 37			
12 1 4	1·0	54·3	10 55 13·1	8·97834	12 1 9	55·7	4·23700	0·39363	0·4853	11 11 12	20 53 5	13 1 49	1	61 9 26	5·1007	9·2431	10·5571
	1·3		4 57 16·2	8·97838				0·39363					2	61 3 33			
20 0 12	1·0	53·0	10 55 9·4	8·97821	20 0 18	52·2	4·23687	0·39371	0·4852	21 2 29	20 48 11	22 1 37	1	61 11 0	5·1024	9·2596	10·5722
	1·3		4 57 8·7	8·97811				0·39371					2	61 6 13			
1868. Jan.																	
3 1 11	1·0	48·6	10 55 37·5	8·97820	3 1 1	49·0	4·23693	0·39311	0·4853	2 11 7	20 48 9	4 1 36	1	61 10 48	5·0943	9·2537	10·5633
	1·3		4 58 26·2	8·97967				0·39311					2	61 9 11			
10 0 56	1·0	58·1	10 53 53·7	8·97776	10 0 46	59·2	4·23708	0·39386	0·4851	12 10 48	20 46 14	11 1 37	1	61 10 13	5·1054	9·2604	10·5745
	1·3		4 56 41·2	8·97781				0·39386					2	61 5 31			
21 1 27	1·0	55·7	10 54 41·9	8·97810	21 0 57	56·4	4·23728	0·39378	0·4852	......	......	22 1 31	1	61 9 17	5·1032	9·2607	10·5737
	1·3		4 57 0·6	8·97810				0·39378					2	61 7 49			
Feb.																	
1 0 48	1·0	62·4	10 53 43·7	8·97796	1 0 47	62·4	4·23734	0·39415	0·4853	3 2 54	20 44 8	2 11 56	1	61 10 46	5·1061	9·2767	10·5891
	1·3		4 56 35·0	8·97798				0·39415					2	61 9 43			
12 1 19	1·0	63·0	10 53 52·5	8·97811	12 1 0	61·4	4·23725	0·39405	0·4854	11 2 27	20 57 10	13 1 49	1	61 9 37	5·1048	9·2613	10·5750
	1·3		4 56 36·2	8·97806				0·39405					2	61 6 45			
21 0 33	1·0	59·8	10 56 10·0	8·97937	21 0 24	59·6	4·23770	0·39373	0·4856	20 0 0	20 35 53	22 1 47	1	61 13 21	5·0988	9·2684	10·5783
	1·3		4 56 51·2	8·97819				0·39373					2	61 8 39			
										Means	20 48 32	......	...	61 8 59	5·1017	9·2609	10·5731

Observations of Deflection and Vibration for absolute measure of H. F.										Declination.		Magnetic Dip.			Values of		
Day, hour, and minutes. 1868.	Distances.	Temperature, Fahr.	Deflection.	$\text{Log}\frac{m}{X}$.	Day, hour, and minutes.	Temperature, Fahr.	Time of one vibration.	Log mX.	Value of m.	Coimbra mean time.	West declination.	Day, hour, and minutes.	Needle.	Dip.	X.	Y.	Total force.
Mar. d h m	foot	°	° ′ ″		d h m	°	s			d h m	° ′ ″	d h m		° ′ ″			
4 1 13	1·0	66·9	10 52 41·2	8·97762	4 1 34	68·7	4·23812	0·39417	0·4852	1 0 0	20 52 16	2 0 57	1	61 11 9	5·1081	9·2715	10·5855
	1·3		4 56 8·7	8·97768				0·39417					2	61 6 33			
12 0 59	1·0	59·2	10 53 24·4	8·97752	12 1 7	60·8	4·23782	0·39376	0·4848	10 11 0	20 45 46	11 1 19	1	61 10 0	5·1071	9·2620	10·5767
	1·3		4 56 16·2	8·97729				0·39376					2	61 5 17			
22 0 52	1·0	66·3	10 53 11·2	8·97791	22 0 35	66·1	4·23809	0·39413	0·4852	21 11 0	20 49 7	20 1 54	1	61 15 56	5·1076	9·2946	10·6056
	1·3		4 56 1·2	8·97748				0·39413					2	61 9 19			
April																	
2 0 41	1·0	66·4	10 53 33·7	8·97816	2 0 28	66·5	4·23861	0·39402	0·4856	5 11 0	20 37 9	1 1 37	1	61 11 6	5·1016	9·2678	10·5792
	1·3		4 57 6·2	8·97904				0·39403					2	61 9 9			
13 0 55	1·0	70·5	10 51 17·5	8·97698	13 0 47	70·8	4·23807	0·39440	0·4849	11 11 0	20 46 14	10 1 42	1	61 9 0	5·1133	9·2698	10·5866
	1·3		4 55 30·0	8·97701				0·39440					2	61 5 13			
21 1 3	1·0	63·7	10 53 28·7	8·97790	21 0 48	64·1	4·23840	0·39387	0·4861	27 11 0	20 45 58	20 1 30	1	61 10 28	5·0945	9·2476	10·5581
	1·3		4 58 53·7	8·98144				0·39388					2	61 7 28			
May																	
3 1 16	1·0	76·3	10 50 40·0	8·97706	3 1 15	79·0	4·23864	0·39471	0·4851	1 11 0	20 40 7	2 1 45	1	61 9 52	5·1125	9·2779	10·5932
	1·3		4 55 2·5	8·97683				0·39471					2	61 7 21			
12 1 44	1·0	68·9	10 51 41·2	8·97712	12 1 49	71·6	4·23832	0·39428	0·4849	11 11 0	19 49 39	10 1 46	1	61 8 56	5·1122	9·2707	10·5868
	1·3		4 55 35·0	8·97701				0·39428					2	61 6 11			
21 1 7	1·0	73·8	10 51 5·0	8·97710	21 1 15	74·9	4·23866	0·39452	0·4851	24 11 0	20 43 52	20 1 9	1	61 12 15	5·1134	9·2805	10·5960
	1·3		4 55 22·5	8·97708				0·39452					2	61 5 15			
										Means	20 38 54	......	...	61 8 55	5·1078	9·2714	10·5853

Observations of Deflection and Vibration for absolute measure of H. F.										Declination.		Magnetic Dip.			Values of		
Day, hour, and minutes. 1868.	Distances.	Temperature, Fahr.	Deflection.	Log $\frac{m}{X}$	Day, hour, and minutes.	Temperature, Fahr.	Time of one vibration.	Log mX.	Value of m.	Coimbra mean time.	West declination.	Day, hour, and minutes.	Needle.	Dip.	X.	Y.	Total force.
June d h m	foot	°	° ′ ″		d h m	°	s			d h m	° ′ ″	d h m		° ′ ″			
2 1 33	1·0	78·3	10 49 38·7	8·97650	2 1 9	80·0	4·23852	0·39491	0·4849	3 11 0	20 57 51	1 10 58	1	61 6 9	5·1194	9·2670	10·5870
	1·3		4 54 41·2	8·97642				0·39491		5 11 0	20 53 33		2	61 3 43			
12 0 59	1·0	82·9	10 48 57·5	8·97640	12 0 43	84·6	4·23894	0·39502	0·4850	11 11 0	20 51 34	10 1 21	1	61 8 24	5·1204	9·2802	10·5991
	1·3		4 54 26·2	8·97641				0·39502					2	61 5 2			
21 0 57	1·0	76·7	10 50 43·7	8·97709	21 0 44	81·4	4·23878	0·39467	0·4849	24 11 0	20 45 33	20 1 15	1	61 8 32	5·1173	9·2734	10·5916
	1·3		4 54 36·2	8·97608				0·39467					2	61 4 32			
July																	
4 1 14	1·0	77·5	10 48 38·7	8·97577	4 1 13	78·1	4·23860	0·39485	0·4845	......	......	2 1 19	1	61 6 36	5·1236	9·2803	10·6007
	1·3		4 54 10·6	8·97561				0·39485					2	61 5 4			
12 0 55	1·0	77·6	10 50 23·7	8·97694	12 0 37	78·4	4·23944	0·39479	0·4851	......	......	10 1 19	1	61 5 9	5·1159	9·2624	10·5814
	1·3		4 55 5·0	8·97695				0·39479					2	61 5 17			
22 0 32	1·0	82·1	10 47 48·7	8·97558	22 0 29	82·4	4·23924	0·39507	0·4845	23 11 0	20 45 34	20 1 27	1	61 8 6	5·1258	9·2911	10·6113
	1·3		4 53 52·5	8·97552				0·39507					2	61 5 43			
Aug.																	
4 0 39	1·0	81·7	10 47 23·7	8·97527	4 1 27	83·1	4·23918	0·39495	0·4844	3 11 0	20 49 20	2 1 9	1	61 6 19	5·1255	9·2807	10·6020
	1·3		4 54 0·0	8·97567				0·39495					2	61 4 22			
11 0 43	1·0	82·2	10 45 56·2	8·97435	11 0 45	82·0	4·23960	0·39498	0·4838	14 11 0	20 46 48	10 1 4	1	61 6 33	5·1321	9·3191	10·6388
	1·3		4 53 6·2	8·97440				0·39498					2	61 12 24			
24 11 44	1·0	77·7	10 46 48·7	8·97457	24 11 23	77·9	4·23987	0·39467	0·4838	20 11 0	20 41 49	21 1 40	1	61 9 37	5·1287	9·3051	10·6249
	1·3		4 53 35·0	8·97475				0·39468					2	61 6 54			
										Means	20 49 0	......	...	61 6 35	5·1232	9·2844	10·6041

Observations of Deflection and Vibration for absolute measure of H. F.										Declination.		Magnetic Dip.			Values of		
Day, hour, and minutes. 1868.	Distances.	Temperature, Fahr.	Deflection.	$\log \frac{m}{X}$.	Day, hour, and minutes.	Temperature, Fahr.	Time of one vibration.	Log *m*X.	Value of *m*.	Coimbra mean time.	West declination.	Day, hour, and minutes.	Needle.	Dip.	X.	Y.	Total force.
Sept. d h m	foot	°	° ′ ″		d h m	°	s			d h m	° ′ ″	d h m		° ′ ″			
2 0 34	1·0	90·3	10 45 28·7	8·97470	2 0 32	92·5	4·24049	0·39516	0·4841	1 11 0	20 35 21	3 11 17	1	61 10 0	5·1311	9·3126	10·6326
	1·3		4 52 55·0	8·97478				0·39516					2	61 7 31			
12 0 16	1·0	76·8	10 45 45·0	8·97380	12 0 20	77·5	4·23985	0·39444	0·4832	......	......	11 1 23	1	61 6 37	5·1324	9·2978	10·6203
	1·3		4 52 58·7	8·97379				0·39444					2	61 5 30			
22 11 55	1·0	67·8	10 47 21·2	8·97417	22 11 57	67·5	4·23982	0·39392	0·4831	......	......	20 1 26	1	61 11 2	5·1276	9·3105	10·6291
	1·3		4 53 36·2	8·97401				0·39392					2	61 7 49			
Oct.																	
1 0 41	1·0	63·8	10 47 42·5	8·97410	1 0 37	64·8	4·23951	0·39372	0·4830	2 11 0	20 41 57	3 1 39	1	61 12 7	5·1263	9·3150	10·6324
	1·3		4 53 52·5	8·97411				0·39372					2	61 8 51			
13 0 42	1·0	74·8	10 46 40·0	8·97425	13 0 36	76·1	4·24024	0·39428	0·4832	10 11 0	20 43 51	12 1 23	1	61 10 9	5·1305	9·3095	10·6296
	1·3		4 53 0·0	8·97366				0·39428					2	61 6 43			
22 0 35	1·0	64·3	10 46 57·5	8·97364	22 1 15	66·8	4·24007	0·39358	0·4827	20 11 0	20 46 32	21 1 14	1	61 10 44	5·1275	9·3059	10·6250
	1·3		4 53 42·5	8·97390				0·39359					2	61 6 44			
Nov.																	
3 0 20	1·0	68·7	10 45 56·2	8·97330	3 0 28	70·1	4·23973	0·39388	0·4826	1 11 0	20 35 45	2 1 37	1	61 11 56	5·1323	9·3190	10·6388
	1·3		4 53 0·0	8·97319				0·39388					2	61 6 54			
11 1 6	1·0	54·9	10 46 52·2	8·97288	11 1 16	55·4	4·23966	0·39304	0·4820	12 11 0	20 37 50	10 1 29	1	61 11 32	5·1289	9·3080	10·6276
	1·3		4 53 38·7	8·97311				0·39304					2	61 5 49			
23 11 41	1·0	60·6	10 46 41·2	8·97318	23 11 53	61·6	4·24001	0·39348	0·4824	22 2 0	20 54 36	20 1 18	1	61 11 39	5·1296	9·3201	10·6385
	1·3		4 53 35·0	8·97344				0·39349					2	61 9 2			
										Means	20 42 16	......	...	61 8 55	5·1296	9·3109	10·6304

Observations of Deflection and Vibration for absolute measure of H. F.										Declination.		Magnetic Dip.			Values of		
Day, hour, and minutes. 1868.	Distances.	Temperature, Fahr.	Deflection.	Log $\frac{m}{X}$	Day, hour, and minutes.	Temperature, Fahr.	Time of one vibration.	Log *m*X.	Value of *m*.	Coimbra mean time.	West declination.	Day, hour, and minutes.	Needle.	Dip.	X.	Y.	Total force.
Dec. d h m	foot	°	° ′ ″		d h m	°	s			d h m	° ′ ″	d h m		° ′ ″			
3 0 31	1·0	61·6	10 45 52·5	8·97272	3 0 27	61·7	4·23984	0·39368	0·48217	......	......	1 1 24	1	61 7 6	5·1343	9·3030	10·6258
	1·3		4 53 2·5	8·97271				0.39368					2	61 5 37			
......	1·0	...	......	......	......	...	......	......	......	13 11 0	20 31 36	10 1 28	1	61 9 24	...	...	......
	1·3		......	......				......					2	61 5 0			
21 0 14	1·0	60·8	10 45 52·5	8·97266	21 0 12	61·8	4·24010	0·39354	0·48217	......	......	20 0 55	1	61 10 47	5·1326	9·3102	10·6312
	1·3		4 53 18·7	8·97306				0·39354					2	61 5 7			
1869. Jan.																	
4 11 27	1·0	54·9	10 46 33·7	8·97268	4 11 36	55·8	4·23988	0·39324	0·48193	1 0 0	20 30 10	2 1 41	1	61 9 22	5·1316	9·3024	10·6240
	1·3		4 53 25·0	8·97278				0·39324					2	61 4 41			
11 0 41	1·0	59·0	10 45 38·7	8·97237	11 0 39	61·3	4·24005	0·39342	0·48188	12 11 0	20 39 57	10 0 47	1	61 7 37	5·1343	9·3019	10·6248
	1·3		4 53 3·7	8·97255				0·39342					2	61 4 45			
21 0 3	1·0	55·3	10 48 35·0	8·97405	21 11 52	55·8	4·24078	0·39300	0·48243	......	......	20 1 20	1	61 15 34	5·1234	9·3336	10·6473
	1·3		4 54 1·2	8·97370				0·39300					2	61 12 51			
Feb.																	
2 0 48	1·0	59·3	10 45 26·2	8·97226	2 0 33	61·1	4·23988	0·39354	0·48185	3 11 0	20 49 0	1 1 25	1	61 7 41	5·1360	9·3052	10·6285
	1·3		4 52 52·5	8·97230				0·39354					2	61 4 45			
......	1·0	...	......	......	......	...	......	......	......	15 11 0	20 43 21	11 1 11	1	61 9 30	...	...	......
	1·3		......	......				......					2	61 7 4			
......	1·0	...	......	......	......	...	......	......	......	......	......	16 1 35	1	61 10 34	...	...	......
	1·3		......	......				......					2	61 4 22			
										Means	20 38 48·8	......	...	61 7 52·6	5·1320	9·3094	10·6303

Observations of Deflection and Vibration for absolute measure of H. F.										Declination.		Magnetic Dip.			Values of		
Day, hour, and minutes. 1869.	Distances.	Temperature, Fahr.	Deflection.	Log $\frac{m}{X}$.	Day, hour, and minutes.	Temperature, Fahr.	Time of one vibration.	Log mX.	Value of m.	Coimbra mean time.	West declination.	Day, hour, and minutes.	Needle.	Dip.	X.	Y.	Total force.
Mar. d h m	foot	°	° ′ ″		d h m	°	s			d h m	° ′ ″	d h m		° ′ ″			
4 0 57	1·0	61·1	10 45 40·0	8·97254	4 0 50	61·3	4·24043	0·39347	0·48189	1 11 0	20 39 20	2 0 44	1	61 8 21	5·1348	9·3048	10·6276
	1·3		4 52 47·5	8·97231				0·39347					2	61 4 37			
12 0 26	1·0	54·8	10 46 47·5	8·97283	12 0 32	55·1	4·23991	0·39312	0·48185	......	......	10 1 38	1	61 10 28	5·1310	9·3139	10·6337
	1·3		4 53 17·5	8·97258				0·39312					2	61 7 30			
......	1·0	...	......	......	......	...	......	......	...	......	......	20 1 40	1	61 7 19	...	...	......
	1·3		......	......				......					2	61 4 24			
April	1·0	...	......	......	......	...	......	......	...	2 11 0	20 36 8	1 1 8	1	61 12 4	...	...	......
	1·3		......	......				......					2	61 5 41			
10 0 22	1·0	68·0	10 44 47·5	8·97248	10 0 18	69·8	4·24074	0·39378	0·48205	......	......	11 1 46	1	61 9 54	5·1367	9·3124	10·6351
	1·3		4 52 27·5	8·97234				0·39378					2	61 4 22			
22 0 21	1·0	69·5	10 45 27·5	8·97242	22 0 34	70·6	4·24038	0·39343	0·48156	24 3 0	20 42 57	20 1 37	1	61 9 20	5·1378	9·3065	10·6305
	1·3		4 52 7·5	8·97133				0·39343					2	61 2 28			
May 4 1 5	1·0	75·0	10 42 40·0	8·97161	4 0 48	76·4	4·24077	0·39425	0·48185	3 3 0	20 34 58	2 1 35	1	61 6 51	5·1444	9·3121	10·6386
	1·3		4 51 33·7	8·97155				0·39425					2	61 3 0			
14 0 49	1·0	64·4	10 45 50·0	8·97289	14 0 26	67·1	4·24105	0·39356	0·48215	......	......	10 1 40	1	61 9 28	5·1330	9·3011	10·6235
	1·3		4 52 55·0	8·97273				0·39356					2	61 3 22			
21 0 27	1·0	67·8	10 43 27·5	8·97158	21 0 22	69·5	4·24029	0·39385	0·48164	......	......	20 1 34	1	61 8 41	5·1419	9·3159	10·6407
	1·3		4 51 58·7	8·97161				0·39385					2	61 3 45			
										Means	20 38 20·7	......	...	61 6 45·3	5·1371	9·3095	10·6328

III. "On the Fossil Mammals of Australia.—Part III. *Diprotodon australis*, Owen." By Prof. Owen, F.R.S. &c. Received December 10, 1869.

(Abstract.)

In this paper the author communicates descriptions, with figures of the fossil remains at his command, of *Diprotodon australis*, which have been received from various localities in Australia, since the first announcement of the genus, founded on a fragment of the lower jaw and tusk, described and figured in the 'Appendix' to Sir Thos. Mitchell's 'Three Expeditions into the Interior of Eastern Australia,' 8vo, 1838.

The fossils in question include the entire cranium and lower jaw with most of the teeth, showing the dental formula of:—$i. \frac{3-3}{1-1}$, $c. \frac{0-0}{0-0}$, $m. \frac{5-5}{5-5} = 28$; portions of jaws and teeth exemplifying characteristics of age and sex; many bones of the trunk and extremities.

After some introductory remarks, the author proceeds to the description of the skull and teeth, which are illustrated by many figures, those of the teeth being of the natural size. The result of the comparisons detailed establishes the marsupial character of *Diprotodon*, and the combination of characters of *Macropus* and *Phascolomys* with special modifications of its own. These latter are more fully and strongly manifested in the bones of the trunk and limbs, subsequently described. The pelvis and femora present resemblances to those in *Proboscidea*, not hitherto observed in any other remains of large extinct quadrupeds of Australia. But in all the bones described essentially marsupial characteristics are more or less determinable. The paper concludes with a summary of the characters of *Diprotodon*, throwing light upon the conditions of its extinction, its analogies with the *Megatherium*, its affinities to existing forms of *Marsupialia*, and the more generalized condition which it manifests of that mammalian type.

A table of the localities in Australia from which remains of *Diprotodon* have been obtained, and a table of the principal admeasurements of the skeleton, are appended to the text.

February 10, 1870.

Lieut.-General Sir EDWARD SABINE, K.C.B., President, in the Chair.

The Right Hon. Lord Napier of Magdala, and M. Charles Eugène Delaunay (Foreign Member) were admitted into the Society.

The following communications were read:—

I. "On some remarkable Spectra of Compounds of Zirconia and the Oxides of Uranium." By H. C. Sorby, F.R.S. Received December 27, 1869.

When a scientific man has been led into an error and afterwards discovers his mistake, I think it a matter of duty that he should take an early opportunity to correct it. I therefore now write the following notice of certain remarkable peculiarities in the spectra of some compounds of the oxides of uranium with zirconia which led both myself and others* to conclude that they were due to a new elementary substance.

Though the spectra of the different salts of those bases which show well-marked absorption-bands often differ in detail, yet they usually resemble each other so much that there is no difficulty in recognizing each particular element. This is so constantly the case in the various compounds of erbium, didymium, and cobalt, and in the ordinary salts of uranium, that for a long time the more I studied this question, the more did it appear to be a general rule, and there seemed to be no reason to suspect that a few special compounds of uranium would give spectra with absorption-bands as unlike as possible those of all others. Such, however, turns out to be the fact, when its oxides are combined with zirconia.

As an excellent illustration of important differences in mere detail, but general correspondence, I would refer to the spectra of didymium in different states of combination†, and would especially refer to the most distinct of the numerous absorption-bands which occurs in the yellow. The various compounds agree in showing this band in the same general position; but by careful management, and by the use of sufficient dispersive power, it may be resolved into a very variable number of narrow bands or black lines. For example, in the case of the crystallized sulphate containing comparatively little lanthanum, it can be resolved into seven narrow lines, two of those near the centre being the darkest, whereas when much lanthanum is present, one line on the side next the green is so much darker than the rest that the others are comparatively absent. On fusing the mixed oxides with borax, the same spectrum is seen as with oxide of didymium alone, and I can resolve the above-named band into only two narrower bands; whereas when the saturated bead is made to deposit crystals by being kept some time at a very dull red heat, this band can easily be resolved into eight equal and very distinct black lines. Although these and similar differences in detail are of much interest, yet in no case are they so considerable as to prevent our recognizing at once that the spectra are all due to didymium. It is also important to notice that the amount requisite to give a most splendid spectrum when the bead is crystalline will scarcely show any trace of bands when it is in a vitreous condition, dissolved in the borax. This is

* Professor Church, 'Chemical News,' vol. xix. p. 121, and Professor Loew, ib. vol. xx. p. 9.

† See also Bunsen's paper, Pogg. Ann. vol. cxxviii. p. 100.

analogous to what occurs in the case of solid and powdered crystals of sulphate of didymium; for the absorption-bands in the spectrum of the light transmitted by a thin layer of the fine powder, strongly illuminated from the other side, are as distinct as in that transmitted by a many times greater thickness of solid and transparent crystal. We may very conveniently take advantage of this fact in studying the spectra of such substances, when the amount of material at command is otherwise too small. This seems to be because the transmitted light does not simply pass through the crystals, but is in great measure reflected from them backwards and forwards, and thus, as it were, passes through a greater thickness. It is also to a considerable extent similar to that reflected from the powder when illuminated from above, as may be clearly proved by what occurs in the case of uranic salts. These when in a state of moderately fine powder transmit light, giving a spectrum showing not only the absorption-bands in the blue, which alone are met with in that transmitted by a clear crystal, but also the bands in the green, which depend on fluorescence, characteristic of that reflected from the powder*. These two kinds of bands can be easily distinguished by means of a plate of deep blue cobalt glass, which has an entirely different action, according as it is placed below or above the object when the bands are due to fluorescence, but has no such effect when they are due to ordinary absorption. It would perhaps be well to mention here that I have in this manner proved that the abnormal bands seen in the spectra of the compounds of zirconia with the oxides of uranium described in this paper are due to genuine absorption, and not to fluorescence.

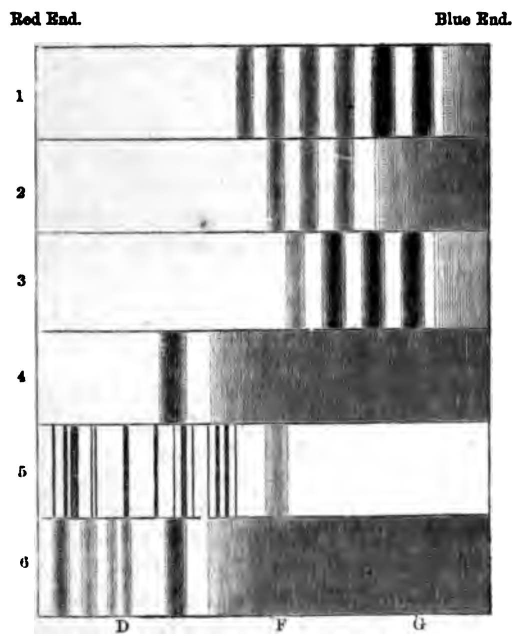
The remarkable spectrum of some jargons has been already described by me in the 'Chemical News'†, and in the Proceedings of the Royal Society‡. One of its most striking peculiarities is that when light passes in a direction perpendicular to the principal axis of the crystal, and the spectrum is divided by means of a double-image prism into two spectra, having the light polarized in opposite planes, though some of the absorption-bands are of equal intensity in both images, yet others are comparatively absent, some in one and some in the other; whereas in the case of other dichroic crystals which give spectra with absorption-bands, they are usually all more distinct in one image than when the light is not polarized, and all fainter, or even comparatively absent, in the other. No sooner had I observed this spectrum (No. 5, given below), than I made various experiments in order to ascertain whether uranium was present or not; and the then known tests that could be applied to the amount of material at my command seemed to show that it was absent. This was quite in accord with the results of the various analyses published by other chemists, none of whom mention the existence of any trace of that substance. Moreover the general character of the spectrum was entirely unlike that of all the known compounds of uranic oxide. The various artificial salts all agree in giving a variable but small

* See Stokes's papers, Phil. Trans. 1852, p. 463, and 1853, p. 392.

† Vol. xix. p. 122.

‡ Vol. xvii. p. 511.

number of moderately broad absorption-bands in the blue end (Nos. 1, 2, and 3); and the same is also seen in the case of several natural minerals; whereas the jargon gave a most unusually large number of narrow black lines (fourteen quite distinct, besides others more faint, and a single broader band which I cannot separate into lines), extending from the red end, so that nearly all occur in that part of the spectrum which is entirely free from bands in all previously known compounds of uranic oxide. This same general fact was also seen in the spectrum (No. 6) of the opaque blowpipe-beads gently flamed, as described in my former paper. These differences will be better understood by means of the following drawing, which shows three of the most striking spectra of uranic salts, that of uranate of soda, and the two which are rendered so abnormal by the presence of zirconia.



Spectra of Uranic Compounds.

Nos. 1, 2, and 3. Uranic salts of the common type, viz.:—

1. Native phosphate.
2. Nitrate in crystals.
3. Acetate in crystals.

No. 4. Uranate of soda in the carbonate-of-soda bead.

Nos. 5 and 6. Uranic compounds with zirconia, viz.:—

5. Jargon after ignition.
6. Crystalline borax blowpipe-bead.

I will not now enter into a description of the various chemical and physical facts which seemed to warrant the conclusion that zircons sometimes contain a new earth; but taking these into consideration, there seemed

to be every reason to believe that spectra which thus differed so much from those of any previously known substance were characteristic of this new earth. Judging from the facts then known, it was more probable that spectra of such a new type were due to a new element, than that they were due merely to a combination of two such elements as zirconium and uranium. Some of these chemical and physical facts can now be explained by the presence of uranium; but besides this and several of the more common earths and oxides, I have detected in some zircons erbium, didymium, yttria, and another substance which exists in such small quantity that I have not yet been able to ascertain whether or no it is the suspected new earth. These accidental constituents do not indeed occur in sufficient quantity to be of importance, except as modifying the physical and optical properties, the didymium giving its usual characteristic absorption-bands (zircons from Sveneroe, Norway), and the manganese the same spectrum as that of garnets (zircons from an unknown locality in Siberia*). This method, however, fails to give evidence of a new earth; for since the publication of my former paper, I have proved that the very abnormal spectra, which seemed sufficient to establish its existence, are really due to compounds of zirconia with the oxides of uranium, which have such a powerful action on light, that an almost inappreciable amount is sufficient to produce the spectra to great perfection—in fact so small an amount, that the total quantity which misled me was only a few thousandths of a grain; and its presence might easily have remained unsuspected, if I had not made a number of experiments, which at first did not seem to have much connexion with the subject.

In studying the spectra of crystalline blowpipe-beads, it seemed desirable to examine those made with carbonate of soda, with or without a little borax. This when melted dissolves certain oxides; and though it crystallizes on cooling, so as to be only partially translucent, yet with strong direct sunlight well-marked spectra may be seen. For example, in the oxidizing flame uranic oxide is easily dissolved by carbonate of soda alone; and when quickly cooled, an orange-coloured bead is obtained, probably containing uranate of soda in a vitreous condition, which gives a single well-marked absorption-band in the green (see spectrum No. 4) with so small a quantity of the oxide; that in a bead $\frac{1}{5}$ inch in diameter shows the spectrum to the best advantage, and even $\frac{1}{10000}$ grain can be easily detected. We need not be surprised that this spectrum differs so much from the usual type of uranic salts (Nos. 1, 2, and 3), since in this case the oxide plays the part of an acid. It may be only an accidental coincidence, but this difference is analogous to the change which commonly occurs on adding an alkali to neutral solutions of vegetable colours†. When gently reheated it seems as if the uranate passed into a crystalline state, for the spectrum then shows four absorption-bands, and is more like the

* For both of these I am indebted to my kind friend Mr. David Forbes.

† See my paper in Proc. Roy. Soc. 1867, vol. xv. p. 433.

ordinary type; but this change does not occur if a little borax has been added. The addition of more and more borax causes the absorption-band to become more and more faint, and to advance towards the blue end, until we obtain a spectrum with very faint bands but of the usual character.

In examining the various products into which I separated jargons in order to study the supposed new earth in a state of purity, I obtained a small quantity of a dark-coloured substance, apparently zirconia containing some oxide which communicated a green tint to a glassy, borax blowpipe-bead, but yet not sufficiently distinct to show that it was due to uranous oxide. I therefore thought that the carbonate-of-soda method might throw light on the question; and though the presence of zirconia prevented solution by pure carbonate of soda, the addition of a little borax enabled me to prove that uranic oxide is really present in some jargons. Such, then, being the case, it seemed desirable to ascertain whether the oxides of uranium would give rise to any special spectra when present along with zirconia in crystalline blowpipe-beads. To my astonishment I found that the spectra were precisely the same as those obtained in the case of what I had thought to be an approximately pure new earth *. When, however, I had ascertained the quantity of oxide requisite to give this result, I was no longer surprised that I had not suspected its presence. In the case of transparent blowpipe-beads of borax with microcosmic salt, it is requisite to have as much as about $\frac{1}{50}$ grain of uranous oxide to show faintly the characteristic absorption-bands, whereas, when present along with zirconia in the crystalline beads, $\frac{1}{50000}$ grain gives an equally well-marked spectrum; and $\frac{1}{5000}$ grain shows it far better than a larger quantity, which makes the beads too opaque. These very minute quantities were obtained by the repeated division of a small known weight, either before or after fusion with borax. This spectrum also differs very considerably from the spectra of the usual salts or blowpipe-beads of uranous oxide. On comparing them side by side, the only common peculiarity is the fact of there being numerous absorption-bands distributed over a large part of the spectrum, but they do not correspond in either number or position. On the contrary, they differ almost as much as possible, and the darker bands in the spectrum of this zirconia compound occur where the transmitted light is the brightest in other cases.

The oxide of uranium is so easily reduced at a high temperature to the state of protoxide in a borax-bead, with excess of boric acid, and is so readily peroxidized at a dull-red heat, when crystallized along with borate of zirconia, that there seemed good reason to refer the change in the spectra to temperature rather than to the state of oxidization, until after it was found that they were due to uranium. By gently flaming the crystalline bead, the spectrum is entirely altered, and becomes like No. 6, which seems to be characteristic of a compound of borate of zirconia with uranic oxide. This gives a spectrum with five well-marked absorption-bands,

* Figs. 1 and 2 of my former paper.

all of which occur at the red end, where no trace of bands exists in the case of ordinary salts, as will be seen on comparing it with Nos. 1, 2, and 3. I have tried many experiments in order to ascertain whether any other element besides zirconia will cause uranium to give similar abnormal spectra, but none show anything of the kind, at all events in similar conditions. A few have special characters, as described below, but the majority exert little or no influence; and even when the blowpipe-beads are crystalline, they show only the usual spectra of the oxides of uranium. Moreover no such great change in the character of the spectra of any other elements which give absorption-bands is to be seen when they are combined with zirconia; and, as far as my present experience goes, it seems as if such very abnormal spectra were met with only in the case of these remarkable compounds of zirconia with the oxides of uranium.

Such, then, being the facts, it appears to me that we are now in a position to explain why certain zircons give three different spectra, as described in my former paper. Some jargons (usually those of a green tint) contain a little uranium so combined that the characteristic spectrum is only faintly visible, whereas, after ignition, the intensity of the absorption-bands is permanently increased to a variable extent, occasionally only a little, but in some cases as much as twenty-five times. This more powerful action on light is accompanied by an increase in hardness and in specific gravity (sometimes as much as from 4·20 to 4·60), as described in my former paper; and I have since found that these changes are approximately proportional to the amount of uranic oxide in the various specimens, as shown by comparing the spectra of the blowpipe-beads. This change may partly depend on the oxidization of the uranous oxide, since some specimens slightly increase in weight when ignited; but I think it cannot be mainly due to that; for sometimes there is no such increase, and uranous oxide combined with zirconia gives rise, not to a spectrum without bands, but to one with several of very marked character, as described below. On the whole, since this abnormal type of spectrum is so characteristic of combination with zirconia, it appears to me more probable that the effect of a high temperature is to cause the uranic oxide to combine more specially with the zirconia, as though the greater part existed naturally as a silicate, but after ignition as a zirconiate. We may also apply the same explanation in the case of zircons more or less strongly coloured by other oxides, which become almost colourless when heated, and thus this unexplained peculiarity of zircons may depend on the fact of zirconia being able to play the part of both a base and an acid, which, as compared with silica, has an affinity for bases varying according to the temperature.

The brown-red zircon from Ceylon, named at page 514 of my former paper, kindly presented to me by Mr. E. L. Mitford, of Rusthall, gives a spectrum precisely like that of the borax blowpipe-beads crystallized after treatment in the deoxidizing flame, and therefore no doubt contains uranous oxide. This spectrum being given by only one part of the crystal, probably

depended upon the presence of some substance which either reduced the uranic oxide or prevented the oxidization of the uranous.

These facts thus clearly show that the various spectra which seemed to indicate the presence of a new element existing in three different physical conditions, are in reality only characteristic of the two oxides of uranium combined with zirconia, or not in combination. Perhaps some may think that my having been thus led astray shows that little or no reliance can be placed on the method of investigation employed; but I contend that the mistake was due to its being such an unexpectedly delicate test for uranium; and, as explained above, the error was ultimately corrected by a further development of the same method. As far as the interests of science are concerned, there is no need to regret the general result. We have lost what appeared to be good evidence of a new earth, but have gained an almost entirely new system of blowpipe testing, which enables us to detect such a minute quantity of some substances as could not be recognized by the ordinary means. I shall not now attempt to give anything like a full account of this subject, since it would be much better to let it form part of a paper on various improvements in blowpipe chemistry, but will merely mention a few facts which have a special bearing on the question before us.

In the first place, I would say that zirconia and the oxides of uranium are most useful reagents in detecting the presence of certain substances with which they unite to form compounds having very special characters. The most striking of these are the compounds already described, which are distinguished by the spectra, and not by any well-marked colour,—the compound of ceric oxide with uranic oxide, which is of a splendid deep blue colour, but shows no absorption-bands; and that of yttria with uranic oxide, which is characterized by a deep orange-colour and extreme fusibility. Thorina and oxide of lanthanum form with uranous oxide compounds which give spectra with absorption-bands in special positions, but of the usual type, and not of such a marked character as to be useful in detecting minute quantities of those substances in mixtures.

In order to see the spectra of the zirconium-uranium compounds, it is requisite that both elements should be combined in a crystalline condition. When both constituents are melted in borax and are held in solution, or if when crystals are deposited any other substance replaces either the zirconia or the oxides of uranium, the characteristic spectra cannot be seen. The most simple application of this test for uranium is in the case of various zircons. As much of the powdered mineral as will dissolve should be melted with borax in a circular loop of platinum wire about $\frac{1}{8}$ inch in diameter, so as to give a bead of moderate thickness. A little boric acid should then be added, which not only tends to keep the uranium in the state of protoxide, but also facilitates the crystallization of the borate of zirconia, which is far less soluble when there is excess of boric acid. The bead should then be kept at a bright red heat, just within the deoxidizing flame, until so much borax has been volatilized that small needle-shaped crystals begin to be

deposited, when it must be allowed to cool rapidly. It should then be transparent with scattered crystals, and the uranium all in the state of protoxide. On gently reheating it, the bead ought to suddenly turn white and almost opaque; and care must be taken not to heat it any more than is just requisite to cause the borate to crystallize out, or else the uranium will rapidly pass into the state of peroxide. Such beads must be examined by strong direct light from the sun, or from a lamp of very great brilliancy, condensed on them by means of an almost hemispherical lens of about $\frac{1}{4}$ inch focal length; and in addition to the means described in my former paper, I have since found it very convenient to place them over a hole in a black card, so as to entirely prevent the passage of any light which has not penetrated through them, even when so arranged in the focus of the microscope that the spectrum of their thin edges may be examined, if the centre be too thick and opaque. If thus properly prepared, the presence of more or less uranium will be shown by the greater or less intensity of the absorption-bands of the spectrum described and shown in fig. 1 of my former paper. This test is so delicate that there is no difficulty in seeing the darker band in the green in the case of zircons which contain no more than $\frac{1}{10}$ per cent. of uranic oxide; and I find that very few localities yield this mineral so free from it that it cannot be easily detected. Those from Miask, Siberia, are the only specimens in which I have not been able to recognize it. The jargons from Ceylon contain an amount varying up to about 1 per cent., although in no published analysis that I have seen is there any allusion to the presence of even a trace. It has also been overlooked in several other cases; and it now becomes important, because it gives rise to various well-defined spectra, which are so characteristic of the different minerals, that they can be very conveniently identified, even when cut and mounted as jewels, by means of the number and position of the absorption-bands, as I intend to explain in a paper on the spectra of minerals.

On flaming the bead at a moderate red heat, the protoxide passes into the peroxide, and the spectrum No. 6, given above, may be seen, if sufficient oxide be present, but considerably more is required than in the case of the protoxide. I may here say that the examination of better preparations has enabled me to detect another distinct band in the extreme red, not shown in fig. 2 of my former paper, and also an additional faint band in the blue, not shown in fig. 1.

In applying this test to detect minute quantities of uranium in other minerals, it is requisite to bear in mind that zirconia may play the part of both an acid and a base, and that various oxides and acids so combine with the zirconia or with the oxides of uranium as to prevent the formation of the compounds which give rise to the characteristic spectra. The zirconia appears to combine with some rather than with the uranous oxide, and with others rather than with the uranic, so that, if one spectrum cannot be obtained, the other may; and there are few, if any, cases when

neither can be seen, especially if care be taken to use excess of zirconia. If, however, the amount of uranium be very small, and so much of other oxides be present as to make the bead very dark, or too opaque from deposited crystals, before it is sufficiently concentrated for the compounds with the oxides of uranium to crystallize out, it may be impossible to detect it. In order to apply the test in the case of complex minerals, a bead of borax, boric acid, and pure zirconia should be prepared, then a small quantity of the mineral added, and, after fusion and sufficient concentration, the bead made to crystallize in the manner already described. If needle-shaped crystals be not deposited in the bead when very hot, and if it do not suddenly turn opaque when reheated, the result may not be satisfactory. In this manner it is easy to detect uranium in $\frac{1}{200}$ grain of such minerals as Fergusonite, tyrite, and yttrotantalite, even when they contain no more than 1 or 2 per cent. If in such cases the spectrum of the uranous compound cannot be obtained, the bead should always be flamed and reexamined, to see if that of the uranic compound is thereby developed.

In a similar manner we may make use of a little oxide of uranium to detect zirconia; but the test is far less delicate than the converse, because it is almost impossible to obtain the compound in a crystalline state, unless there be an excess of zirconia. Not more than $\frac{1}{1000}$ grain of uranic oxide should be employed, or the bead may be too opaque. There is no difficulty in thus detecting zirconia in zircons, or in katapleiit; but the presence of so much of other bases in minerals like eudialyte prevents our obtaining a satisfactory result. There certainly could not be a more characteristic test to confirm the results of other methods, or to identify such a small quantity of approximately pure zirconia as could not easily be distinguished in any other way.

The only other compound of uranic oxide of very abnormal character which I have so far discovered is that with ceric oxide. So much of both oxides should be fused with borax in the oxidizing flame as will yield a bead which is perfectly clear, and of pale yellow colour when rapidly cooled, but crystallizes when gently reheated. If the constituents be present in a certain proportion, it then turns from pale yellow to a deep blue, as though coloured by oxide of cobalt. In mos cases the bead is rendered nearly opaque by the number of crystals; but sometimes, though it turns deep blue, it remains transparent, owing to the compound being set free in a state similar to that of the red oxide of copper in a borax blowpipe-bead, with carbonate of soda and oxide of tin, treated in the reducing flame. The spectrum of these blue beads shows no absorption-bands, but merely a general absorption at the red end; and it is curious to find that the combination of two yellow substances gives rise to a deep blue, in much the same manner as when the yellow ferrocyanide of potassium is added to a yellow ferric salt. The production of this blue colour on the addition of a little uranic oxide might be employed with advantage to identify moderately small quantities of cerium, even when mixed with a number of other

substances; but unfortunately the presence of much oxide of lanthanum, which is so commonly associated with it, interferes, as though the ceric oxide had a stronger affinity for the oxide of lanthanum than for uranic oxide.

The most characteristic peculiarity of the compound of yttria and uranic oxide is that it will not crystallize out from a borax blowpipe-bead, and that the affinity of the uranic oxide for yttria is stronger than for zirconia. Perhaps erbia may prove to act in the same way, but I have not been able to examine that earth quite free from yttria. On adding yttria to a bead with zirconia and a little uranic oxide, and gently flaming it in the oxidizing flame, the uranic oxide combines with the yttria and rises to the surface as an orange-coloured scum, which has a great tendency to collect on the platinum wire; and if sufficient yttria be added, the crystallized borate of zirconia is left in the interior almost colourless, and so free from uranic oxide that no absorption-bands can be seen in the spectrum. We may take advantage of this circumstance to detect yttria in small quantities of compound minerals like Gadolinite and Fergusonite; and I may here say that by combining such means with the observation of the spectra of the transparent or crystalline beads, and of the form of the crystals when slowly deposited, with or without the addition of suitable reagents*, we may often detect twice as many constituents in minerals as could be accomplished by the ordinary methods of blowpipe chemistry—an advantage which I am sure will be appreciated by those engaged in the study of rocks, when it is often so important to obtain satisfactory results with small quantities of material. I have also found these methods of great practical use in examining small residues in the qualitative analysis of minerals, and have thus unexpectedly discovered small quantities of comparatively rare elements.

I have tried the effect of many other substances along with zirconia and the oxides of uranium, and find that most of them have no sensible influence, unless they are present in considerable relative quantity. The most striking effect is that of oxide of tin, which causes the two absorption-bands in the yellow and yellow end of the green in the spectrum of the uranous oxide compound to be nearly equally dark, whereas without the oxide of tin that in the yellow is comparatively faint. This is another illustration of the manner in which certain substances, having no special action on light, influence by their presence the properties of another. The oxides of uranium are unusually sensitive to such actions, and thus not only lend themselves to us as blowpipe-reagents, but also seem more than any others to afford the means of explaining the relation between the physical conditions of compounds and their action on light.

The only compound of zirconia with any other oxide to which I need now draw attention is that with chromic oxide, as deposited from a borax blowpipe-bead. After treatment in the deoxidizing flame, when the cooled very pale-green bead is gently reheated, this compound crystallizes out

* See my paper, Monthly Microscopical Journal, vol. i. p. 349.

so as to give a fine red-pink colour by transmitted light, even when so little chromium is present that the glassy bead is scarcely at all green. If too strongly heated the pink tint is lost. This compound is of interest in connexion with the colour of rubies and other minerals coloured red by chromic oxide. To others, like the emerald, it imparts a green colour, and on the whole it acts on light in such a variable manner according to the presence of other substances, that the spectra may be made use of as a means of identifying particular minerals, though they do not present anything like such striking anomalies as those met with in the compounds of zirconia with the oxides of uranium.

II. "On the Mathematical Theory of Stream-lines, especially those with four Foci and upwards." By William John Macquorn Rankine, C.E., LL.D., F.R.SS. Lond. and Edinb., &c. Received January 1, 1870.

(Abstract.)

A *Stream-line* is the line that is traced by a particle in a current of fluid. In a steady current each individual stream-line preserves its figure and position unchanged, and marks the track of a filament or continuous series of particles that follow each other. The motions in different parts of a steady current may be represented to the eye and to the mind by means of a group of stream-lines.

Stream-lines are important in connexion with naval architecture; for the curves which the particles of water describe relatively to a ship, in moving past her, are stream-lines; and if the figure of a ship is such that the particles of water glide smoothly over her skin, that figure is a *stream-line surface*, being a surface which contains an indefinite number of stream-lines.

The author in a previous paper proposed to call such stream-lines *Neoïds*; that is, ship-shape lines.

The author refers to previous investigations relating to stream-lines, and especially to those of Mr. Stokes, in the Cambridge Transactions for 1842 and 1850, on the "Motion of a Liquid past a Solid," and of Dr. Hoppe, on the "Stream-lines generated by a Sphere," in the Quarterly Journal of Mathematics for 1856, and to his own previous papers on "Plane Water-lines in Two Dimensions," in the Philosophical Transactions for 1864, and on "Stream-lines," in the Philosophical Magazine for that year. He states that all the neoïd or ship-shape stream-lines whose properties have hitherto been investigated in detail are either *unifocal* or *bifocal*; that is to say, they may be conceived to be generated by the combination of a uniform progressive motion, with another motion consisting in a divergence of the particles from a certain point or focus, followed by a convergence either towards the same point or towards a second point. Those which are

continuous closed curves when unifocal are circular, and when bifocal are blunt-ended ovals, in which the length may exceed the breadth in any given proportions. To obtain a unifocal or bifocal neoïd resembling a longitudinal line of a ship with sharp ends, it is necessary to take a part only of a stream-line, and then there is discontinuity of form and of motion at each of the two ends of that line.

The author states that the occasion of the investigation described in the present paper was the communication to him by Mr. William Froude of some results of experiments of his on the resistance of model boats, of lengths ranging from three to twelve feet. A summary of those results is printed at the end of a Report to the British Association on the "State of Existing Knowledge of the Qualities of Ships." In each case two models were compared together of equal displacement and equal length; the water-line of one was a wave-line with fine sharp ends, that of the other had blunt rounded ends, each joined to the midship body by a slightly hollow neck—a form suggested, Mr. Froude states, by the appearance of water-birds when swimming. At low velocities, the resistance of the sharp-ended boat was the smaller; at a certain velocity, bearing a definite relation to the length of the model, the resistances became equal, and at higher velocities the round-ended model had a rapidly increasing advantage over the sharp-ended model.

Hence it appeared to the author to be desirable to investigate the mathematical properties of stream-lines resembling the water-lines of Mr. Froude's bird-like models; and he has found that endless varieties of such forms, all closed curves free from discontinuity of form and of motion, may be obtained by using *four* foci instead of two. They may be called from this property *quadrifocal stream-lines*, or, from the idea that suggested such shapes to Mr. Froude, *cycnoïds*; that is, swan-like lines*.

Those lines are not to be confounded with the lines of a yacht having at a distance the appearance of a swan, which was designed and built some years ago by Mr. Peacock, for the figure of that vessel is simply oval.

The paper contains four chapters. The first three are mainly cinematical and geometrical, and relate to the forms of stream-line surfaces in two and in three dimensions, especially those with more than one pair of foci and surfaces of revolution, to the methods of constructing graphically and without calculation, by means of processes first applied to lines of magnetic force by Mr. Clerk Maxwell, the traces of such surfaces, which methods are exemplified by diagrams drawn to scale, and to the motions of the particles of liquid past those surfaces. The fourth chapter is dynamical: it treats of the momentum and of the energy of the disturbance in the liquid, caused by the progressive motion of a solid that is bounded by a ship-shape stream-line surface of any figure whatsoever; of the ratio borne by the total energy of the disturbance in the liquid to that of the disturbing body when that body displaces a mass of liquid equal to its own mass,

* Κυκνοειδής.

which ratio ranges in different cases from $\frac{1}{2}$ to 1; of the acceleration and retardation of ships as affected by the disturbance in the water, and especially of the use of experiments on the retardation of ships in finding their resistance; and of the disturbances of pressure which accompany the disturbances of motion in the liquid. Up to this point the dynamical principles arrived at in the fourth chapter are certain and exact, like the geometrical and cinematic principles in the three preceding chapters. The results obtained in the remainder of the fourth chapter are in some respects approximate and conjectural, and are to a great extent designed to suggest plans for future experiments, and rules for their reduction. These results relate to the disturbances of level which accompany the disturbances of motion when the liquid has a free upper surface, to the waves which originate in those disturbances of level, and the action of those waves in dispersing energy and so causing resistance to the motion of the vessel, to friction, or skin-resistance, and the "wake" or following current which that kind of resistance causes the disturbing solid body to drag behind it, and to the action of propelling instruments in overcoming different kinds of resistance.

The resistance caused by viscosity is not treated of, because its laws have been completely investigated by Mr. Stokes, and because for bodies of the size of ships, and moving at their ordinary velocities, that kind of resistance is inconsiderable compared with skin-resistance and wave-resistance. The resistance caused by discontinuity of figure is stated to be analogous in its effects to friction, but it is not investigated in detail, because ships ought not to be built of discontinuous (commonly called "unfair") figures.

Supplement. Received January 8, 1870.

The author in the first place calls attention to the agreement between the position of the points at which there is no disturbance of the pressure on the surface of a sphere, as deduced from Dr. Hoppe's investigation, published in 1856 (Quarterly Journal of Mathematics), and on the surface of a short vertical cylinder with a flat bottom, as determined by the experiments of the Rev. E. L. Berthon before 1850 (Proc. Roy. Soc. vol. v. 1850; also Transactions of the Society of Engineers, 6th December, 1869). The theoretical value of the angular distance of those points from the foremost pole of the sphere is $\sin^{-1}\frac{2}{3}=41° 49'$; the value deduced from experiment is 41° 30'.

The author then adds some remarks on a suggestion made by Mr. William Froude, that the wave-resistance of a ship is diminished when two series of waves originating at different points of her surface partially neutralize each other by interference; and states that, with regard to this and many other questions of the resistance of vessels, a great advancement of knowledge is to be expected from the publication in detail of the results of experiments on which Mr. Froude has long been engaged.

III. "On Linear Differential Equations."—No. II. By W. H. L. Russell, F.R.S. Received January 20, 1870, being made up of two Papers received December 30, 1869, and January 6, 1870.

The principles laid down in my former paper will enable us to integrate a proposed differential equation, when the solution can be expressed in the form $\frac{P}{Q}\epsilon^{\omega}$, where P, Q, ω are rational and entire functions of (x).

Let

$$(\alpha_0+\alpha_1 x+\alpha_2 x^2+\ldots+\alpha_m x^m)\frac{d^n y}{dx^n}+$$

$$(\beta_0+\beta_1+\beta_2 x^2+\ldots+\beta_m x^m)\frac{d^{n-1}y}{dx^{n-1}}+$$

$$(\gamma_0+\gamma_1 x+\gamma_2 x^2+\ldots+\gamma_m x^m)\frac{d^{n-2}y}{dx^{n-2}}+\ldots$$

$$+(\lambda_0+\lambda_1+\lambda_2 x^2+\ldots+\lambda_m x^m)y=0$$

be the general linear differential equation of the nth order, where none of the indices of (x) in the coefficients of the succeeding terms are greater than those in the coefficients of the two first. Then if the equation admit of a solution of the form $\epsilon^{\int\frac{\phi_2(x)}{\phi_1(x)}dx}$, where $\phi_1(x)$, $\phi_2(x)$ are rational and entire functions of x, and $\phi_1(x)$ and $\alpha_0+\alpha_1 x\ldots+\alpha_m x^m$ have no factors in common, and if the degree of the coefficients of the two first terms is the same,

$$y=\mathrm{E}(x)\,\epsilon^{-\int\rho dx},$$

where ρ is a rcot of the equation

$$\rho^n\alpha_m-\rho^{n-1}\beta_m+\rho^{n-2}\gamma_m\ldots\pm\lambda_m=0\,;$$

and if $\alpha_m=0$,

$$y=\mathrm{E}(x)\epsilon^{-\int dx\left\{\frac{\beta_{m-1}}{\alpha_{m-1}}-\frac{\gamma_m}{\beta_m}-\frac{\alpha_{m-2}\beta_m}{\alpha^2_{m-1}}+\frac{\beta_m x}{\alpha_{m-1}}\right\}}\,;$$

and if $\alpha_m=\alpha_{m-1}=0$,

$$y=\mathrm{E}(x)$$

$$\epsilon^{-\int dx\left\{\frac{\beta_{m-2}}{\alpha_{m-2}}-\frac{\gamma_m}{\beta_m}+\frac{\alpha_{m-3}\beta_{m-1}}{\alpha^2_{m-2}}+\frac{\alpha^2_{m-3}\beta_m}{\alpha_{m-2}}-\frac{\alpha_{m-4}\beta_m}{\alpha^2_{m-2}}+\left(\frac{\beta_{m-1}}{\alpha_{m-2}}-\frac{\alpha_{m-3}\beta_m}{\alpha^2_{m-2}}\right)x+\frac{\beta_m}{\alpha_{m-2}}x^2\right\}},$$

and so on, where the value of y is to be substituted in the proposed equation, which then becomes a linear equation to determine the rational and entire function E(x).

When, however, $\alpha_m=\beta_m=0$, or, in other words, when the degree of the coefficients of the succeeding terms of the proposed equation exceeds the degree of the coefficients of the two first, some modification is required;

thus if

$$(\alpha+\beta x)\frac{d^3y}{dx^3}+(\alpha'+\beta'x+\gamma'x^2)\frac{d^2y}{dx^2}+(\alpha''+\beta''x+\gamma''x^2+\delta''x^3+\zeta''x^4)\frac{dy}{dx}$$
$$+(\alpha'''+\beta'''x+\gamma'''x^2+\delta'''x^3+\zeta'''x^4+\eta'''x^5)y=0,$$
$$y=E(x)e^{-\int dx\left\{\frac{\zeta'''}{\zeta''}-\frac{\delta''\eta'''}{\zeta''^2}+\frac{\gamma'\eta'''^2}{\zeta''^3}-\frac{\beta\eta'''^3}{\zeta''^4}+\frac{\eta'''}{\zeta''}x\right\}}$$

where $E(x)$ is to be determined as before.

But now let $\phi_1 x$ and $a_0+a_1x+a_2x^2+\ldots a_mx^m$ have factors in common. We have the two equations,

$$y=\frac{P}{Q}e^{\omega},\quad y=e^{\int\frac{\phi_2 x}{\phi_1 x}dx},$$

$$PQ\frac{dy}{dx}=QP'-PQ'+P\omega',\quad \phi_1 x\frac{dy}{dx}=\phi_2 x;$$

hence, since $\frac{\phi_2 x}{\phi_1 x}$ is a fraction in the lowest terms, any common factors of $\phi_1 x$ and $a_0+a_1x+\ldots+a_mx^m$ must be factors of P or Q; hence if $x-a$ be one of the factors of $a_0+a_1x+\ldots+a_mx^m$, we may ascertain if it is a factor of P and Q by putting in the proposed differential equation

$$y=A_m(x-a)^m+A_{m+1}(x-a)^{m+1}+A_{m+2}(x-a)^{m+2}+\ldots,$$

and shall thus obtain an equation to determine the index (m); and we must treat the other factors of $a_0+a_1x+a_2x^2+\ldots a_mx^m$ in the same way, and thus ascertain those which are also factors of P and Q.

I shall illustrate these remarks by applying them to the well-known differential equation

$$\frac{d^2u}{dx^2}-\frac{i(i+1)}{x^2}u-q^2u=0.$$

We have

$$x^2\frac{d^2u}{dx^2}-i(i+1)u-q^2x^2u=0.$$

Let

$$u=Ax^{\mu}+Bx^{\mu+1}+Cx^{\mu+2}+\ldots$$

Substituting $\mu(\mu-1)-i(i+1)=0$, whence $\mu=-i$; putting then $u=\frac{z}{x^i}$

$$x\frac{d^2z}{dx^2}-2i\frac{dz}{dx}-q^2xz=0;$$

hence $z=E(x)\,e^{qx}$, if the equation can be integrated in the form $y=\frac{P}{Q}e^{\omega}$, which gives us

$$x\frac{d^2E}{dx^2}+2(qx-i)\frac{dE}{dx}-2iqE=0.$$

Putting

$$E(x)=a_0+a_1x+a_2x^2+\ldots,$$

we have

$$m(m-2i-1)a_m+2q(m-i-1)a_{m-1}=0,$$

which determines the function $E(x)$, a rational and entire function of the ith degree.

I conclude this paper with a proposition of much importance in the theory of linear differential equations,

Let

$$\phi_n x \frac{d^n y}{dx^n} + \phi_{n-1} x \frac{d^{n-1} y}{dx^{n-1}} + \phi_{n-2} x \frac{d^{n-2} y}{dx^{n-2}} + \ldots + \phi_0 x y = 0$$

be any linear differential equation. Then in general this equation will not admit a solution of the form $y = f(\epsilon^x)$. For then, putting for (x) successively $x + 2\pi i$, $x + 4\pi i$, ..., we should have

$$\phi_n(x) \frac{d^n f(\epsilon^x)}{dx^n} + \phi_{n-1} x \frac{d^{n-1} f(\epsilon^x)}{dx^{n-1}} + \phi_{n-2} x \frac{d^{n-2} f(\epsilon^x)}{dx^{n-2}} + \ldots = 0,$$

$$\phi_n(x + 2\pi i) \frac{d^n f(\epsilon^x)}{dx^n} + \phi_{n-1}(x + 2\pi i) \frac{d^{n-1} f(\epsilon_x)}{dx^{n-1}} + \ldots = 0,$$

$$\phi_n(x + 4\pi i) \frac{d^n f(\epsilon^x)}{dx^n} + \phi_{n-1}(x + 4\pi i) \frac{d^{n-1} f(\epsilon^x)}{dx^{n-1}} + \ldots = 0.$$

And these equations can be indefinitely continued. It will be observed that this solution does not comprise integrals of the form $\frac{P}{Q} \epsilon^{\frac{u}{v}}$, where $\frac{u}{v}$ is a rational function.

February 17, 1870.

Dr. WILLIAM ALLEN MILLER, Treasurer and Vice-President, in the Chair.

The following communications were read :—

I. "On a distinct form of Transient Hemiopsia." By HUBERT AIRY, M.A., M.D. Communicated by the Astronomer Royal. Received January 6, 1870.

(Abstract.)

From a comparison of the different accounts of "Hemiopsia," "Half-vision," or "Half-blindness," given by Dr. Wollaston (Phil. Trans. 1824, p. 222), M. Arago (Annales de Chimie et de Physique, tom. xxvii. p. 102), Sir David Brewster (Phil. Mag. 1865, vol. i. p. 503, and Transactions of Royal Society of Edinburgh, vol. xxiv. part 1), the Astronomer Royal (Phil. Mag. July 1865, vol. ii. p. 19), Professor Dufour (in a letter to the Astronomer Royal), Sir John Herschel (Familiar Lectures on Scientific Subjects, p. 406, Lecture IX., and private letters), Sir Charles Wheatstone (in a private letter), Mr. Tyrrell (On the Diseases of the Eye, 1840, vol. ii. p. 231), and the author of this paper, it is plain that there are different forms of transient Hemiopsia, irrespective of the wide primary distinction

between the transient and permanent forms, which have all been included under the same name Hemiopia or Hemiopsia.

It seems that Wollaston, Arago, Brewster, and Tyrrell are describing one form of the transient affection, while Sir John Herschel, Sir Charles Wheatstone, the Astronomer Royal, Professor Dufour, and the author agree in describing another.

In the experience of the former group, the limits of the blind region, as projected on the field of view, are ill-defined; there is no variety of colour, and the progress of the disease presents no remarkable features.

In the latter group, the blind region is at first very small, and gradually spreads outwards, to left or right, with a zigzag margin of bright and dark lines, tinged in most cases with various colours,—clear vision gradually returning in the centre and following the outward advance of the curved cloud; usually the blindness occupies only one lateral half of the field of view; but in one very remarkable instance recorded by Sir John Herschel, the course of the cloud was from the extreme left to the extreme right, sweeping over the whole of the visual area.

Possibly the gap between these two forms may be filled by connecting links, as further evidence arises, and it may be found that they differ only in degree of prominence of different features. The remarkable account given by Sir Charles Wheatstone (who has kindly given permission for its publication), where the zigzag luminous lines are strongly marked, but without colour, perhaps offers the first link in the connecting chain.

The author's experience dates from 1854. Since then he has repeatedly suffered from these attacks. The circumstances and features of the complaint have varied somewhat in different attacks, but the type has remained unaltered from that time to this.

The blindness comes on usually while the eyes are engaged in toilsome reading: some word or letter on the page near the sight-point (generally below to the left) is found to be obliterated; this germ of blindness slowly spreads, with zigzag margin, defined by alternate bright and dark lines, with gleams of colour, the margin rapidly trembling and slowly rolling at the same time.

These three orders of motion, (1) gradual outward growth of the whole, (2) slow rolling of parts, (3) rapid tremor of the margin, are especially characteristic of this affection.

The region of blindness takes a horseshoe shape; the upper arm points to the centre of sight, while the lower spreads downwards and outwards away from the centre. The zigzag pattern is minute near the centre, and grows larger the further it recedes. The gleams of colour, most conspicuous at the margin, are red and blue, yellow, green, orange, in order of frequency. As the blindness spreads outwards, clear vision returns gradually in the concavity of the horseshoe. The sight of both eyes is affected at once, exactly in the same manner and in the same degree; though naturally that eye *seems* most affected which corresponds to the obliterated side of

the field of view, because the nasal half of the field of view of either eye is more limited, and vision there is less distinct than on the temporal side.

Looking at any surface of uniform colour, the cloud partakes of the general hue of the field on which it lies, and shows little that is characteristic except its bright margin, tremor, and boiling.

Against bright light a faint shadowy curved cloud is seen, with bright margin, tremor and boiling, and slight colour.

Against dark shade the cloud is seen to show faint light.

When part of the cloud is seen against dark shade and part against bright light, the boundary between the light and shade is wholly obliterated.

Viewed in the dark, the cloud presents inherent luminosity, especially at the margin. Its various colours are seen as well in dark as in light.

The cloud spreads outwards in horseshoe shape till it reaches the outskirts of the field of view, and fades away after great boiling and turbulence. The lower arm is the first to fade, then the middle, and finally the upper arm, which remains pointing to the centre of the field to the very last.

The climax is reached in about twenty-five minutes from the first beginning. The whole duration of the attack is just half an hour.

Often, midway in the attack, a fresh germ of blindness arises near the birthplace of the first, but always proves abortive unless it takes root on the opposite side, when a second attack may develope itself immediately after the first.

This half-bindness is followed by oppressive headache, lasting many hours.

From the resemblance of the angular margin of the cloud to a fortified wall "with salient and reentering angles, bastions, and ravelins" (to use Sir John Herschel's words), the author ventures to suggest the name *Teichopsia* for this striking form of transient half-blindness.

Among the circumstances that have seemed to favour an attack may be mentioned sudden change of air and living, over-exercise, and insufficient sleep. The attack has sometimes been nocturnal.

The most usual position of the germ of blindness is 3° or 4° below, and 3° or 4° to the left of the centre of vision.

In one or two cases, after reaching a certain stage, the cloud has parted in the middle, and died away without ripening.

The cloud, whether developed in the right or the left half of the field, has never (the author believes) transgressed the vertical median line.

Lately, one or two attacks have been followed by a slight disturbance of hearing.

Of three cases coming under the author's immediate observation, in one these attacks have been very frequent, from an early age to middle life. The bastioned outline is always present, with more or less colour. Formerly the attendant headache used to be very severe, accompanied with prolonged vomiting. Latterly the blindness has been more oppressive than

the headache, and its advent greatly dreaded. The speech is often affected, and sometimes the memory; and on one occasion the mouth was noticed to be drawn to one side. The cause has seemed to be mental anxiety.

In the second case, which is adduced for the sake of contrast, the phenomena are much less definite. There is no serrated margin, no colour, no curve, nothing of which a picture can be made. The obscurity grows from a small but ill-defined germ, and gathers like a cloudy film or gauze over the field, oppressive to the eyes, and accompanied by headache and nausea, and passes away after a doubtful period, leaving the impression that it is caused by disorder of the stomach.

In the third case, the blindness is sometimes brought on by looking at a striped wall-paper or a striped dress. The appearance before the eyes is described as zigzag, wavy, quivering, without colour. The first attack, in adult age, was followed by partial paralysis of one side, and later attacks have almost always had a sequel of defective speech, and tingling at the tip of the tongue, at the tip of the nose, and in the fingers and thumb.

At any rate it is certain that there does exist a distinct form of transient hemiopsia, presenting the following main characteristics:—

1. Dependence on mental anxiety, bodily exhaustion, overwork to the eyes, gastric derangement (?), want of exercise.
2. Origin from a small germ near the centre of vision.
3. Orderly centrifugal growth from the original germ.
4. Blindness to boundaries, but not to general impressions of light and colour.
5. Proper luminosity in the dark.
6. Bright-bastioned margin, with gleams of various colours.
7. Tremor and "boiling."
8. Gradual occupation of one lateral half of the field of view.
9. Gradual recovery of clear vision in rear of the outward-growing cloud.
10. Disappearance of the phenomenon after about half an hour.
11. Sequelæ: headache and nausea, and sometimes affection of speech and hearing, and even symptoms of hemiplegia.

As to the actual seat of the visual derangement, the exact agreement of the two eyes in the nature, extent, and degree of their affection proves (assuming the semidecussation of the optic nerves at the chiasma) that the seat of the affection must lie at some point behind the chiasma of these nerves. All the causes that are found to lead to transient half-blindness point to the brain as the seat of disturbance. Still clearer is the evidence given by the loss of speech and of memory, the derangement of hearing, and the partial paralysis that sometimes follow an attack of teichopsia. Such cases as Sir John Herschel's, where the cloud passed over the whole field from left to right, can only be explained by supposing the disturbance to lie in some region of the brain where the opposite halves are

in contact. The mischief may possibly be seated in the corpora quadrigemina or geniculata, or even in the cerebellum itself.

As to the nature of the mischief in the brain, it is difficult to do more than hazard guesses. Is it a temporary suspension of function among the nerve-cells of the visual sensorium, due to vascular congestion, and relieved by the relief of that congestion? Does the headache tell of the further propagation of the nervous disturbance into parts of the brain where disturbance is ache, as in the visual tract disturbance is abnormal sensation of light? And the detriment to speech and hearing,—does it mean extension of the same disturbance still further into the regions of brain-substance appropriate to those functions? Or is the attack in any way analogous to a fit of epilepsy?

The phenomena are so definite and so localized, and their course is so regular, that we can hardly avoid the conviction that their cause is equally definite and equally localized; and it is difficult to admit so vague an agent as nervous sympathy with gastric derangement, except as acting through the medium of some secondary local manifestation in the brain.

II. "Account of the Great Melbourne Telescope from April 1868 to its commencement of operations in Australia in 1869." By Albert Le Sueur. Received January 8, 1870. Communicated by the President.

A description of the great Melbourne reflector, and its history, up to the time of inspection by the Committee, have been communicated to the Royal Society; the following additional account of the doings connected therewith since the instrument was consigned to my care may be of interest to the Society.

Mr. Grubb commenced taking down the telescope at the end of April 1868; this was accomplished in no great length of time, and without any difficulty. The specula (by the advice of Mr. Lassell, who had found this method answer perfectly) were coated over with shellac varnish to prevent oxidation on the voyage out; they were then protected in their cells and on their lever supports by strong double wood casings, and the other parts of the telescope and machinery cased or otherwise protected. The only casualty which there seemed to be any reason to fear could give rise to any serious consequences was a tilting over of the speculum cases; their great weight was, perhaps, a sufficient guarantee from such an event: it was nevertheless thought prudent that the telescope, and machinery generally, should not be left entirely to the tender mercies of the shipping and crane labourers; I was therefore present at the shipping in Dublin on board a steam-tug hired for the purpose, and at the transshipment in Liverpool, on board the 'Empress of the Seas.'

Both these operations were performed satisfactorily, and without any serious casualty.

The 'Empress of the Seas' sailed from Liverpool on the 17th or 18th of July; I followed by the August Overland Mail.

On my arrival in Melbourne I found that, beyond the selection of a site in the Observatory grounds, nothing had yet been done towards the erection of piers or building; this was principally owing to the fact that Mr. Ellery and the Board of Visitors had not considered the information which they possessed sufficiently definite to warrant their placing the matter in the hands of the Works Department; it had therefore been thought advisable to await my arrival.

Some necessary modifications having been made in the drawings, the construction of the piers was soon proceeded with, and satisfactorily terminated at the beginning of this year.

In the mean time the 'Empress of the Seas,' with her precious cargo, had arrived, after a very long voyage, which for some time was the cause of much uneasiness; parts of the instrument were unpacked and temporarily housed: the whole appeared in fair order; there was certainly no material damage done to anything.

Arrangements being in progress for the erection of a suitable building, it was thought advisable to delay mounting the telescope until part of the building was constructed; little therefore was done for some time beyond setting up, as accurately as possible, the plummer-blocks which contain the polar axis bearings.

The building was commenced early in the year, and when it was thought that sufficient progress had been made, the crane which had been used in the erection of the piers was removed to a more convenient position, and the various heavy parts of the instrument lifted on to the floor of the telescope-room, over the walls or through a gap left for that purpose, and, for convenience in after operations, in the north wall and north end of the west wall.

The mounting was then proceeded with, and satisfactorily accomplished in little more than a week, as regards the main parts, without much difficulty.

Attempts were made on one or two occasions to use the instrument for adjustment and observation, but it was found that the dust (a dreadful enemy in the summer) and the grit caused by the building accumulated to such an extent as to lead to fear of considerable damage to the bearings and more delicate parts of the machinery; it was therefore deemed prudent to cover up the telescope as well as possible with tarpaulins, and leave it in that state for some time.

The building is rectangular, 80 feet long meridionally by 25 wide, with walls 11 feet high. Of the meridional length the telescope-room occupies the north 40 feet; the next 12 feet are appropriated to the polishing-machine, crane, and engine; the remaining 28 feet are divided into two rooms, one of which is at present used as an office, the other, 25 by 14, is intended

for a laboratory. The moveable roof is 40 feet long, and runs on rails laid the whole length of the walls; the telescope-room may therefore be completely covered in, and as completely uncovered when required, the roof in the latter case resting on the south building, which on that account has a very low permanent roof.

The roof is constructed of six triangular wrought-iron principals, cross-braced, which abut at each side on a broad horizontal plate formed of two parallel lengths of stout angle-iron, connected at various points by iron bands; for additional strength, a broad vertical plate is bolted to the outer angle-iron piece. There are four pairs of wheels, 26 inches in diameter, flanched on the inside; these lie along the middle of the horizontal plate, the journals being bolted to the angle-iron pieces which form the plate.

The roof is covered with galvanized corrugated iron; it is therefore on the whole a somewhat heavy affair. The mechanical arrangements for moving are, however, simple and effective; a stout iron shaft runs across the building, and gears by wheel and pinion on the axles of the two south end wheels; to this shaft is fixed a spoked hand-wheel, by means of which the operator readily sets the roof in motion, and standing on a small platform connected therewith, is himself carried along at the same time.

The design of the roof is due to Mr. Merrett, of the Works department. On the whole, there is much to be said in favour of this rectangular form of roof: the temperature even in this climate frequently descends too low to be pleasant; but the occasional bodily inconvenience produced thereby is more than counterbalanced by perfect freedom to the observer, and the gratification of knowing that the instrument is in the best possible conditions for satisfactory performance. Only one really serious annoyance have I found connected with complete exposure; I allude to occasional heavy dew rendering it almost impracticable to use the sketching and other papers, the speculum meanwhile remaining free from deposit if precaution is taken not to work at too great an altitude.

The telescope, when housed, lies meridionally on the east side of the pier, and nearly in a horizontal direction, provision having been made to prevent the tube being lowered beyond a certain small inclination.

The piers are in keeping with the massiveness of the instrument; they are constructed of large, not to say huge blocks of basalt axed to a fine surface, altogether a substantial and beautiful piece of work.

The height of the walls with reference to the piers is such that very little of the sky range is curtailed. The north wall cuts off objects having a lesser altitude than about 10°. When resting on the east or west walls the telescope is nearly horizontal; in both these directions trees interfere, especially on the west side, where the ground rises. This curtailment will probably be a matter of very small importance, as with a four-feet

aperture observations at low altitudes are almost impracticable, and would probably never have to be resorted to except in the case of comets. The roof itself cuts off some of the range near the subpolar meridian; this, again, is not likely to be of much consequence.

The steam-engine, polishing-machine, and crane have been mounted in the room devoted to them; this room adjoins and is on the same floor (raised 4 feet from the ground, and 3 to 6 feet from the floor of the other rooms) as the telescope-room. To the east end of this machine-room, and communicating therewith, a small lean-to boiler-house has been added; in the west wall is a window which, when open, will leave sufficient clear space to admit of viewing a distant nearly horizontal object for the purpose of testing the mirrors.

The large speculum (A) was originally attached to the tube in its varnished condition; on the first favourable occasion it was taken down and unvarnished—a process which proved more troublesome than had been anticipated. The lac was very refractory, and the difficulty of removal exaggerated by the extreme heat then prevalent; after a process of solution in alcohol, mopping up, and washing with water frequently repeated, although there seemed no lac which would still dissolve, a large number of markings caused originally by the varnish brush were apparent, and the whole surface had an unpleasant mealy appearance.

It was thought, however, that the light lost would not prove serious, and in any case it did not seem that any further operation except polishing would improve matters; the speculum was therefore remounted and tried; and although it was of course impossible to say what would have been the effect of a more perfect polish, the views given of the brighter nebulæ were grand in the extreme, and left nothing to be desired.

By degrees, however, and without much exposure, the surface became more and more tarnished, with evident effect on the performance.

In the meantime the second mirror (B) had been unvarnished; in this case naphtha was used as the solvent, the solution mopped up, and the surface washed with soap and water. After a frequent repetition of this process, the surface seemed clear of impurities, and though not so bright as I had frequently seen it in Mr. Grubb's workshop, there were no signs of mealiness, the only unpleasant casualty being a considerable pitting of two patches some two inches square, produced by droppings from the muriate used in soldering the tin cover. These pittings are deep and unsightly; but the extent of surface corroded is comparatively so small that the effect must be inconsiderable.

The specula were exchanged about two months ago, and A put on the machine; but nothing has yet been done towards repolishing, as the necessary arrangements have not been got together for performing that delicate operation with due convenience.

Of work done, I cannot yet speak with any satisfaction since it became at all practicable to use the telescope; the history which I have to relate is

a long chapter of weary heart-breaking watchings, with an occasional half hour's work.

η Argo was the first object observed for purpose of delineation; after the first night's work little (and that by snatches) was done towards it, a new inroad of workmen and a long course of extremely unfavourable weather having carried the nebula out of convenient reach. The search, which was reluctantly given up, will, however, be again soon resumed.

I enclose two sketches, 4403 and 3570, of the 1864 catalogue.

4403. The horseshoe nebula is a grand object, conspicuous and with shape even in the finder (Plate I.). In the sketch the principal stars are laid down from measured position-angles about different centres; they are not as accurate as I could wish, and will be reobserved differently under better conditions; in no case, however, can there be sufficient error to influence in any material degree the configurations of the nebula or the smaller stars sketched in by eye.

It will be seen that the sketch contains considerably more detail than the corresponding figure in Herschel's Catalogue; there appears, however, to be no marked difference (with perhaps one exception) which may not be accounted for by the difference of aperture used.

The exception to which I allude is the presence of a small but conspicuous double star at the s. p. angle of the knot which lies between the ♉ and the bright streak; the experiment has not been tried of cutting down the aperture to approximate to an 18-inch Herschelian, but the intrinsic brightness of the principal star, and the presence in the C. G. H. of stars not more bright (No. 3 of Herschel's catalogue is certainly less bright) go far to show, without this experiment, that the star did not exist as such with its present brilliancy at the time of the C. G. H. and P. T. 33 observations . . . I have not seen Mr. Mason's drawing, but look forward with much interest to examining it and his remarks thereon.

The important position of the star, and the careful scrutiny which the knot and its neighbourhood must have repeatedly undergone, forbid the assumption that it was simply overlooked by Sir John Herschel.

The star β (I keep to Sir John Herschel's numbers and letters) is conspicuously and beautifully double, the companion of considerable brilliancy, about 15 mag.; with its present brilliancy and elongation it should, I think, be within reach of an 18-inch.

The knot is what I presume should be called resolvable; the appearance is sparkling, though no discrete stars can be seen, except perhaps a second faint one, which is suspected at the s. f. angle. Part of the streak near to the knot is also sparkling, but not in so marked a manner; the other portions appear of the ordinary milky nebulosity.

The fainter nebulosity (S) of the bright streak pretty well marks out the borders of the almost vacuous lane which leads up to and past the knot. On receding from the lane it becomes very faint: nor is this faintness uniform; but the appearances are so fugitive that, after repeated and painful

effort, I have been unable to catch them; the borders, however, stretching to the stars, as in the figure, are occasionally pretty well seen. On one or two occasions I have suspected the existence of a link between the nebulosity about the star No. 10 and the lower portion of the ʊ; this, however, requires verification.

At the f. end the upper and smaller semicircle is plainly marked, the lower and larger very faint, and consequently its exact figure uncertain; there is certainly some very faint nebulosity leading through the groups of stars north of the three bright f. end stars, but it has not been added to the sketch on account of its uncertain figure and extreme faintness.

3570. A small but beautiful spiral. The two brighter knots are resolvable; the greater brightness of these knots is not particularly shown in Sir John Herschel's sketch (Plate I.), but is mentioned in the observations; the general ground is only slightly nebulous.

Of work out of the regular course, amongst other things, Neptune has been observed on some five or six occasions for figure and a second satellite, with only negative results.

In the absence of a photographic apparatus to be used at the uninterrupted focus of large mirror, attempts have been made to utilize the 2nd or Cassegrain image; an average exposure of near ten minutes on an eight-day moon produced pictures which (by no means good) were of sufficient promise to make it worth while to resume the attempt under more favourable conditions.

The time of exposure is somewhat surprising, and would seem to accuse a great loss of chemical rays by a second perpendicular reflexion; but perhaps the more legitimate conclusion would be that the inactivity was mainly due to absorption at the surface of the large mirror, which was then very yellow.

The spectroscope arrived some time ago, but has not been much used; it is thought that for *star* work of any value some modification will be required, principally the exchange of the present collimator for one of longer focal length. A greater dispersion, moreover, seems desirable; for nebular work, however, for which it was mainly designed, the spectroscope in its present form, which is handy and compact, will be of much service.

For spectroscopic work on objects having a sensible diameter, the great telescope itself labours under some disadvantages; the enormous focal length and consequent magnification of the image is a serious inconvenience in the case of faint objects, and may be only partially remedied by a suitable condenser. This magnifying of the image may, however, in some cases be advantageous: I allude to the possibility thereby afforded of viewing small definite portions of moderately bright objects; unfortunately the objects with which we have to deal are seldom of such a character.

Of nebulæ, Orion has been examined for purpose of practice. The three lines are plainly and conspicuously seen; the hydrogen line is comparatively much fainter than I had anticipated, and disappears in the fainter

portions of the nebula. 30 Dorado shows the nitrogen line with facility; the second line certainly, but not in all positions, and always with difficulty; the hydrogen line is suspected only. I can see no trace of a continuous spectrum.

η Argo has been observed on only one unfavourable morning; the nitrogen line was seen over a considerable space; of the presence or absence of others, or of a continuous spectrum, I am unable to speak with certainty.

With respect to future operations, it is intended that at first the routine work shall consist of a detailed delineation of the objects figured by Sir John Herschel, or any others which may prove interesting: this will take some time; for even without the impediment of cloudy weather, the delineation, with any degree of satisfactory correctness, of a moderately large nebula requires a considerable amount of work and careful and frequent scrutiny. It is hoped, however, that this work will by practice be found less painfully difficult than it is at present.

The spectroscope will be used as much as possible, the moon photographed, and attempts made to photograph the nebulæ, when a photographic apparatus has been procured, and staging, photographic room, &c. added to the building. It is, moreover, hoped that before long a refractor, of some nine inches aperture, may be procured, to be mounted with the reflector, or, preferably, as a separate instrument.

This telescope, besides being of much general use, will find much and valuable employment in determining micrometrically the chief points in the nebulæ under examination with the reflector, with more expedition and accuracy than at present; for spectroscopic work this telescope would be a valuable adjunct, especially if it be constructed of such comparatively short focal length as seems now to be practicable.

The great interest which the Royal Society has taken in everything connected with the Melbourne reflector is my sole apology for sending thus early such a meagre account.

February 24, 1870.

Lieut.-General Sir EDWARD SABINE, K.C.B., President, in the Chair.

The following communications were read:—

I. "Note on certain Lichens." By JOHN STENHOUSE, LL.D., F.R.S., &c. Received January 10, 1870.

Through the kindness of W. Carruthers, Esq., of the Botanical Department of the British Museum, I obtained a considerable quantity of lichens from the neighbourhood of Moffat in Scotland. These were *Cladonia rangiferina*, and a mixture of *Usnea barbata* and *Evernia prunastri*,

the latter of which were carefully separated by picking—a somewhat tedious operation, as they were much interlaced.

Usnea barbata.

In order to extract the usnic acid from this lichen, it was macerated for about thirty minutes with a dilute solution of sodic carbonate, squeezed, again treated once or twice in a similar manner, and the turbid solution precipitated by a slight excess of hydrochloric acid. The crude dark-green acid thus obtained was mixed with milk of lime and a considerable quantity of warm water (40° C.), filtered, and the clear lemon-coloured solution of usnate of calcium acidulated with hydrochloric acid. The acid was thus precipitated in pale yellow flocks, which were collected.

The reason that I adopted this modification of the process I formerly proposed* is, that although usnic acid readily dissolves in milk of lime when it has been extracted, yet in order to exhaust this and other compact lichens, it requires to be treated a great many times if lime be employed, whilst two or three are sufficient with carbonate of sodium.

The partially purified usnic acid obtained in the manner above described was easily rendered quite pure by taking advantage of the peculiar property which this acid possesses of forming an insoluble calcium salt when boiled with lime. The crude yellow acid was placed in a flask with a quantity of water and an excess of slaked lime, and the mixture boiled for twenty minutes or half an hour. The insoluble calcium usnate was collected, well washed with hot water, and the lime then removed by boiling it with a slight excess of hydrochloric acid. The tolerably pure usnic acid was then collected, and well washed with boiling water. It was advisable to continue the digestion with hydrochloric acid for half an hour, as it rendered the acid more compact and easy to collect. By this alternate treatment with lime and acid, a large quantity of some dark-coloured impurity was removed. This forms the best process for recovering usnic acid in a state of tolerable purity from residues.

It was found, however, to be better, when considerable quantities of usnic acid were to be prepared, to boil the pale yellow usnic acid paste, as obtained by precipitation from the lime solution, with a small quantity of water, to which strong caustic soda solution was gradually added, sufficient to dissolve nearly the whole of the usnic acid. It was then set aside to crystallize, and when cold the very dark coloured supernatant mother liquor decanted, and the crystals of sodic usnate washed once or twice by decantation, with a small quantity of cold water. It was then redissolved and recrystallized once or twice in the same manner.

The nearly pure sodic usnate was now dissolved in a considerable quantity of hot spirit, filtered, and the boiling solution strongly acidulated with acetic acid. The usnic acid then separated in fine needles, which when cold were collected, well washed with cold spirit (in which they are almost

* Ann. der Chem. und Pharm. vol. lxviii. p. 98.

insoluble), and recrystallized from boiling spirit to render them quite pure.

When the quantity of acid operated on was but small, the best process was to dissolve it, by means of caustic soda solution, in a large quantity of boiling spirit, filter from the insoluble impurities, and strongly acidulate with acetic acid. The nearly pure usnic acid, which crystallizes out in large needles when the solution cools, was collected, washed, and recrystallized two or three times from spirit.

I. ·130 grm. usnic acid gave ·298 grm. carbonic anhydride and ·060 grm. water.

II. ·245 grm. usnic acid gave ·564 grm. carbonic anhydride and ·188 grm. water.

					I.	II.	Hesse.
C_{18}	=	216	=	62·43	62·53	62·79	62·80
H_{18}	=	18	=	5·20	5·13	4·99	5·00
O_7	=	112	=	32·37			
		346		100·00			

I. was purified by boiling with lime, and II. by repeated crystallization of the crude acid from spirit.

In the analyses published by W. Knop, Rochleder, and Heldt, and also by myself in 1848, the carbon is about ·75 per cent. higher than the above, and the formula deduced from it was $C_{19}\ H_{16} O_7$. Hesse* from his analyses proposed the formula $C_{18}\ H_{18}\ O_7$, which I have adopted.

Usnate of Sodium.

This was best prepared by adding one part pure usnic acid to twenty of boiling water, and then sufficient caustic soda solution to dissolve nearly the whole of the acid, filtering, and setting aside to crystallize.

After one recrystallization it was subjected to analysis.

I. ·598 grm. usnate of sodium gave ·114 grm. sulphate of sodium.

II. ·864 grm. usnate of sodium gave ·168 grm. sulphate of sodium.

					I.	II.	Mean.
C_{18}	=	216	=	58·70			
H_{17}	=	17	=	4·61			
Na	=	23	=	6·25	6·18	6·30	6·24
O_7	=	112	=	30·44			
		368		100·00			

This salt crystallizes in pale yellow silky needles, is not very soluble in cold water, but more so in spirit. It is readily decomposed by carbonic anhydride; so much so, that when pure sodium usnate is exposed for some time to the atmosphere, it absorbs carbonic acid, and is no longer completely soluble in water. By passing a current of carbonic anhydride through its aqueous solution, the usnic acid is entirely precipitated.

* Ann. der Chem. und Pharm. vol. cxvii. p. 345.

Calcium Usnate.

When pure usnic acid was moistened with spirit, and then rubbed up in a mortar with milk of lime, it combined and formed a deep yellow paste, which, on the addition of more water and filtration, yielded a lemon-coloured solution, containing calcium usnate, and hydrate. When this solution was heated it became turbid, and after boiling some time, the whole of the usnic acid was deposited as an insoluble calcium compound, in the form of small deep yellow rhomboidal crystals. Although I made several analyses of this compound, prepared at different times, I was unable to obtain it of a constant composition, probably owing to its being mixed with variable quantities of calcium carbonate and hydrate.

The formation of this insoluble calcium salt is very characteristic of usnic acid, and is an excellent test of its presence. As with the sodium salt, carbonic anhydride entirely decomposes the calcium compound. Usnic acid appears, therefore, to be a very feeble acid.

An attempt was made to prepare *ethylic usnate* by treating usnate of silver with ethylic iodide, but without success. When usnic acid was treated with bromine it was completely decomposed, and converted into an orange-coloured uncrystallizable resin.

Evernia prunastri—Evernic Acid.

The evernic and usnic acids that this lichen contains were extracted by the lime process, which consists in macerating the lichen two or three times successively with milk of lime for about half an hour each time. The solution of the mixed acids was then filtered, precipitated by a slight excess of hydrochloric acid, and the precipitate collected and dried. In order to extract the evernic acid from the mixture, it was agitated for about five minutes with four parts boiling alcohol and filtered. The acids remaining undissolved were treated two or three times with the same quantity of boiling alcohol, and the dissolved evernic acid precipitated by the addition of an equal bulk of water. By this means the evernic acid, being readily soluble in boiling alcohol, was in a great measure separated from the usnic acid, which dissolves with difficulty in that menstruum unless digested with it for a considerable time. The crude evernic acid thus obtained amounted to about one-third of the mixed acids, and was purified by repeated crystallization from strong spirit, taking care not to digest it for any length of time. The process is much facilitated by completely removing the mother liquors by Bunsen's vacuum filter.

Pure evernic acid, as has been already described by myself* and Hesse†, consists of aggregations of minute needles, melting at 164° C. It is a feeble acid, and does not decompose solutions of bicarbonate of sodium in the cold; as, however, the adhering colouring-matter is somewhat soluble in that menstruum, it may be employed to free the crude acid to a great

* Ann. der Chem. und Pharm. vol. lxviii. p. 84.
† Ibid. vol. cxvii. p. 298.

extent from that impurity. The solution of calcium evernate is decomposed by a long-continued current of carbonic anhydride, which precipitates calcic carbonate and unaltered evernic acid.

On theoretical grounds it has been stated* that, by the action of potassic or baric hydrate, evernic acid should be resolved into orsellinic and everninic acids. This prediction, however, is incorrect, as I find, as formerly stated†, that everninic acid is the only fixed product.

Tetrabrom-evernic Acid.

Perfectly dry and finely powdered evernic acid was treated in the cold with a slight excess of dry bromine, large quantities of hydrobromic acid were given off, and a brominated compound produced. In order to prevent any portion of the acid escaping bromination, the product was finely powdered and again treated with bromine. After standing some time to allow the excess of bromine to volatilize, the finely powdered compound was well washed with bisulphide of carbon, to remove the last traces of bromine, and a small quantity of a resinous body which is produced at the same time. Two or three crystallizations from boiling spirit render it quite pure. When subjected to analysis, it gave the following results:—

I. ·312 grm. acid gave ·362 grm. carbonic anhydride and ·067 grm. water.

II. ·321 grm. acid gave ·373 grm. bromide of silver.

					I.	II.
C_{17}	=	204	=	31·48	31·64	
H_{12}	=	12	=	1·86	2·03	
Br_4	=	320	=	49·48		49·44
O_7	=	112	=	17·28		
		648		100·00		

This analysis agrees very well with the formula $C_{17} H_{12} Br_4 O_7$, four equivalents of hydrogen in evernic acid being replaced by bromine.

Tetrabrom-evernic acid is rather soluble in hot alcohol, from which it crystallizes on standing some time in small colourless prisms. It is insoluble in water and bisulphide of carbon, slightly soluble in hot benzol, and readily in ether, which when quickly evaporated leaves it as a transparent colourless resin; it melts at 161° C. The acid is very soluble in alkaline solutions, which on evaporation dry up to a gummy mass. When heated with concentrated sulphuric acid it decomposes.

Usnic Acid from Evernia prunastri.

The usnic acid left undissolved in the preparation of evernic acid usually retained traces of that acid even after repeated treatment with alcohol; but this was entirely removed by boiling with lime, as described in the first part of this paper. This decomposed and removed the evernic acid and other impurities, leaving the usnic acid in the form of an insoluble calcium

* Watts's Dict. Chem. vol. ii. p. 611.

† Ann. der Chem. und Pharm. vol. lxviii. p. 86.

salt. The acid when freed from lime and purified, melted at 202° C., and by analysis gave the following results :—

I. ·409 grm. usnic acid gave ·939 grm. carbonic anhydride and ·188 grm. water.

					I.
C_{18}	=	216	=	62·43	62·63
H_{18}	=	18	=	5·20	5·11
O_7	=	112	=	32·37	
		346		100·00	

From the above analyses it will be seen that the usnic acid from *Evernia prunastri* is identical in composition with that from *Usnea barbata.* It has the same melting-point, and agrees with it in all its other properties.

Cladonia rangiferina.

In 1848 * I extracted the lichen acid from *Cladonia rangiferina,* and by analysis found it to have the same composition as usnic acid, with which it agrees very closely in its properties. Hesse, however, observed† that this acid had a different melting-point (175° C.) from ordinary usnic acid (203° C.), and proposed, therefore, as it so closely resembled ordinary usnic acid in its general character, to call it β-usnic acid.

Cladonic Acid, β-orcin.

I formerly obtained‡ β-orcin by subjecting to destructive distillation a mixture of the acids from *Cladonia rangiferina* and various species of *Usnea*; but I have lately found that ordinary usnic acid, melting at 203° C., obtained from *Evernia prunastri, Ramalina calicaris,* and the various *Usneas,* does not yield a trace of β-orcin when distilled, whilst, on the contrary, the acid extracted from *Cladonia* (Hesse's β-usnic acid melting at 175° C.), on being subjected to the same treatment, yields β-orcin, thus showing a marked difference in the products of its decomposition from ordinary usnic acid, as well as in its melting-point. Under these circumstances, therefore, I think that it would be better to name the acid from *Cladonia rangiferina* "Cladonic Acid," instead of β-usnic acid, as proposed by Hesse.

I expected to have been able to subject cladonic acid to a more careful examination, and procured for that purpose a quantity of *Cladonia rangiferina* from the neighbourhood of Moffat. Unfortunately, however, it was not gathered until the beginning of December, and I was surprised to find that it contained scarcely a trace of cladonic or any similar acid. I intend to obtain a new quantity next summer, when I hope to be more successful.

I cannot conclude this paper without acknowledging the efficient assistance I have received from Mr. Charles E. Groves.

* Ann. der Chem. und Pharm. vol. lxviii. p. 98.
† Ibid. vol. cxvii. p. 347.
‡ Ibid. vol. lxviii. p. 104.

II. "On the successive Action of Sodium and Iodide of Ethyl upon Acetic Ether." By E. Frankland, F.R.S., and B. F. Duppa, Esq., F.R.S. Received January 13, 1870.

In a paper by Mr. J. Alfred Wanklyn, bearing the above title, and published in the Proceedings of the Royal Society, vol. xviii. p. 91, the author refers to our memoir on the same subject printed in the Philosophical Transactions for 1866, vol. clvi. p. 37, and expresses his opinion that our interpretation of the nature of the reaction must be erroneous because it involves the disengagement of hydrogen. This opinion is founded upon certain experiments which Mr. Wanklyn has himself made, and which are described in the number of Liebig's 'Annalen' for January 1869, and in the Chemical Society's Journal, vol. ii. p. 371.

In reference to this opinion we have to remark, first, that it is founded upon experiments which differ essentially from our own; and, second, that even the results obtained in those experiments by the author do not warrant the conclusion, at variance with ours, which he has drawn from them, viz. that the evolution of hydrogen in this reaction is inadmissible.

The reaction, the theoretical explanation of which Mr. Wanklyn seeks to controvert, is described in the Philosophical Transactions, vol. clvi. p. 38, as follows:—"When acetic ether is placed in contact with sodium it becomes hot, and a considerable quantity of gas is evolved, which, after being passed first through alcohol and then through water, burns with a non-luminous flame, and the products of combustion do not produce the slightest turbidity on agitation with baryta-water. In fact the gas is pure hydrogen. When the action is complete, the liquid solidifies on cooling to a mass resembling yellow beeswax. By putting the sodium into the acetic ether as just described, it is difficult to conduct the operation to completion, owing to the liquid gradually assuming such a thick and pasty condition as to prevent the further action of the sodium." Owing to the difficulty of carrying the reaction far enough in this way we frequently employed a modification of this process, which is minutely described in the same memoir. The modification consisted in placing the sodium in a separate vessel and causing the acetic ether to distil continuously over it; thus the portions of acetic ether still unacted upon were brought, again and again, into contact with the sodium, whilst the non-volatile product of the operation was retained in a lower vessel. As we acted upon several pounds of acetic ether at once, the operation frequently lasted several days, and *during the whole time torrents of hydrogen were evolved.* The temperature of the liquid in the distillation vessel was allowed to rise to 130° C., and the amount of sodium consumed was not much less than one atom for each molecule of acetic ether employed.

We have made several attempts to determine quantitatively the volume of hydrogen given off from a known weight of sodium, and also from a known weight of acetic ether, but in neither operation could we obtain a

trustworthy result. In the first case because the sodium, which fuses during the reaction, breaks up into a vast number of very minute globules, the final disappearance of which in the highly coloured and pasty product it is impossible to verify. In the second case because the thickening of the liquid prevents the reaction being pushed far enough to decompose the whole of the acetic ether employed. In a quantitative experiment, in which 4·857 grammes of acetic ether were acted upon by sodium in slight excess, 344·79 cub. centims. of hydrogen at 0° C. and 760 millims. pressure were obtained. If one molecule of acetic ether had lost one atom of hydrogen, 615·9 cub. centims. of gas ought to have been collected. It was evident, however, that a large proportion of acetic ether still remained unattacked at the close of the experiment.

Such, then, was our mode of operating; the hydrogen evolved was allowed freely to escape, the whole process was conducted at the ordinary atmospheric pressure, and the temperature varied from the boiling-point of acetic ether to 130° C. Moreover the acetic ether used was prepared with the greatest care so as to ensure the absence of alcohol and water. By our method of preparation, described in the memoir already cited, no traces of the former could be detected even in the crude ether; nevertheless it was first placed for several days over fragments of fused calcic chloride, which apparently remained perfectly dry and unaffected; it was then in some cases boiled for ten days or a fortnight upon many pounds of sodium-amalgam, which we find to be entirely without action upon pure acetic ether, whilst it rapidly attacks and removes alcohol, if the latter be added even in very small proportion to the acetic ether. When acetic ether, so treated and then distilled from the sodium-amalgam, was brought into contact with the sodium, an abundant evolution of hydrogen immediately commenced, and continued during the entire treatment, which, as already remarked, frequently lasted several days. The general impression, however, produced upon us by the whole of our operations was, that the evolution of hydrogen was not quite so great as that theoretically required by the reactions which we believe to take place; nevertheless it was obvious that no equations, from which free hydrogen was excluded, could possibly correctly express the chemical changes effected in this action. Certain experiments were undertaken to trace the missing hydrogen, but as they have not hitherto been completed we will not further allude to them here.

We now turn to Mr. Wanklyn's mode of experimenting. This is not stated in his communication to the Royal Society, but is given in the Journal of the Chemical Society, vol. xvii. p. 371, and in the Ann. Chem. u. Pharm. for January 1869, as follows:—

Exp. 1. "I sealed up a quantity of sodium with acetate of ethyl, *which had been very carefully deprived of alcohol and water*, and weighed the tube containing these materials. I then heated the tube to 130° C. for some time, until the contents had changed from liquid to solid. After opening the tube and allowing any gas that might have formed to escape, I weighed it again. *The loss amounted to* 0·5 *in* 100 *parts of acetic ether.*"

Exp. 2. "5 cub. centims. of good acetate of ethyl and 0·3 grm. of sodium were sealed up in a small glass tube and heated in a water-bath to 100° C. until all the sodium had disappeared. The tube was then opened under water; *the evolved gas measured* 25 cub. centims. at ordinary temperature, but at 0° C. and 760 millims. pressure and dry it measured 23 cub. centims. If the volume of hydrogen be calculated, which is equivalent to 0·3 grm. sodium, it will be found to be 140 cub. centims."

Exp. 3. "Another specimen of acetic ether, which was prepared with greater care, evolved no gas by the action of potassium or sodium."

It is thus evident that whilst we allowed all evolved gas freely to escape, Mr. Wanklyn operated in sealed tubes under great pressure,—an alteration in the conditions of the experiment which might well lead to a modification of the result. Mons. L. Cailletet has recently shown that the evolution of hydrogen from zinc and hydrochloric acid is gradually diminished and finally stopped under increasing pressure; and the same chemist also finds that the evolution of hydrogen from sodium-amalgam and water is diminished and finally stopped in a sealed tube. It follows from these experiments that pressure retards or even interrupts a reaction in which a permanent gas is evolved, whilst it is known to exercise little or no influence upon other chemical changes in which no evolution of gas takes place. This influence of pressure upon certain kinds of chemical action affords an explanation of the difference between the results of Mr. Wanklyn's experiments and our own, as regards the evolution of hydrogen during the action of sodium upon acetic ether. We can confirm his observation that sodium dissolves in valeric ether, under ordinary atmospheric pressure, without the evolution of any gas. A reaction, whatever its nature may be, which thus proceeds readily with ethylic valerate can scarcely be impossible with its homologue, acetic ether, and it is probable that this reaction goes on side by side with those which we have described in our memoir; but when the pressure is moderate those changes chiefly take place which involve the disengagement of hydrogen, whilst under the great pressure arising in sealed tubes these changes are more or less suppressed, and the reaction observed by Mr. Wanklyn comes into prominence.

Lastly, Mr. Wanklyn's own experiments scarcely justify his unqualified opinion that "equations which assume evolution of hydrogen in these reactions are inadmissible." In two out of three of his experiments, hydrogen in considerable quantity was evolved; and although in experiment No. 2, given above, he attributes the hydrogen to the presence of alcohol, yet in experiment No. 1 its origin cannot be so explained, as he states expressly that the acetic ether employed "had been very carefully deprived of alcohol and water;" yet the proportion of hydrogen evolved in this case was much larger than in experiment No. 2.

We reserve our observations upon Mr. Wanklyn's views regarding the changes which take place when sodium acts upon acetic, butyric, and valeric ethers, until the publication of the experimental data upon which those views are founded.

March 3, 1870.

Lieut.-General Sir EDWARD SABINE, K.C.B., President, in the Chair.

In accordance with the Statutes, the names of the Candidates for election into the Society were read as follows:—

William Baker, C.E.
Edward Middleton Barry, R.A.
Rev. Francis Bashforth, B.D.
Bernard Edw. Brodhurst, F.R.C.S.
Samuel Brown, P.I.A.
James Brunlees, C.E.
Frank T. Buckland, M.A.
George William Callender, F.R.C.S.
Commander William Chimmo, R.N.
Frederick Legros Clark, F.R.C.S.
Henry Dircks, Esq.
Alexander Fleming, M.D.
Peter Le Neve Foster, M.A.
Sir Charles Fox, C.E.
William Froude, M.A.
Thomas Minchin Goodeve, M.A.
Edward Headlam Greenhow, M.D.
Edmund Thomas Higgins, F.R.C.S.
Rev. Thomas Hincks, B.A.
Charles Horne, Esq.
Rev. A. Hume, LL.D.
James Jago, M.D.
William Stanley Jevons, M.A.
George Johnson, M.D.
M. Kelburne King, M.D.
James Atkinson Longridge, C.E.
Nevil Story Maskelyne, M.A.
Maxwell Tylden-Masters, M.D.
Thomas George Montgomerie, Major R.E.
Alfred Newton, M.A.
Andrew Noble, Esq.
Thomas Nunneley, F.R.C.S.
Edward Latham Ormerod, M.D.
Capt. Sherard Osborn, R.N.
Rev. Stephen Parkinson, B.D.
Capt. Robert Mann Parsons, R.E.
William Overend Priestley, M.D.
Charles Bland Radcliffe, M.D.
William Henry Ransom, M.D.
Edward James Reed, C.B.
William James Russell, Ph.D.
Robert H. Scott, Esq.
John Shortt, M.D.
Edward Thomas, Esq.
Cromwell Fleetwood Varley, C.E.
George Frederic Verdon, C.B.
Augustus Voelcker, Ph.D.
Arthur Viscount Walden, P.Z.S.
George Charles Wallich, M.D.
A. T. Houghton Waters, M.D.
Samuel Wilks, M.D.
Capt. Charles William Wilson, R.E.
John Wood, F.R.C.S.

The following communications were read:—

I. "Results of the Monthly Observations of Dip and Horizontal Force made at the Kew Observatory, from April 1863 to March 1869 inclusive." By BALFOUR STEWART, F.R.S., Superintendent of the Observatory. Received January 26, 1870.

1. In a communication made to this Society by the President, Sir E. Sabine, and published in the Philosophical Transactions for 1863, page 273, we have the results of the monthly observations of Dip and Hori-

zontal Force made at Kew for the six years ending March 1863. In the present communication it is proposed to reduce in a similar manner the results of the following six years.

The mode of observation has already been so fully described by Sir E. Sabine that no further account of it is necessary; suffice it to say that in October 1863 Mr. Chambers left the Observatory for India, and that Mr. George Whipple took his place as Magnetical Assistant. Mr. Whipple has since continued to observe every month with the utmost care and assiduity, employing the same instruments that were used by his predecessor, and the same methods of observation and reduction. A smaller number of observations by other instruments and other observers have likewise been made, but it has been thought desirable to limit this discussion to the series made by the regular observer. I ought likewise to state that both the dip-circle and unifilar stood in need of slight repairs, and that they were put into the hands of opticians for this purpose; the first observation with the repaired dip-circle being that of January 1869, and with the repaired unifilar that of December 1868.

I. Dip.

2. In Table I. we have a record of the observed values of dip made with circle No. 33 by Barrow, each observation being the mean of two made with the two needles belonging to that circle.

Table I.—Observed values of the Magnetic Dip at Kew.

	1863.	1864.	1865.	1866.	1867.	1868.	Mean of 6 years.
	° ′	° ′	° ′	° ′	° ′	° ′	° ′
April	68 15·1	68 9·4	68 7·7	68 5·8	68 3·8	68 3·4	68 7·53
May	11·0	8·7	6·6	5·9	2·9	1·1	6·03
June	10·1	10·1	9·8	4·8	2·3	1·1	6·37
July	10·8	10·8	10·0	4·9	2·6	1·8	6·82
August	14·6	9·6	9·4	4·0	1·8	1·9	6·88
September	68 13·2	68 10·0	68 10·1	68 7·0	68 0·2	68 3·4	68 7·32
	68 12·47	68 9·77	68 8·93	68 5·40	68 2·27	68 2·12	68 6·83

	1863–64.	1864–65.	1865–66.	1866–67.	1867–68.	1868–69.	Mean of 6 years.
	° ′	° ′	° ′	° ′	° ′	° ′	° ′
October	68 11·8	68 11·0	68 9·9	68 6·5	68 2·8	68 5·6	68 7·93
November	11·8	8·5	8·9	7·8	5·6	67 59·9	7·08
December	11·2	9·6	7·5	4·2	2·9	[68 0·8]	6·03
January	10·3	9·5	7·4	6·5	3·1	68 1·8	6·43
February	10·7	7·1	7·7	4·1	2·7	2·8	5·85
March	68 9·9	68 7·4	68 7·0	68 3·8	68 0·7	68 2·0	68 5·14
	68 10·95	68 8·85	68 8·07	68 5·48	68 2·97	68 2·15	68 6·41
	68 11·71	68 9·31	68 8·50	68 5·44	68 2·62	68 2·13	68 6·62

The number within brackets is interpolated.

3. The absolute values of the dip corresponding to the beginning of October in each of the years comprehended in the above Table and the secular change in each year are as follows:—

From April 1863 to March 1864	68° 11′·71		
„ „ 1864 „ „ 1865	68 9·31	secular change	−2′·40
„ „ 1865 „ „ 1866	68 8·50	„ „	−0·81
„ „ 1866 „ „ 1867	68 5·44	„ „	−3·06
„ „ 1867 „ „ 1868	68 2·62	„ „	−2·82
„ „ 1868 „ „ 1869	68 2·13	„ „	−0·49
Mean of the six years corresponding to middle epoch, April 1, 1866	68 6·62	with a mean annual secular decrease of 1′·92.	

4. The annual variation or semiannual inequality may be shown to be as follows:—

TABLE II.

Date.	Corrections for secular change.	68° 6′·62 ± secular change.	Observed values.	Observed *minus* calculated.	
				April to Sept.	October to March.
July 1, 1863 ...	+5′·28	68° 11′·90	68° 12′·47	+0′·57	′
Jan. 1, 1864 ...	+4·32	10·94	10·95	......	+0·01
July 1, 1864 ...	+3·36	9·98	9·77	−0·21	
Jan. 1, 1865 ...	+2·40	9·02	8·85	......	−0·17
July 1, 1865 ...	+1·44	8·06	8·93	+0·87	
Jan. 1, 1866 ...	+0·48	7·10	8·07	......	+0·97
July 1, 1866 ...	−0·48	6·14	5·40	−0·74	
Jan. 1, 1867 ...	−1·44	5·18	5·48	......	+0·30
July 1, 1867 ...	−2·40	4·22	2·27	−1·95	
Jan. 1, 1868 ...	−3·36	3·26	2·97	......	−0·27
July 1, 1868 ...	−4·32	2·30	2·12	−0·18	
Jan. 1, 1869 ...	−5·28	68 1·34	68 2·15	......	+0·81
Mean differences between the observed and calculated values in the respective semiannual periods				−0·27	+0·27

5. We therefore deduce from these six years' observations the existence of a semiannual inequality, in virtue of which the dip is on an average 0′·27 lower in the six months from April to September, and 0′·27 higher in the six months from October to March than is due to its mean value.

This result is in the same direction as that found by Sir E. Sabine for the six years ending March 1863, but is less in amount than the latter, that determined from the first six years exhibiting a range of 1′·31, while that determined from the last six years only exhibits a range of 0′·54.

6. As already mentioned, the observations for the first six years were

nearly all made by Mr. Chambers, and those for the last six years nearly all by Mr. Whipple.

From the first six years we deduce a mean dip equal to 68° 20'·07, corresponding to middle epoch April 1, 1860, and from the latter six, a mean dip equal to 68° 6'·62, corresponding to middle epoch April 1, 1866, while the secular change deduced from the first series is 2'·00, and that deduced from the last series 1'·92, the mean of these two values being 1'·96.

If we apply this mean value of the secular change to the mean result corresponding to the epoch April 1, 1860, in order to bring it to the epoch April 1, 1866, we obtain

$$68^\circ\ 20'{\cdot}07 - 11'{\cdot}76 = 68^\circ\ 8'{\cdot}31,$$

whereas that deduced from the second series corresponding to this epoch is 68° 6'·62.

The former of these is 1'·69 higher than the latter, and it may be desirable to investigate the cause of this difference.

7. In the first place, it cannot I think be due to any personal equation in the observer. Of late I have made occasional observations with the circles and needles used by Mr. Whipple, with the view of determining whether there is any personal peculiarity in the dip observations of either of us.

The mean of 12 such dips taken by me is .... 68° 3'·95
while the mean of 12 comparable dips taken by Mr. Whipple is 68° 3'·85
showing a difference of not more than 0'·1, which small difference may probably be occasioned by accidental disturbance rather than by personal peculiarity.

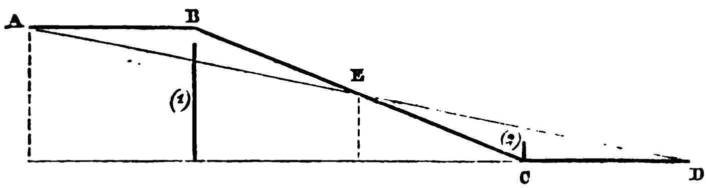
During the time when Mr. Chambers was at Kew no comparative observations were made with this particular object in view, and I cannot find a sufficient number of strictly comparable dips to determine with certainty what was the mean difference, if any, between his readings and mine. The result would, however, seem to indicate that his reading is rather lower than mine, certainly not more than half a minute, but probably much less.

There is therefore no reason for supposing that Mr. Whipple reads the dip to an appreciable amount lower than Mr. Chambers, so that the difference of 1'·69 cannot be accounted for by this supposition.

8. Sir E. Sabine has remarked as follows (rPoceedings of the Royal Society, Nov. 30, 1865, p. 491):—"The general effect of the disturbances of the inclination at Toronto is to increase what would otherwise be the amount of that element; therefore if the disturbances have a decennial period, the absolute values of the inclination (if observed with sufficient delicacy) ought to show in their annual means a corresponding decennial variation, of which the minimum should coincide with the year of minimum disturbance, and the maximum with the year of maximum disturbance." At Toronto, where the true secular change is very small, the effect of this

superimposed variation is very visible, so that the yearly values of the inclination appear to increase up to the period of maximum disturbance and to decrease after it. At Kew, the general effect of disturbances is probably the same as at Toronto, that is to say, tending to increase the inclination; but the secular change being considerable, and tending to decrease the inclination, the joint effect of the secular change and the superimposed variation might be expected to appear in a diminution of the yearly secular change for those years during which the disturbances are increasing from their minimum to their maximum value, and in an increase of the yearly secular change for those years during which the disturbances are decreasing from their maximum to their minimum.

The law of diminution of the dip at Kew due to the conjoint action of these two causes, may thus be graphically represented in the following exaggerated curve—



where B represents the epoch of maximum, and C that of minimum disturbance.

Also, we may regard A B E as denoting the first six years' results, and E C D those of the second six years, the epoch of maximum approximately falling about the middle of the first six years' observations, and the epoch of minimum about the middle of the second.

Now the slope of the line A E D represents the average secular change, also (1) represents the mean of dips deduced from the first series of six years, and (2) the mean of those deduced from the second series, (1) being above the line of average dip, and (2) being below it. From this it is evident that, in order to bring (1) to the same epoch as (2), we should have to apply to (1) a greater than the average secular change. But before this reasoning can be used to account for the difference of 1'·69, we must examine whether, as a matter of fact, in the Kew observations the secular change is less than the mean during periods of increasing disturbance, and greater than the mean when the disturbances are decreasing.

9. Taking the two series of six years as comprising the most regular and reliable observations made at Kew, and deducting the mean dip for 1857, in the Table prepared by Sir E. Sabine, from that for 1868 in Table I. of this paper, we find a mean secular change of 2'·07.

On the other hand, the actual yearly changes and their differences from the mean are as follows:—

TABLE III.

Date.	Observed secular change.	Observed *minus* average secular change.
	′	′
1857-58	2·31	+0·24
1858-59	1·15	−0·92
1859-60	2·12	+0·05
1860-61	1·87	−0·20
1861-62	2·53	+0·46
1862-63	3·18	+1·11
1863-64	2·40	+0·33
1864-65	0·81	−1·26
1865-66	3·06	+0·99
1866-67	2·82	+0·75
1867-68	0·49	−1·58

If we take the first three and the last two of the above differences as belonging to the years when the disturbances were increasing, we find a secular change less than the average by a mean of 0′·29; and if we take the remaining six differences as belonging to the years when the disturbances were decreasing, we find a secular change greater than the average by a mean of 0′·24.

It would therefore appear that the Kew observations present a peculiarity similar to those at Toronto, so that the difference of 1′·69 between the two sets of observations may probably be accounted for by this cause.

10. We may, in fact, exhibit the peculiarities of the graphical representation given above by means of the actual results. Thus, if we take the first year's dip (1857), or 68° 24′·87, and deduct from it every year 2′·07 (which is the average secular change), we shall obtain a series of yearly values representing the yearly positions of the line A E D; and if the diagram truly represents the facts, the observed yearly values ought to range above the line for the first six years, and under it for the second six.

We may see by the following Table that this is actually the case:—

TABLE IV.

Observed yearly values (1).	Yearly values of A E D (2).	(1)-(2).
° ′	° ′	′
68 24·87	68 24·87	0
22·56	22·80	−0·24
21·41	20·73	+0·68
19·29	18·66	+0·63
17·42	16·59	+0·83
14·89	14·52	+0·37
11·71	12·45	−0·74
9·31	10·38	−1·07
8·50	8·31	+0·19
5·44	6·24	−0·80
2·62	4·17	−1·55
68 2·13	68 2·13	0

On the whole, therefore, we have good evidence of a behaviour at Kew analogous to that at Toronto.

11. The probable error of a single monthly determination of the dip, derived from the seventy-two monthly determinations given in Table I., and after the application of the correction for secular change and annual variation, as derived from the results of these observations, has been made, is $\pm 0'\cdot 96$. There is, however, reason to believe that this probable error is increased to some extent by periods of disturbance, some of them of cons derable duration, which exhibit themselves when the residual errors have been deduced after the method indicated above. In order to test this, I have formed a series of seventy-two yearly values of the dip corresponding in epoch to the various monthly values of Table I.

These yearly values will, of course, average the semiannual inequality, while each yearly value may possibly be supposed to be affected to some extent with the same sort of disturbance which affects the monthly value to which it corresponds. Were both affected in precisely the same way by these disturbances, the differences between the monthly and yearly values would only be occasioned by the semiannual inequality and by errors of observation. It is, however, too much to expect that all effects of disturbance will be eliminated from the differences by this method; nevertheless we may naturally expect that they will be reduced in amount.

12. Grouping these differences together in six monthly periods, we obtain the following results corresponding to those given in Table II:—

Table V.

Date.	Observed *minus* Calculated.	
	April to September.	October to March.
July 1, 1863	−0·20	
January 1, 1864	....	−0·05
July 1, 1864	−0·13	
January 1, 1865	....	−0·15
July 1, 1865	+0·26	
January 1, 1866	....	+0·32
July 1, 1866	−0·71	
January 1, 1867	....	+0·68
July 1, 1867	−0·95	
January 1, 1868	....	+0·55
July 1, 1868	−0·14	
January 1, 1869	....	+0·30
Mean	−0·31	+0·28

It will be seen from this Table that the irregularities of the two last columns of Table II. are very much reduced by this process, while the result remains nearly the same.

The probable error of a single observation is also reduced, and becomes (when the correction for annual variation is applied to the individual dif-

ferences determined by this process) $\pm 0'.87$, instead of $\pm 0'.96$, which it was before.

II. Horizontal Force.

13. In Table VI. we have a record of the observed values of horizontal force made with the Kew unifilar by Mr. Whipple, each observation being made and reduced precisely after the manner of those described by Sir E. Sabine in his analysis of the first six yearly series.

Table VI.

Monthly values of the Horizontal Component of the Earth's Magnetic Force at Kew, calculated from the results of observations of deflection and vibration with Collimator Magnet K.C.I.

	1863.	1864.	1865.	1866.	1867.	1868.	Mean of 6 years.
April	3·8201	3·8240	3·8277	3·8338	3·8449	3·8464	3·8328
May	3·8199	3·8260	3·8251	3·8373	3·8459	3·8504	3·8341
June	3·8198	3·8246	3·8258	3·8383	3·8469	3·8495	3·8341
July	3·8260	3·8331	3·8330	3·8410	3·8427	3·8414	3·8362
August	3·8243	3·8264	3·8246	3·8384	3·8445	3·8511	3·8349
September	3·8205	3·8314	3·8298	3·8386	3·8467	3·8475	3·8358
	3·8218	3·8276	3·8277	3·8379	3·8453	3·8477	3·8346
	1863–64.	**1864–65.**	**1865–66.**	**1866–67.**	**1867–68.**	**1868–69.**	**Mean of 6 years.**
October	3·8142	3·8274	3·8271	3·8354	3·8446	3·8470	3·8326
November	3·8214	3·8243	3·8325	3·8410	3·8494	3·8503	3·8365
December	3·8218	3·8293	3·8360	3·8396	3·8475	3·8539	3·8380
January	3·8239	3·8276	3·8364	3·8443	3·8511	[3·8521]	3·8392
February	3·8242	3·8353	3·8335	3·8405	3·8492	3·8504	3·8388
March	3·8229	3·8319	3·8357	3·8403	3·8469	3·8521	3·8383
	3·8214	3·8293	3·8335	3·8402	3·8481	3·8510	3·8372
	3·8216	3·8284	3·8306	3·8391	3·8467	3·8493	3·8360

The value within brackets is interpolated.

14. The absolute values of the horizontal force corresponding to the beginning of October in each of the years comprehended in the above Table, and the secular change in each year are as follows:—

From April 1863 to March 1864	3·8216	secular change +0·0068
„ „ 1864 „ „ 1865	3·8284	„ „ +0·0022
„ „ 1865 „ „ 1866	3·8306	„ „ +0·0085
„ „ 1866 „ „ 1867	3·8391	„ „ +0·0076
„ „ 1867 „ „ 1868	3·8467	„ „ +0·0026
„ „ 1868 „ „ 1869	3·8493	
Mean of the six years corresponding to middle epoch, April 1, 1866	3·8360	With a mean annual secular increase of 0·00 55.

15. Forming now the following Table similar to Table II., we fail to detect in it any trace of semiannual inequality.

Table VII.

Date.	Correction for secular change.	3·8360 ± secular change.	Observed values.	Observed *minus* Calculated. April to September.	Observed *minus* Calculated. October to March.
July 1, 1863	−0·0152	3·8208	3·8218	+·0010	
January 1, 1864 ..	−0·0125	3·8235	3·8214	....	−·0021
July 1, 1864	−0·0097	3·8263	3·8276	+·0013	
January 1, 1865 ..	−0·0070	3·8290	3·8293	....	+·0003
July 1, 1865	−0·0042	3·8318	3·8277	−·0041	
January 1, 1866 ..	−0·0014	3·8346	3·8335	....	−·0011
July 1, 1866	+0·0014	3·8374	3·8379	+·0005	
January 1, 1867 ..	+0·0042	3·8402	3·8402	....	·0000
July 1, 1867	+0·0070	3·8430	3·8453	+·0023	
January 1, 1868 ..	+0·0097	3·8457	3·8481	....	+·0024
July 1, 1868	+0·0125	3·8485	3·8477	−·0008	
January 1, 1869 ..	+0·0152	3·8512	3·8510	....	−·0002
Mean difference between the observed and calculated values in the respective semiannual periods				+·0000	−·0001

16. Again, from the first six years we have a mean value of the horizontal force equal to 3·8034, corresponding to the middle epoch April 1, 1860, and from the latter six years' observations given above, we have, as has been shown, a mean value of horizontal force equal to 3·8360, corresponding to epoch April 1, 1866; also the secular change deduced from the first six years is +·0053, while that deduced for the second six is +·0055, the mean of the two being +·0054.

If we apply this mean value of the secular change to the mean result corresponding to epoch April 1, 1860 in order to bring it to epoch April 1, 1866, we obtain 3·8034+0·0324=3·8358, a value which agrees as nearly as possible with that deduced from the second series, and corresponding to the same epoch which, as we have seen above, was 3·8360.

17. The coincidence of these two values naturally leads us to imagine that the secular change of the horizontal force does not present the same peculiarity as that observed in the case of the dip, and exhibited in the diagram.

In order to test this, let us form for the horizontal force the following Table, similar to Table III.

Table VIII.

Date.	Observed secular change.	Observed *minus* average secular change.
1857-58	·0051	−·0003
1858-59	·0057	+·0003
1859-60	·0056	+·0002
1860-61	·0058	+·0004
1861-62	·0044	−·0010
1862-63	·0051	−·0003
1863-64	·0068	+·0014
1864-65	·0022	−·0032
1865-66	·0085	+·0031
1866-67	·0076	+·0022
1867-68	·0026	−·0028

If we take the first three and the last two of the above differences as belonging to years, when the disturbances are increasing, we find a secular change only less than the average by a mean of ·00008; and if we take the remaining six differences as belonging to years when disturbances are decreasing, we find a secular change greater than the average by a mean of ·00007; both being differences which form such an extremely small proportion of the whole change that they may be neglected.

18. Or again, if we take the first year's horizontal force (1857) or 3·7899, and add to it every year ·0054, which is the average secular change, we shall, as before, obtain a series of values representing the yearly positions of the line A E D in the diagram, from which we may construct the following Table similar to Table IV.

Table IX.

Observed yearly values.	Yearly values of A E D.	
(1)	(2)	(1)−(2)
3·7899	3·7899	0·0000
3·7950	3·7953	−0·0003
3·8007	3·8007	0·0000
3·8063	3·8061	+0·0002
3·8121	3·8115	+0·0006
3·8165	3·8169	−0·0004
3·8216	3·8223	−0·0007
3·8284	3·8277	+0·0007
3·8306	3·8331	−0·0025
3·8391	3·8385	+0·0006
3·8467	3·8439	+0.0028
3·8493	3·8493	0·0000

From this Table we fail to perceive a trace of the behaviour exhibited by the dip in Table IV.

Apart, therefore, from all theoretical considerations, we have reason to believe that, as a matter of fact, the behaviour exhibited in the diagram holds for the dip, but does not appreciably manifest itself in the case of the horizontal force.

19. The probable error of a single monthly determination of the horizontal force derived from the seventy-two monthly determinations given in Table VI., and after the application of the correction for secular change has been applied, is ±0·0021. There is, however, reason to believe that, as in the case of the dip, the probable error is increased to some extent by periods of disturbance, and the same method may be applied to test this as was applied to the dip observations. Forming, therefore, a series of seventy-two yearly values of the horizontal force, corresponding severally in epoch to the seventy-two monthly values of Table VI., and deducting each from the corresponding observed monthly value, we obtain, as before, a series of seventy-two differences; and we derive from these the following modifications of the last two columns of Table VII.

Table X.

Date.	Observed *minus* Calculated.	
	April to September.	October to March.
July 1, 1863	+·0016	
January 1, 1864	....	−·0014
July 1, 1864	+·0012	
January 1, 1865	....	+·0006
July 1, 1865	−·0019	
January 1, 1866	....	+·0006
July 1, 1866	+·0004	
January 1, 1867	....	−·0009
July 1, 1867	+·0005	
January 1, 1868	....	+·0008
July 1, 1868	−·0008	
January 1, 1869	....	+·0008
Mean	+0·0002	+0·0001

Thus we see, as in the case of the dip, that the irregularity of the numbers in these columns is much diminished, the result being, however, left the same as before. Finally, if we deduce the probable error of a single observation by means of the series of differences so obtained, we find this to be ±0·0018 instead of ±0·0021, which it was before.

III. Total Force.

20. We find in Table VI. that the mean of the April to September values of the horizontal component of the force in the last six years is 3·8346, corresponding in epoch to January 1, 1866; and in Table I. that the mean of the April to September values of the dip in the same six years is 68° 6′·83.

We find also that the mean of the October to March values are for the horizontal force 3·8372, and for the dip 68° 6′·41, corresponding to epoch July 1, 1866.

We may reduce these to a common epoch by applying to the former dip the correction $-0'\cdot96$, this being the proportional secular change (as shown by these six years) necessary to reduce the former epoch to the latter. The former dip will therefore become $68^\circ\ 6'\cdot83 - 0'\cdot96 = 68^\circ\ 5'\cdot87$.

Reducing in the same way the horizontal force, we have

$$3\cdot8346 + 0\cdot00275 = 3\cdot83735.$$

The values thus become as follows:

	Hor. force.	Dip.
From the April to September observations (reduced to epoch July 1, 1866)......	3·83735	68° 5′·87
And from the October to March observations (corresponding to the same epoch)	3·83720	68° 6′·41

The total force derived from the first series will therefore be 10·28717, and that derived from the second series 10·29080, showing thus a difference of 0·00363 in British units as the measure of the greater intensity of the terrestrial magnetic force in the October to March period, than in the April to September period. This is in the same direction, and very nearly of the same amount, as that determined by Sir E. Sabine from the first six years, which exhibited a similar difference of 0·00317 in British units.

Thus we find that the two series agree in showing nearly the same semiannual variation for the total force, while the first period exhibits the greatest semiannual variation of the dip. It ought, however, to be borne in mind that the two series bear a different relation to the disturbance period, the maximum of disturbances occurring about the middle of the first series, and the minimum near the middle of the second.

II. "Spectroscopic Observations of the Nebula of Orion, and of Jupiter, made with the Great Melbourne Telescope." By A. Le Sueur. Communicated by the Rev. T. R. Robinson, D.D. Received January 27, 1870.

In one particular the spectroscopic observations of the nebula of Orion are not void of interest; they show distinctly that considerable nebulosity exists *within* and about the trapezium. The image at the slit is sufficiently large to well separate the stars of the trapezium, so that when two of these are, as it were, threaded on the slit, a clear space lies between them; this in the spectroscope gives the well-known lines with little, if at all, less brilliancy than the general bright nebula.

The small comparison-mirror being removed, the available slit is ·4 inch high, equivalent in the case of the Cassegrain image to about 43″ arc; with an image condensed about three times (which is the usual arrangement and still allows sufficient separation), the slit may, therefore, be made to considerably overlap the trapezium contour, and thereby, at the same time as the trapezium, light from the brightest part of the nebula is under inspection; it is curious to see that the spectral lines run with almost continuous brightness throughout the height of the slit.

Inaccuracy of focus of the image on the slit might perhaps somewhat mislead, but this has not been allowed to come into play; for the focus is readily adjusted with considerable delicacy, by examination of the breadth of star spectrum, which is reduced to a minimum.

In Sir John Herschel's Cape drawing, a slight nebulosity is seen within and immediately about the trapezium; and in the description is found the following extract from note-book :—"In the interior of the trapezium there exists positively no nebulosity, at least none comparable in intensity to that immediately without it."

There being (as far as I can now see or remember) no other special reference to this matter in the description, it is not quite clear whether or not the nebulosity in the drawing rests on this evidence or on that of other nights when it may have been more conspicuous.

In Lord Rosse's drawing the trapezium is enclosed in, and itself encloses, a space totally free from nebulosity. My own telescopic observations here (on not good nights unfortunately) indicate a positive though comparatively faint nebulosity within and about the trapezium, somewhat as in the Cape drawing; the spectroscope, however, shows with much force that this nebulosity not only exists, but is comparable in brightness to that surrounding the trapezium at some distance,—the brightest part of the nebula in fact; and therefore that, in ordinary observation, the faintness or apparent complete absence of nebula is mainly due to the disturbing brightness of the four stars, and not to any intrinsic extreme faintness or absolute vacuity.

Jupiter has been examined with results, if not, as far as may be judged at present, important, at least interesting. Here, again, the large size of image is brought into prominent play; with the original Cassegrain image the light is barely sufficient, but with the image condensed (at pleasure within certain limits) fair work becomes possible, the spectrum being considerably bright.

The lines G, F, *b*, C, D, are seen without the slightest difficulty, C (being near to visible limit) not so readily, but unmistakably, and many other lines with attention. A marked feature is a dark nebulous band between C, D; from measures this turns out to be one of the bands examined by Mr. Huggins, 882 of his scale* (C_6 of Brewster ?).

The observations were made generally with Jupiter not far from the meridian. On one night only of those employed was the atmosphere at all free from perceptible haze; as far, however, as memory could be trusted, there did not appear to be any perceptible difference in the intensity of the line on the different nights. This line is always so conspicuous that, were it not for Mr. Huggins's more critical observation, I would be inclined to think that in Jupiter it is much stronger than in an equally bright daylight spectrum, under conditions even more favourable than those afforded by the

* [Wrong identification: see next paper.]

altitude of the planet and the state of the atmosphere at the times of observation; considering, moreover, that in Mr. Huggins's observation (as he himself remarks) the relative positions of the sun and Jupiter were such as considerably to exaggerate the effect of the earth's atmosphere on the sky spectrum, it is difficult (in the absence of a more crucial observation pointing in a contrary direction) to escape the impression that this line is in no small degree due to Jupiter's own atmosphere. The band specially examined by Mr. Huggins I have not yet succeeded in seeing with any degree of certainty; but the opportunities have been so few, that the optical conditions for its most favourable development have not been fairly tried.

On one night the eye-aspect of Jupiter was as follows:—N to P (Plate I. fig. 1) of yellowish colour, with occasional appearance in good defining moments of hair-line structure; P to Q almost white, slight tinge of blue; Q to R yellowish, but much darker than N P; R to S also yellow, slightly brighter than N P, and with no suspicion of fine lines.

P, Q, R brown, much darker than general surface, the two latter with a red or yellow tinge, the former with a greenish one. [These are merely the impressions without attempt to eliminate effect of contrast.]

The absolute positions and definite shape of these bands, as given in the diagram, have no special pretensions to minute accuracy; considerable care was, however, employed, and in any case the sketch in its broad features is sufficiently near to the truth for the special purpose in view. In the spectrum, G, F, E, D, C_6, C are laid down from measures on Jupiter. I have called the band between C, D, C_6 for reference purposes, subject to rectification, although there can, I think, be little doubt of the coincidence.

A point specially aimed at in these observations was to note any peculiarity in the appearance of spectral lines of known atmospheric origin according to the part of the surface viewed.

With the slit perpendicular to Jupiter's equator and the advantage of a large image, an admirable opportunity is afforded of noting the behaviour of the lines as they cross the different parts of the surface, a spectroscopic picture of the planet, as it were, being presented beautifully to the eye.

The nebulous line C_6 was specially and narrowly watched, but without any satisfactory evidence being elicited; as this line crosses the bright band P Q it is perhaps slightly less nebulous; on the assumption that C_6 is in great part due to Jupiter's atmosphere, this peculiarity, by no means marked, is yet in the direction to be accounted for by the usual suppositions concerning the natureof Jupiter's visible disk. On this assumption, however, one would expect more decisive evidence of change in the line according as it is due to the cloud-band or to the surface; there is evidence certainly, but so faint that, due regard being had to the possible disturbing effect of the somewhat greater brightness of cloud band, and to the bias which cannot be totally eliminated from the mind, it does not seem entitled to much weight.

This almost, if not altogether, complete sameness of the line might perhaps

(on the foregoing assumption) be accounted for by supposing that the cloud-bands are very near the surface, so near that the light reflected therefrom has to pass through a thickness and density of atmosphere comparable in its effects to that above the more uninterrupted parts of the surface.

Further observations may obviate the necessity for this or any other more feasible explanation, by proving that the band is mainly due to the earth; but, as before shown, the weight of evidence, Mr. Huggins's observation taken into account, is in favour of the assumption that the line, as seen on Jupiter at considerable altitude, is mainly due to the planet itself.

The general appearance of the spectroscopic image is one of nearly uniform brightness, with the marked exception of the brighter band P Q, and the much darker band Q R: in this band the principal absorption takes place at the more refrangible end of the spectrum, where it is very considerable, gradually diminishing, but yet conspicuous, up to E; at moments it may be traced very faintly up to D, with no certainty beyond.

In this band Q, R are not separable; considering the size of the image this can hardly be due entirely to closeness, but would seem to show that (at the more refrangible end at least) the absorption of the yellow and somewhat dark space enclosed between Q, R is little inferior to that of Q, R themselves. P is not conspicuous, but is unmistakably seen in good moments as a narrow streak at the blue end.

The experiment was made of placing the slit parallel to the planet's equator; when in this position and moved slowly over the surface, or arrested at particular points, no peculiarity was distinguishable; so little do the parts differ in brightness, that by this method it could not, from the mere evidence of the spectrum, be told what part was being admitted through the slit; in this method, however, greater delicacy of adjustment is required, for slight want of parallelism of the slit to the bands brings in disturbing effects.

The edges of the disk were examined, but without result.

Observatory, Melbourne, December 5, 1869.

III. "On the Nebulæ of Argo and Orion, and on the Spectrum of Jupiter." By A. Le Sueur. Communicated by Prof. G. G. Stokes, Sec. R.S. Received February 21, 1870.

Among the following observations made with the great Melbourne telescope, the most important are those of η Argo; the spectrum of this star is crossed by *bright lines*.

The mere fact of a bright-line spectrum is not very difficult to ascertain on a good night; for although from faintness of the light the phenomenon is necessarily delicate, yet the bright lines occasionally flash out so sharply that the character of the spectrum cannot be mistaken. The most marked

lines I make out to be, if not coincident with, very near to C, D, *b*, F, and the principal green nitrogen line. There are possibly other lines, but those mentioned are the only ones manageable.

Direct spark comparison has hitherto been found impossible; though plainly marked at moments the lines require concentrated attention, and will not permit the disturbing effect of other light in the field; attempts were made to diminish the brilliancy of the spark spectrum but with no good result, a different method was therefore adopted.

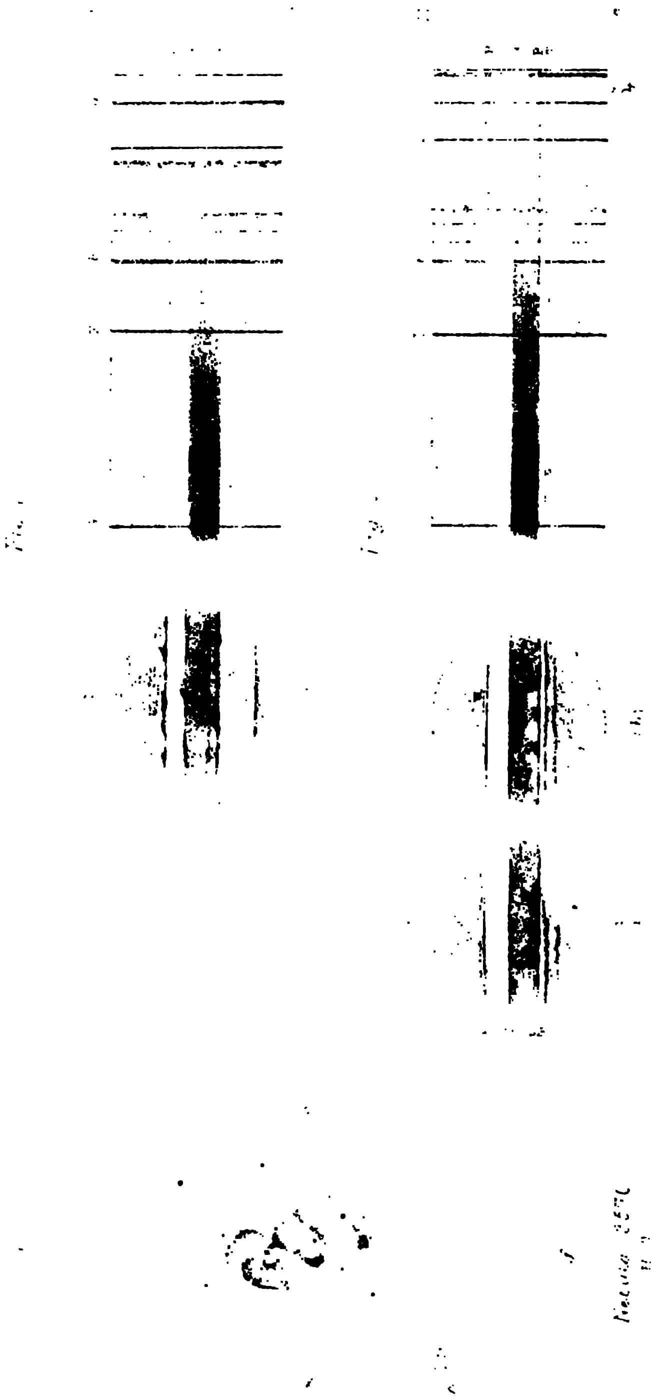
By watching for good moments the pointer was placed on a particular star line, the spark spectrum was then turned on, and the position of the pointer noted.

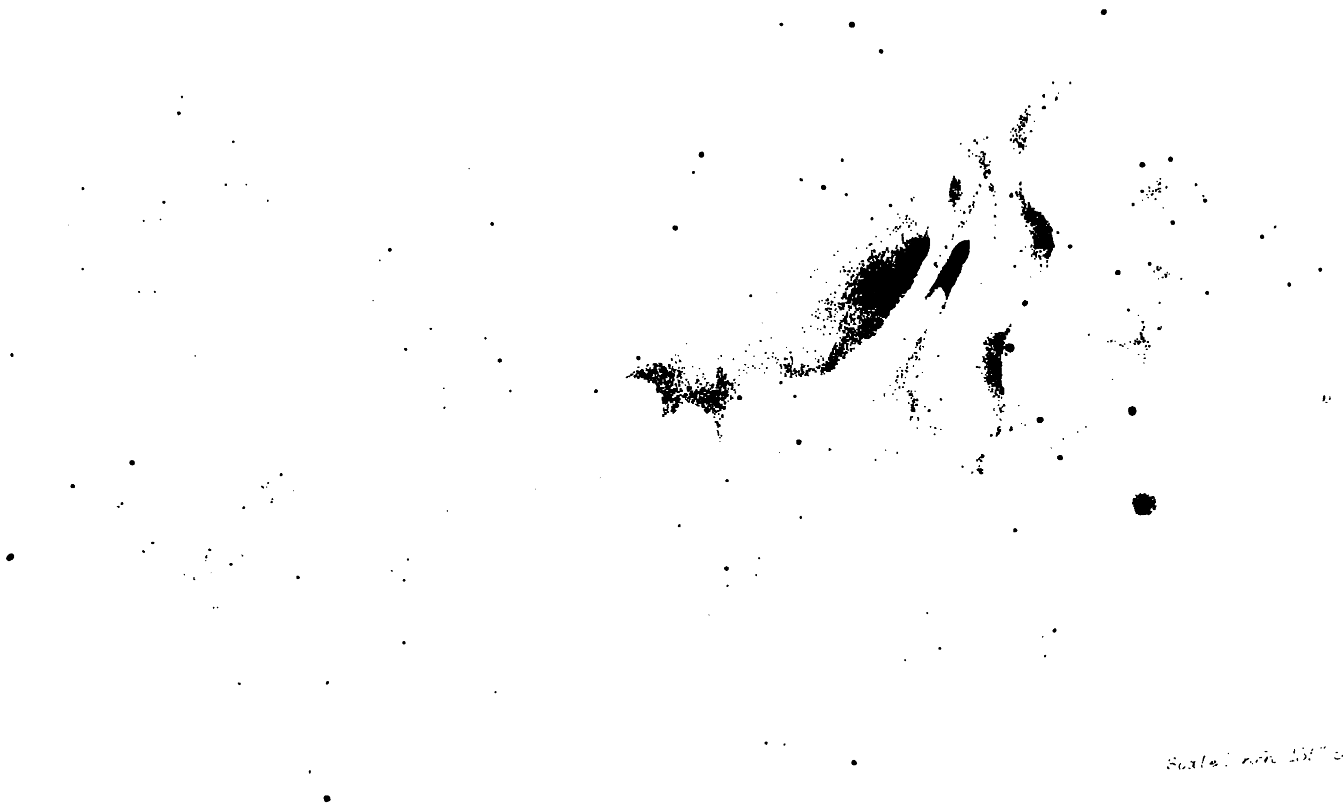
By this means it was seen in repeated trials that star lines within the limits of the dispersion used (about 7°) were coincident with C, F, the principal green nitrogen line, and *b*, or rather (the spark employed was platinum in air) the air-band involved in *b* group. It cannot be determined whether the coincidence is with the magnesium group or the air-band; nothing more definite can be said than that the star line lies within the limits of the group.

The comparison spectrum employed does not show F, but the position of the previously adjusted pointer, with reference to air lines in the neighbourhood, leaves little doubt as to the identity of the blue star line with F, due regard being had to the collateral evidence (when such close limits are reached) that C coincides with a red star line. The yellow (or orange?) line in the star has not yet received sufficient attention; it is, however, very near D.

With the dispersion employed, D and the bright air line on less refrangible side of D are well separated; so that, notwithstanding the delicacy of the star line, I hope, if not to get satisfactory evidence of coincidence with a particular line, at least to eliminate one of the competitors; at present it cannot even be said whether the line may not be slightly more refrangible than D; the limits are, however, very small, placing the bright air group about 1180 of Mr. Huggins's scale completely outside the possible range.

I would remark that the very faint nebulosity (if any) in the immediate neighbourhood of the star η is incompetent to give a trace of spectral lines with even a wide slit; for a considerable space s. and f. of η no lines are at all visible; the nearest nebula bright enough to show a line (the three usual lines are now easily seen on a good night over the brighter parts) is reached in the direction about 45° n. p. from η, and even then the distance from η, as judged by the appearance in the spectroscope with η threaded on the thus directed slit, is little less than one minute. This remark is of some importance in connexion with the ordinary telescopic observations of the nebula, but is mentioned at this point to relieve any impression which might arise that the nitrogen line seen on the star spectrum is merely the chief nebula line crossing it.





Scale: [illegible]

Nebula 4403.

[redacted]

In the present state of the inquiry there is little doubt left as to the presence of hydrogen in the star; the other lines may perhaps be accounted for by nitrogen alone, or by nitrogen, magnesium, and sodium.

On the whole the weight of collateral evidence will probably be considered to be in favour of the latter combination, with the possibility that for sodium may have to be substituted the substance which produces the line in sun-protuberance spectrum.

For although there is no direct evidence as to identity of the line near D, if the coincidence were with the orange nitrogen line it would be reasonable to expect a line in the star corresponding to the yellow line 1180±, yet none has been made out in that position; again, the second green line has probably less claims for visibility than the orange or yellow lines, yet in the star spectrum this line is not less well seen than that coinciding with the chief nitrogen line. These considerations, though perhaps not entitled to great weight, at least lead in the direction of the above inference.

Owing to faintness of the general spectrum no dark lines are made out; one in the red is strongly suspected, and occasionally there is an appearance as if of a multitude over the spectrum generally, but they refuse to be seen separately and certainly.

It is fortunate that these observations have been possible in the present magnitude of the star; may not the bright-line character of the spectrum be due to a commencement of increase? The star has not perceptibly changed since I knew it.

I extract the following estimation from the Melbourne observations:—

		Mag.
1863.	Oct. 14	5
1864.	May 6	4·5
„	„ 19	4·5
1865.	„ 22	4·5
„	June 14	4·5
1868.	April 21	4
„	May 27	4
„	„ 26	5
1869.	„ 2	6½
„	„ 11	6½
„	„ 30	6

The estimations are by Mr. White, who has charge of the Transit Circle. Mr. Ellery estimated it last year as a 6½+, and now thinks it is somewhat brighter.

At an earlier stage of the observations with the Melbourne reflector, I was on the whole inclined to think that the difference between the view of the nebula about η Argo as seen with the 4-feet reflector, and that seen by Sir J. Herschel with his 18-inch, though strongly marked in the neighbourhood of η, was yet, due regard being had to aperture and other disturbing causes, capable of being accounted for without going to the length of assuming such enormous changes as would result if the sketches represented the true facts in both cases. It was thought that the presence of the star η might have a large disturbing effect, increased by aperture, and that therefore an erroneous impression might be formed of the confi-

guration and character of the nebula in its proximate neighbourhood. (The trapezium of Orion, as will be seen from observations to be presently recorded, is a case in point.)

The spectroscope has, however, decided that η in no way influences the configuration as now seen; the star is not only apparently but really on a background, if not completely dark, at least free from nebulosity at all comparable to the brighter parts; moreover the nebulosity at s. end of lemniscate (the shape there is occasionally made out, showing that nebula does exist) is of a similar faint character.

With this evidence that the eye-view with the 4-feet approaches the actual facts, and a due consideration of those facts, it seems difficult to imagine any conditions of aperture, definition, or other disturbing causes which could produce a view at all approaching to that seen by Sir John Herschel.

We have therefore evidence of much weight that enormous changes have taken place in this wonderful region. Is not the presence of nitrogen and hydrogen in the star η a significant fact in connexion with these changes, which appear to be nothing less than a destruction of nebula specially in its neighbourhood?

Orion has been examined with a new and interesting result; the spectroscope proves that in and about the trapezium nebula exists comparable with the bright surrounding nebula.

This observation is rendered easy by the large separation of the stars consequent on great focal length of telescope; indeed the whole separation of the original image is not required, the observation being made more crucial by a condension of between two and three times; with this arrangement the separation is still sufficient, and the advantage is gained of viewing at the same time the bright surrounding nebula.

The stars, sharply focused to give a linear spectrum, being threaded on the slit singly or in pairs, or cautiously removed out of the field, it is seen that the bright lines cross the trapezium with little if at all diminished brilliancy.

The ordinary telescopic view is therefore an erroneous one, produced by the disturbing effect of the bright group.

Jupiter has been examined (generally on moonlight nights); with this object the orginal Cassegrain image is too faint for good work, but by interposition of a suitable lens the image is condensed at pleasure within certain limits; with the light thus increased the Fraunhofer lines G, F, *b*, E, D are always easily seen, C also easily on a clear night; the lines to which special attention has been directed are the telluric lines 914 and 838 (for convenience of reference I use throughout the numbers in Mr. Huggins's

Jupiter and sky diagram). These are the only lines seen with certainty between C and D.

The identity of 914 and 838 rests partly on measures and partly on spark comparison, where, for the identification of 914, it is seen that this line is near to the air band 807 of Mr. Huggins's chemical scale.

The line 914 is so easily seen, that having in mind Mr. Huggins's statement concerning the difficulty of discerning it at all, originally very imperfect measures on a bad night and with the apparatus imperfectly adjusted misleading in the same direction, this line was at first mistaken for 882, from which, however, it is separated far beyond the limit of error in a proper state of adjustment of apparatus.

882 is not seen at all with Jupiter at considerable altitude. On the night of December 29th, however, between the hours of 12.30 and 1, Jupiter being low, 882 was seen almost as conspicuous as 914, which, I may remark, did not seem to have perceptibly increased in darkness by the additional absorption of the earth's atmosphere.

On the night of December 14th (both objects being near the meridian) the spectroscope was turned on Jupiter and the moon alternately several times. On Jupiter 914 and 838 were easily visible, the former (as usual) the more conspicuous; on the moon no line could be certainly made out between C and D. Mr. Ellery was present at the time and gave the same verdict.

So far these observations are merely confirmatory of those made by Mr. Huggins. There is one point, however, not unworthy of consideration, arising from a comparison of the observations in connexion with the conditions under which they are made.

It is probable that Mr. Huggins, with his earlier apparatus, was under more favourable conditions as regards light than, if not the best at my command, at least those under which 914 is now plainly seen. When condensed as much as arrangements allow (about four times), I probably get a somewhat brighter image at the slit than that produced by Mr. Huggins's telescope; but with little or no condensation, and a dispersion of near 7° (B to H=6° 50′), the line in question is still conspicuous. Yet Mr. Huggins speaks of this line as barely distinguishable, or not at all visible with his earlier apparatus. Width of slit, of course, plays a prominent part; but I cannot be wrong in assuming that, for prospecting purposes, Mr. Huggins tried various widths. Moreover when the slit is gradually cut down, 914 is visible as long as the chief Fraunhofer lines, and is still readily seen when the light is insufficient to show a trace of C or 838 near it.

These considerations, if not entitled to much weight, at least point to a possible variability of the line in question. If this prove to be the case, it will be interesting to note its degree of visibility in connexion with the character of the surface at the different times of observation.

I cannot find whether 914 or 838 is involved in the lines proved by M.

Jansen as special to aqueous vapour. An answer one way or the other would be equally interesting; for Mr. Huggins's observations and my own later ones (which are indeed merely corroborative) go far to prove that, whatever the cause of the lines, that which produces 914 and 838 has on Jupiter more efficacy than that which produces 882, while the reverse appears to be the case on the earth.

Jupiter was taken in hand specially to note any peculiarity in the spectrum of different parts of the surface, as regards general or specific absorption. The best observations were made on the night of December 11th, when the phenomena were as given in the diagram (Plate I. fig. 2), to which the second figure of Jupiter is added merely for any additional interest to be derived from two views on the same night (*a* at 9.30, *b* at 11.30$\pm$).

The space N P is slightly yellowish, and appears at good defining moments to be crossed by a multitude of fine hair lines (this has been seen more than once); P Q is white, and considerably brighter than the general surface; Q R dusky yellow, much darker than N P; R T white; T S similar to P Q, but more approaching to white.

P, Q, R, T dark brown with occasional suspicion of green tinge.

The spectrum, as given in the diagram, is an inversion (to suit telescopic image of planet) of what is seen in the spectroscope with the slit perpendicular to Jupiter's equator.

The absorption of Q, R is most marked beyond F, fading gradually away to about E; beyond this Q, R are seen separately with an apparently undiminished spectrum between them; P Q is much brighter than the general spectrum, and is normal throughout; T R occasionally flashes out brightly; P stretches equally across the spectrum; T is most marked at the less refrangible end (the reverse of this was the case for one of the belts on a former occasion).

A special point aimed at in these observations was to note any peculiarity in the lines 914, 838 as they cross the various parts of the surface in this position of the slit, but no satisfactory evidence could be elicited. As before mentioned, by the interposition of a suitable lens the image, still focused on the slit, may be condensed at pleasure within certain limits; a point is therefore chosen at which the compromise between brilliancy of spectrum and size of image is deemed most suitable for the object in view. The light is quite adequate for the purpose when the bands T Q, Q R are still of considerable width; any difference, if not very slight, in the line 914 as it crossed the different bands ought therefore to have been detected. This was not the case. The experiment was tried of placing the slit parallel to the bands, but with no new result.

Melbourne Observatory, January 3, 1870.

Presents received February 3, 1870.

Transactions.

London:—Society of Antiquaries. Archæologia. Vol. XLII. Part 1. 4to. *London* 1869. Proceedings. Vol. IV. Nos. 3–6. 8vo. *London* 1868–69. The Society.

Lyons:—Académie Impériale des Sciences, Belles Lettres et Arts. Mémoires. Classe des Sciences. Tome I.–IX. 8vo. *Lyon* 1851–59. Classe des Lettres. Tome I.–VI. 8vo. *Lyon* 1851–58. Annales des Sciences Physiques et Naturelles, publiées par la Société Impériale d'Agriculture. 3e série. Tome I.–XI. 8vo. *Lyon* 1857–67. The Academy.

Shrewsbury:—The Caradoc Field Club, Shropshire. 12mo. *Shrewsbury* 1869. The Rev. E. Donald Carr.

Oxford:—Radcliffe Observatory. Second Radcliffe Catalogue, containing 2386 Stars, deduced from Observations extending from 1854 to 1861, and reduced to the epoch 1860 under the superintendence of the Rev. R. Main. 8vo. *Oxford* 1870. The Radcliffe Trustees.

Brown (R.) On the Geographical Distribution of the *Coniferæ* and *Gnetaceæ*. 8vo. *Edinburgh* 1869. The Author.

Hansen (P. A., For. Memb. R.S.) Entwickelung eines neuen veränderten Verfahrens zur Ausgleichung eines Dreiecksnetzes. 8vo. *Leipzig* 1869. Supplement zu der geodätische Untersuchungen. 8vo. *Leipzig*. The Author.

Jenzsch (G.) Ueber eine mikroskopische Flora und Fauna krystallinischer Massengesteine (Eruptivgesteine). 8vo. *Leipzig* 1868. The Author.

Jones (Dr. Bence, F.R.S.) The Life and Letters of Faraday. 2 vols. 8vo. *London* 1870. The Author.

Kölliker (A., For. Mem. R. S.) Anatomisch-systematische Beschreibung der Alcyonarien. Erste Abtheilung, erste Hälfte. 4to. *Frankfurt a. M.* 1870. The Author.

Lowe (E. J., F.R.S.) Natural Phenomena and Chronology of the Seasons. Part 1. 8vo. *London* 1870. The Author.

Marignac (C.) De l'Influence de l'Eau sur les doubles décompositions salines. 8vo. *Genève* 1869. The Author.

Playfair (L., F.R.S.) On Primary and Technical Education. 8vo. *Edinburgh* 1870. The Author.

Schmidt (J. C.) Elemente zur Begründung einer Mathematisch-physikalischen Organismenlehre. 8vo. *München* 1869.

Annual Report on Meteorological Observations registered in the Punjab, by A. Neil, 1867. fol. *Lodiana* 1868. The Indian Government.

Report of the Meteorological Reporter to the Government of Bengal for the year 1868–69. fol. *Calcutta* 1869. The Indian Government.

Statistics of the Colony of Victoria for the year 1868. Parts 6, 7, 8. fol. *Melbourne.* The Registrar-General of Victoria.

February 10, 1870.

Transactions.

Coimbra:—Universidade. Resumos Annuaes das Observações Meteorologicas feitas no Observatorio Meteorologico e Magnetico 1864–65, 1865–66. 4to. *Coimbra* 1870. Observações Meteorologicas. Junho–Outobro 1866. 4to. The University.

Florence:—Academia del Cimento. Tentamina Experimentorum Naturalium quibus commentarios addidit P. van Musschenbroek. 4to. *Lugduni Bat.* 1731. Dr. Sharpey, Sec. R.S.

London:—Pathological Society. Transactions. Vol. XX. 8vo. *London* 1869. The Society.

Palermo:—R. Istituto Tecnico. Giornale di Scienze Naturali ed Economiche, pubblicato per cura del Consiglio di Perfezionamento. Anno 1869. Vol. V. Fasc. 3 & 4. 4to. *Palermo* 1869. The Institute.

Paris:—Société de Géographie. Bulletin. Mai–Décembre 1869. 8vo. *Paris.* The Society.

Vienna:—Eilfter Jahres-Bericht der öffentlichen Ober-Realschule in der innern Stadt. 8vo. *Wien* 1869. The School.

Hill (C. J. D.) Mathescos Fundamenta nova Analytica. Parts 2 & 3. 4to. *Lundæ* 1868–69. Sur une forme générale de développement et sur les Intégrales définies. 4to. *Lund.* The Author.

Lamy (A.) Sur un Nouveau Pyromètre. 4to. *Paris* 1869. The Author.

Larrey (Baron) Discours prononcé a l'Inauguration de la Statue de Guillaume Dupuytren à Pierre-Buffière le 17 Oct. 1869. 4to. *Paris* 1869. The Author.

Mensbrugghe (G. van der) Sur la Tension superficielle des Liquides considerée au point de vue de certains Mouvements observés à leur Surface. Premier Mémoire. 4to. *Bruxelles* 1869. The Author.

Neil (A.) Annual Report on Meteorological Observations registered in the Punjab, 1868. fol. *Lodiana* 1869. The Government of India.

Peretti (Paolo) Sull' Albuminato di Ferro ed alcuni saggi chimici sull' Albume di Uovo. 8vo. *Roma* 1869. The Author.

Pictet (F. J.) Rapport sur l'état de la question relative aux limites de la période jurassique et de la période crétacée. 8vo. *Genève* 1869. The Author.

Plateau (F.) Réflexions et Expériences sur le Vol des Coléoptères. 8vo. *Genève* 1869. The Author.

Walker (Col. J. T., F.R.S.) Extracts from General Report of the Operations of the Great Trigonometrical Survey during 1868–69. fol. *Dehra Doon* 1869. The Author.

Fifth Report of the Board of Visitors to the Observatory, with Annual Reports of the Government Astronomer (Victoria). fol. *Melbourne* 1869. The Observatory.

Report of the Salmon Commissioners, Tasmania. fol. 1869. The Commissioners.

American (The) Journal of Science and Arts. Vol. XLVIII. Nos. 142–144. 8vo. *New Haven* 1869. The Editors.

February 17, 1870.

Transactions:—

Berlin:—Königliche Statistische Bureau. Monatliche Mittel des Jahrganges 1868 für Druck, Temperatur, Feuchtigkeit und Niederschläge und fünftägige Wärmemittel nebst Abweichungen derselben für die Jahre 1866, 1867, 1868. 4to. *Berlin* 1869. The Bureau.

Giessen:—Ludewigs-Universität. Verzeichniss der Vorlesungen 1868–70. 4to. *Giessen*. Index Librorum, 1867–68. Thesen, von L. Arcularius, P. Biedert, C. Brettel, F. Credner, H. Curschmann, J. T. Dalquen, A. Dillmann, K. Drescher, B. Goldbeck, M. J. Höfner, B. Horn, W. Hüffell, F. R. Klein, P. Knoll, H. Koch, C. Küchler, L. Lange, W. Leissler, L. Loeb, A. Martin, E. May, F. Noellner, A. Rolly, O. Schack, A. von Seckendorff, E. Thurn, G. Vierheller, J. A. Vullers, C. Wallenstein, E. Weiss, H. G. Weiffenbach, H. Weil, T. Welcker, K. Werner, L. Weyland, E. Zimmermann. 8vo & 4to. *Giessen* 1868–69. The University.

London:—Anthropological Society. Memoirs. 1867-8-9. Vol. III. 8vo. *London* 1870. The Anthropological Review. No. 28. 8vo. *London* 1870. The Society.

Munich:—Königl. bayer. Akademie der Wissenschaften, 1869. I. Heft 4; II. Hefte 1 & 2. 8vo. *München* 1869. Catalogus Codicum Latinorum

Transactions (*continued*).

Bibliothecæ Regiæ Monacensis. Tomi I. Pars 1. 8vo. *Monac h* 1868. The Academy.

Sternwarte. Verzeichniss von 6323 telescopischen Sternen zwischen +3° und + 9° Declination. VIII. Supplementband zu den Annalen. 8vo. *München* 1869. Verbesserungen zu den Declinationen des "Verzeichnisses von 9412 Aequatorial-Sternen zwischen +3° und −3° Declination." 8vo. *München.* The Observatory.

Delesse (—) Distribution de la Pluie en France. 8vo. *Paris* 1868. The Author.

Fizeau (H.) Tableau des dilatations par la Chaleur de divers corps simples métalliques ou non métalliques et de quelques composés hydrogénés du Carbone. 4to. *Paris* 1869. The Author.

Leclanché (G.) Notes sur l'emploi des piles électriques en Télégraphie. 8vo. *Paris* 1867. Treatise on the Leclanché Battery. 8vo. *London* 1869. C. V. Walker, F.R.S.

Schellen (H.) Die Spectralanalyse in ihrer Anwendung auf die Stoffe der Erde und die Natur der Himmelskörper. 8vo. *Braunschweig* 1870. W. Spottiswoode, F.R.S.

Zantedeschi (Fr.) Sulla Riduzione della Lignite e della Torba che abbondano in alcune contrade d'Italia in buon Carbone fossile Inglese. 8vo. *Venezia* 1869.

Emploi de l'armature externe du Câble sous-marin pendant que l'armature interne ou conducteur isolé transmet la dépêche télégraphique. 8vo. *Padoue* 1869. The Author.

Astronomische Nachrichten, begrundet von H. C. Schumacher; herausgegeben von C. A. F. Peters. Band LXXII.–IV. 4to. *Altona* 1868–69. The Editor.

Report of the Astronomer to the Marine Committee, Mersey Docks and Harbour Board. 8vo. *Liverpool* 1869. The Committee.

February 24, 1870.

Transactions.

Berlin:—Kön. Akademie der Wissenschaften. Monatsbericht. Sept.–Dec. 1869. 8vo. *Berlin.* The Academy.

London:—Entomological Society. Transactions for the year 1869. Parts 5 & 6. 8vo. *London.* The Society.

Transactions (*continued*).

London :—Geological Society. Quarterly Journal. Vol. XXV. Parts 3 & 4. Vol. XXVI. Part I. List of Members. 8vo. *London* 1869–70. The Society.

Meteorological Society. Proceedings. Vol. IV. Nos. 43 & 44. Vol. V. Nos. 45 & 46. List of Members. 8vo. *London.* 1869–70. The Society.

University of London. The Calendar for the year 1870. The University.

Paris :—Faculté des Sciences. Thèses presentés par A. Descamps, S. Meunier, C. Friedel, A. Gautier, G. Pouchet, M. Girard, C. Caye, Berthaud, J. O. E. Perrier, H. Baubigny, L. Lartet. Nos. 306–316. 4to. *Paris* 1869. The Faculty.

Vienna :—K. K. Geologische Reichsanstalt. Jahrbuch ; Jahrgang 1869 XIX. Band, No. 3. Verhandlungen, No. 10. roy. 8vo. *Wien* 1869. The Union.

Haidinger (W. Ritter von, For. Mem. R. S.) Das Kaiserlich-Königliche Montanistische Museum und die Freunde der Naturwissenschaften in Wien in den Jahren 1840 bis 1850. 8vo. *Wien* 1869. The Author.

Jerwood (J.) On the Application of Weight to test the figure of the Earth. 8vo. 1869. The Author.

Montucci (H.) Résolution Numérique complète des Equations du Cinquième Degré. 8vo. *Paris* 1869. The Author.

Neumayer (G.) Discussion of the Meteorological and Magnetical Observations made at the Flagstaff Observatory, Melbourne, during the years 1858–1863. 4to. *Mannheim* 1867. The Government of Victoria.

Stevenson (T.) On ascertaining the intensity of Storms by the Calculations of Barometric Gradients. roy. 8vo. *Edinb.* 1867. The Author.

Tennant (J. F., F.R.S.) Report on the Total Eclipse of the Sun, August 17–18, 1868, as observed at Guntoor. 4to. *London* 1869. The Author.

Vignoles (Charles B., F.R.S.) Address on his Election as President of the Institution of Civil Engineers, Session 1869–70. roy. 8vo. *London* 1870. The Author.

Vishnubawa Brahmachan. An Essay in Marathi on Beneficent Government, translated by Capt. A. Phelps. 12mo. *Bombay* 1869. The Author.

Report of the Government Astronomer upon the Proceedings of the Observatory in Connexion with total Eclipse of the Sun on Aug. 18, 1868, as observed at Masulipatam, Vunpurthy, Madras, and other stations in Southern India. 8vo. 1868. The Government of Madras.

March 3, 1870.

Transactions.

Edinburgh:—Royal Society. Transactions. Vol. XXV. Part 2. 4to. *Edinburgh* 1869. Proceedings, Session 1868–69. 8vo. *Edinburgh.* The Society.

Geneva:—Institut National Genevois. Mémoires. Tome XII. 4to. *Genève* 1869. Bulletin. Nos. 32–34. 8vo. *Genève* 1869. The Institute.

Göttingen:—Königliche Gesellschaft der Wissenschaften. Abhandlungen. Band XIV. 4to. *Göttingen* 1869. Nachrichten, aus dem Jahre 1869. 12mo. 1869. Astronomische Mittheilungen von der Konigl. Sternwarte zu Göttingen. Theil I. 4to. *Göttingen* 1869. The Society.

Leipzig:—Fürstlich Jablonowski'schen Gesellschaft. Preisschriften XIV., XV. 8vo. *Leipzig* 1869. The Society.

London:—British Museum. Catalogue of Diurnal Lepidoptera described by Fabricius, by A. G. Butler. 8vo. *London* 1869. The Trustees.

National Association for the Promotion of Social Science. Transactions. Bristol Meeting, 1869. 8vo. *London* 1870. The Association.

Royal Asiatic Society. Journal. New series. Vol. IV. Part 2. 8vo. *London* 1870. The Society.

Middlesbrough:—Iron and Steel Institute. Transactions. Session 1869. Vol. I. No. 4. 8vo. *Newcastle-upon-Tyne* 1869. The Institute.

Haidinger (W. v., For. Mem. R. S.) Review of the Royal Society Catalogue of Scientific Papers (Verhandlungen der k. k. geologischen Reichsanstalt). 8vo. *Wien* 1870.

Schiaparelli (G. V.) Alcuni risultati preliminari tratti dalle Osservazioni di Stelle Cadenti pubblicate nelle Effemeridi degli anni 1868, 1869, 1870. 8vo. The Author.

Wechniakof (T.) Introduction aux Recherches sur l'Économie des Travaux Scientifiques et Esthétiques. 8vo. *Paris* 1870. The Author.

Birmingham Free Libraries. Catalogue of the Reference Department, by J. D. Mullins. 8vo. *Birmingham* 1869. The Committee of the Libraries.

March 10, 1870.

WARREN DE LA RUE, Esq., Vice-President, in the Chair.

The following communications were read:—

I. "On some Elementary Principles in Animal Mechanics.—No. III. On the Muscular Forces employed in Parturition." By the Rev. SAMUEL HAUGHTON, M.D., Fellow of Trinity College, Dublin. Received January 31, 1870.

In the first stage of natural labour, the involuntary muscles of the uterus contract upon the fluid contents of this organ, and possess sufficient force to dilate the mouth of the womb, and generally to rupture the membranes. I shall endeavour to show, from the principles of muscular action already laid down, that the uterine muscles are sufficient, and not much more than sufficient, to complete the first stage of labour, and that they do not possess an amount of force adequate to rupture, in any case, the uterine wall itself.

In the second stage of labour, the irritation of the fœtal head upon the wall of the vagina provokes the reflex action of the voluntary abdominal muscles, which aid powerfully the uterine muscles to complete the second stage by expelling the fœtus. The amount of available additional force given out by the abdominal muscles admits of calculation, and will be found much greater than the force produced by the involuntary contractions of the womb itself.

The mechanical problem to be solved for both cases is one of much interest, as it is the celebrated problem of the equilibrium of a flexible membrane subjected to the action of given forces. It has been solved by Lagrange (Mécanique Analytique, p. 147) in all its generality. In the most general case of the problem, the following beautiful thereom can be demonstrated:—Let T denote the tensile strain acting in the tangential plane of the membrane, applied to rupture a band of the membrane 1 inch broad; let P denote the pressure resulting from all the forces in action, perpendicular to the surface of the membrane, and acting on a surface of one square inch; and let ρ_1 and ρ_2 denote the two radii of principal curvature of the membrane at the point considered. Then we have the following equation:—

$$P=T\times\left(\frac{1}{\rho_1}+\frac{1}{\rho_2}\right).$$

If the surface, or a portion of it, become spherical, the two principal curvatures become equal, and the equation becomes

$$P=\frac{2T}{\rho}.$$

In the case of the uterus and its membranes, the force P arises from

hydrostatical pressure only, and is therefore easily measured, and the supposition of spherical curvature is approximately admissible.

It is evident from the form of the gravid uterus, that its curvature is greatest near its mouth; and the equation shows that for a given hydrostatical pressure the tensile strain is proportional to the radius of curvature; hence this strain will be greatest at the fundus of the uterus, in which part, accordingly, we find the muscular coat thicker than elsewhere. If we assume the shape of the uterus to be that of a prolate ellipsoid, whose longer diameter is 12 inches, and shorter diameter 8 inches, its mean curvature will be that of a sphere whose diameter is 9·158 inches.

The volume of the gravid uterus is found from the expression

$$\text{Volume} = \frac{4}{3}\pi a b^2;$$

in which a and b are the semiaxes, and π is the ratio of the circumference of a circle to its diameter; substituting for a and b their numerical values, we find the contents of the uterus to be 402·13 cubic inches.

The surface of the gravid uterus may be found from the equation

$$\text{Surface} = \frac{2\pi ab}{e}\left(\sin^{-1}e + e\sqrt{1-e^2}\right);$$

in which e is the excentricity of the generating ellipse. If the numerical values be substituted in this expression, it will be found that the surface of the uterus is 270·66 square inches *.

Some highly interesting conclusions may be drawn from the preceding calculations, combined with the weight of the total muscular tissue of the uterus. Heschl estimates the weight of the uterine muscles at from 1 lb. to 1·5 lb., Montgomery found the muscles of the gravid uterus to weigh 1·5 lb., and Levret estimates them at 51 cubic inches, which, with a specific gravity of 1·052, I find to be equivalent to 1·93 lb. Taking the mean of these estimates we have:—

Weight of Muscular Fibres of Gravid Womb.

	lbs.
Heschl	1·25
Montgomery	1·50
Levret	1·93
Mean	1·56

If we now suppose this quantity of muscle to be spread over the entire surface of the uterus, we find

$$\text{Mean thickness of muscular wall of uterus} \;\ldots\; \left\{\frac{1{\cdot}56 \times 7000 \times 1000}{252{\cdot}5 \times 270{\cdot}66 \times 1052}\right\} = 0{\cdot}1519 \text{ inch.}$$

* Levret estimates the contents of the gravid uterus at 408 cubic inches, and its surface at 339 square inches.

Poppel estimates the contents at 300 cubic inches, and the surface at 210 square inches.

If we suppose a ribbon, one inch in width, to be formed from the wall of the uterus, its thickness will be 0·1519 inch; and as each square inch of cross section of muscular fibre is capable of lifting 102·55 lbs., we find for the greatest tensile force producible by the contraction of the uterine muscles:—

$$\left.\begin{array}{r}\text{Tensile strain of uterine}\\ \text{wall per inch } \ldots\ldots\end{array}\right\} 102{\cdot}55 \times 0{\cdot}1519 = 15{\cdot}577 \text{ lbs.}$$

Substituting this value of T in the equation

$$P = \frac{2T}{\rho},$$

and for ρ its mean value 9·158 inches, we obtain the maximum hydrostatical pressure inside the gravid uterus that can be produced by the contraction of its muscular fibres:—

$$\left.\begin{array}{r}\text{Maximum hydrostatical pressure}\\ \text{produced by uterine contraction}\end{array}\right\} \frac{2 \times 15{\cdot}577}{9{\cdot}158} = 3{\cdot}402 \text{ lbs.}$$

This pressure, applied to a circular surface of $4\frac{1}{2}$ inches in diameter, is equal to 54·106 lbs. One hundred experiments were made by Duncan and Tait upon the hydrostatical pressure necessary to rupture the membranes which contain the liquor amnii, which are recorded in Dr. Duncan's book* (pp. 306–311). The greatest pressure observed was 3·10 lbs., and the least was 0·26 lb.; and I find that the mean rupturing pressure of all their experiments was 1·2048 lb.

Combining this experimental result with the calculation already given, of the amount of pressure producible by the muscular tissue of the womb, we may conclude that the uterine muscles are capable of rupturing the membranes in every case, and possess, in general, nearly three times the amount of force requisite for this purpose.

In the second stage of labour, the voluntary action of the abdominal muscles is called into play to aid the expulsive efforts of the uterine muscles. I have attempted to calculate the force available from the contraction of these muscles as follows.

The abdominal muscles are four in number, viz. *rectus abdominis, obliquus externus, obliquus internus* and *transversalis*. The last three muscles form curved sheets, acting upon the corresponding muscles of the opposite side by means of tendinous *aponeuroses* which meet in the linea alba, and form the sheath of the vertical *rectus abdominis* muscle. From the arrangement of all four, it is plain that the tensile force of muscular contraction in the curved wall of the belly, from the xiphoid cartilage to the symphysis pubis, is to be measured by the sum of the united forces of all the muscular sheets. If we knew the force of each muscle, and the principal curvatures of the belly in the middle line, we could calculate, by Lagrange's theorem, the hydrostatical pressure inside the abdominal cavity and available to expel fæces, urine, or a fœtus.

* Researches in Obstetrics. Edinburgh, 1868.

In order to ascertain the force of the muscles, I measured carefully their average thicknesses in three subjects, of whom one was a young woman who had borne children, and the others were men of ordinary size and appearance. The results obtained were the following:—

Thicknesses of Abdominal Muscles.

	No. 1. Male.	No. 2. Female.	No. 3. Male.
	in.	in.	in.
Rectus abdominis	0·275	0·29	0·34
Obliquus externus	0·200	0·25	0·19
Obliquus internus	0·235	0·17	0·24
Transversalis	0·127	0·15	0·14
Total	0·837	0·86	0·91

The average total thickness of the muscular walls is 0·869 inch, which is nearly identical with the measurement obtained from the female subject. It has been ascertained by careful observations, that we must add 50 per cent. to the weights of muscles in the dead subject in order to bring them to the living weights; this correction gives us 1·3035 inch for the mean thickness of the muscles causing tension in the central line of the belly, where the forces of all the muscles come into play together. Multiplying this thickness by 102·55 lbs., or coefficient of muscular contraction, we find

$$T = 1{\cdot}3035 \times 102{\cdot}55 = 133{\cdot}67 \text{ lbs.}$$

This is the tensile strain producible by the contraction of the abdominal muscles along the curved central line of the belly.

It remains now to ascertain the principal curvatures of the abdominal surface, and to use the equation

$$P = T\left(\frac{1}{\rho_1} + \frac{1}{\rho_2}\right)$$

so as to determine P, the hydrostatical pressure per square inch inside the cavity of the belly, and available, either in whole or in part, for the expulsion of the fœtus during the second stage of labour.

In order to ascertain the curvature of the belly, I made experiments on three young men placed lying on their backs upon the floor, and made them depress and raise the abdominal wall as much as possible. The result was as follows:—Taking a straight line from the upper part of the symphysis pubis to the xiphoid cartilage as the fixed line of comparison, it was found possible to depress the navel one inch below this fixed line and to raise it two inches above it. When the belly was distended to the utmost by the action of the abdominal muscles, I measured the longitudinal and transverse curvatures by measuring the sagittas corresponding to a given length of tangent, with the following results:—

Number.	Diameter of longitudinal curvature.	Diameter of transverse curvature.
	in.	in.
J. G. H.	22·93	12·30
H. O.	22·73	12·80
S. H.	22·52	12·80
Mean	22·727	12·633

The curvature of the distended belly at the navel is found to be, from the foregoing measurements,

$$\frac{1}{\rho_1}+\frac{1}{\rho_2}=\frac{1}{11\cdot3635}+\frac{1}{6\cdot3166}=\frac{1}{4\cdot0596}.$$

Multiplying this curvature into the tension of the abdominal muscles at the navel already found, viz. 133·67 lbs. per inch, we obtain, finally,

$$P=\frac{133\cdot67}{4\cdot0596}=32\cdot926 \text{ lbs. per square inch.}$$

This amount of expulsive force per square inch is available, although not usually employed, to assist the uterus in completing the second stage of labour. If we suppose it applied to the surface of a circle $4\frac{1}{2}$ inches in diameter (the usual width of the pelvic canal), we find that it is equivalent to 523·65 lbs. pressure.

Adding together the combined forces of the voluntary and involuntary muscles, we find—

Involuntary muscles	= 54·106	lbs.
Voluntary muscles	= 523·65	„
Total	577·75	„

Thus we see that, on an emergency, somewhat more than a quarter of a ton pressure can be brought to bear upon a refractory child that refuses to come into the world in the usual manner*.

In order to determine by actual experiment the expulsive force of the abdominal muscles, I placed two men, of 48 and 21 years of age respectively, lying on a table upon their backs, and put a disk measuring 1·87 inch diameter just over the navel; weights were placed upon this disk and gradually increased until the extreme limit of weight that could be lifted with safety was reached; this limit was found to be in both cases 113 lbs. As the circle whose diameter is 1·87 inch has an area of 2·937 square

* The preceding result will no doubt remind the curious and well-informed reader of the statement made by Mr. Shandy, on the authority of Lithopædus Senonensis, 'De partu difficili,' that the force of the woman's efforts in strong labour pains is equal upon an average to the weight of 470 lbs. avoirdupois acting perpendicularly upon the vertex of the head of the child.

inches, the pressure perpendicular to the abdominal wall produced by the action of the abdominal muscles was

$$P=\frac{113}{2\cdot 937}=38\cdot 47 \text{ lbs. per square inch,}$$

a result which differs little from that already found by calculation from the actual measurements of the muscles and curvatures.

II. "Tables of the Numerical Values of the Sine-integral, Cosine-integral, and Exponential Integral." By J. W. L. GLAISHER, Trinity College, Cambridge. Communicated by Professor CAYLEY, LL.D. Received February 10, 1870.

(Abstract.)

The integrals

$$\int_0^x \frac{\sin u}{u}\,du, \quad \int_\infty^x \frac{\cos u}{u}\,du, \quad \int_{-x}^\infty \frac{e^{-u}}{u}\,du,$$

called the sine-integral, cosine-integral, and exponential integral, were used by Schlömilch to express the values of several more complicated integrals, and denoted by him thus,—Si x, Ci x, Ei x; the last function, however, is for all real values of x only another form of the logarithm-integral, the relation being

$$\text{Ei}\,x=\text{li}\,e^x.$$

These functions have since been shown to be the key to a very large class of definite integrals, and several hundreds have been evaluated in terms of them by Schlömilch, De Haan, &c., so that for some time they have been considered primary functions of the integral calculus, and forms reduced to dependence on them have been regarded as known.

Considering, therefore, the large number of integrals dependent on them for their evaluation, and their consequent importance as a means of extending the integral calculus, it seemed very desirable that they should be systematically tabulated, the only values which have previously been obtained being those of Si x, Ci x, Ei x, Ei $(-x)$ for the values $x=1, 2, \ldots 10$ calculated by Bretschneider, and printed in the third volume of Grunert's 'Archiv der Mathematik und Physik,' and a Table of the logarithm-integral published by Soldner at Munich in 1806.

The present Tables contain the values of Si x, Ci x, Ei x, Ei $(-x)$ for values of x from 0 to 1 at intervals of $\cdot 01$ to nineteen places of decimals, for values of x from 1 to 5 at intervals of $\cdot 1$, and from 5 to 15 at intervals of unity, to ten places, and for $x=20$ to twelve places. Also values of Si x and Ci x only for values of x from 20 to 100 at intervals of 5, to 200 at intervals of 10, to 1000 at intervals of 100, and for several higher values to seven places; besides Tables of the maxima and minima values of these functions, corresponding in the case of the sine-integral to multiples of π, and in the case of the cosine-integral to odd multiples of $\frac{\pi}{2}$, also to seven places.

III. "Researches on Solar Physics.—No. II. The Positions and Areas of the Spots observed at Kew during the years 1864–66, also the Spotted Area of the Sun's visible disk from the commencement of 1832 up to May 1868." By WARREN DE LA RUE, Esq., Ph.D., F.R.S., F.R.A.S., BALFOUR STEWART, Esq., LL.D., F.R.S., F.R.A.S., Superintendent of the Kew Observatory, and BENJAMIN LOEWY, Esq., F.R.A.S. Received February 15, 1870.

(Abstract.)

The paper commences with a continuation for the years 1864–66 of Tables II. and III. of a previous paper by the same authors; it then proceeds to a discussion of the value of the pictures of the sun made by Hofrath Schwabe, which had been placed at the disposal of the authors, and the result is that these pictures, when compared with simultaneous pictures taken by Carrington and by the Kew heliograph, are found to be of great trustworthiness. From 1832 to 1854 the pictures discussed are those of Schwabe, who was the only observer between these dates; then follows the series taken by Carrington, and lastly the Kew series, which began in 1862.

A list is given of the values of the sun's spotted area for every fortnight, from the beginning of 1832 up to May 1868, and also a list of three-monthly values of the same, each three-monthly value being the mean of the three fortnightly values which precede and of the three which follow it. These three-monthly values are also given for every fortnight.

A plate is appended to the paper, in which a curve is laid down representing the progress of solar disturbance as derived from the three-monthly values; and another curve is derived from this by a simple process of equalization, representing the progress of the ten-yearly period. The values of the latter curve, corresponding to every fortnight, are also tabulated. From this Table are derived the following epochs of maxima and minima of the longer period:—

Minimum	Nov. 28, 1833.	
Maximum	....	Dec. 21, 1836.
Minimum	Sept. 21, 1843.	
Maximum	....	Nov. 14, 1847.
Minimum	April 21, 1856.	
Maximum	....	Sept. 7, 1859.
Minimum	Feb. 14, 1867.	

This exhibits a variability in the length of the whole period.

Thus we have between	1st and 2nd minimum	9·81 years.
	2nd and 3rd do.	12·58 „
	3rd and 4th do.	10·81 „
	Mean of all the periods	11·07 years.

Another fact previously noted by Sir J. Herschel is brought to light,

namely, that the time between a minimum and the next maximum is less than that from the maximum to the next minimum.

Thus the times from the minimum to the maximum are for the three periods 3·06, 4·14, and 3·37, while those from the maximum to the minimum are 6·75, 8·44, and 7·44 years.

In all the three periods there are times of secondary maxima after the first maximum; and in order to exhibit this peculiarity, statistics are given of the light-curve of R Sagittæ and of β Lyræ, two variable stars which present peculiarities similar to the sun.

Finally, the results are tested to see whether they exhibit any trace of planetary influence; and for this purpose the conjunctions of Jupiter and Venus, of Venus and Mercury, of Jupiter and Mercury, as well as the varying distances of Mercury alone in its elliptical orbit, have been made use of, and the united effect is exhibited in the following Table, the unit of spotted area being one-millionth of the sun's visible hemisphere:—

	Excess or Deficiency.			
Angular separation.	Jupiter and Venus.	Venus and Mercury.	Mercury alone (Perihelion=0).	Mercury and Jupiter.
0 to 30	+ 881	+1675	− 380	−227
30 to 60	− 60	− 139	−1188	−317
60 to 90	− 452	−1665	−1287	−594
90 to 120	− 579	−2355	−1262	−714
120 to 150	− 705	−2318	−1208	−508
150 to 180	− 759	−1604	−1027	−491
180 to 210	− 893	− 481	− 519	−416
210 to 240	− 752	+ 547	+ 430	−189
240 to 270	− 263	+ 431	+1082	− 25
270 to 300	+ 70	+ 228	+1436	+154
300 to 330	+ 480	+1318	+1282	+164
330 to 0	+1134	+2283	+ 586	− 45

IV. "On the Contact of Conics with Surfaces." By William Spottiswoode, M.A., F.R.S. Received February 16, 1870.

(Abstract.)

It is well known that at every point of a surface two tangents, called principal tangents, may be drawn having three-pointic contact with the surface, *i. e.* having an intimacy exceeding by one degree that generally enjoyed by a straight line and a surface. The object of the present paper is to establish the corresponding theorem respecting tangent conics, viz. that "at every point of a surface ten conics may be drawn having six-pointic contact with the surface;" these may be called Principal Tangent Conics. In this investigation I have adopted a method analogous to that employed in my paper "On the Sextactic Points of a Plane Curve" (Phil.

Trans. vol. clv. p. 653); and as I there, in the case of three variables, introduced a set of three arbitrary constants in order to comprise a group of expressions in a single formula, so here, in the case of four variables, I introduce with the same view two sets of four arbitrary constants. If these constants be represented by $\alpha, \beta, \gamma, \delta, \alpha', \beta', \gamma', \delta'$, I consider the conic of five-pointic contact of a section of the surface made by the plane $\varpi - k\varpi' = 0$, where $\varpi = \alpha x + \beta y + \gamma z + \delta t$, and $\varpi' = \alpha' x + \beta' y + \gamma' z + \delta' t$, and k is indeterminate; and then proceed to determine k, and thereby the azimuth of the plane about the line $\varpi = 0$, $\varpi' = 0$, so that the contact may be six-pointic. The formulæ thence arising turn out to be strictly analogous to those belonging to the case of three variables, except that the arbitrary quantities cannot in general be divided out from the final expression. In fact, it is the presence of these quantities which enables us to determine the position of the plane of section, and the equation whereby this is effected proves to be of the degree 10 in $\varpi : \varpi' = k$, and besides this of the degree $12n - 27$ in the coordinates x, y, z, t (n being the degree of the surface), giving rise to the theorem above stated.

Beyond the question of the principal tangents, it has been shown by Clebsch and Salmon that on every surface U a curve may be drawn, at every point of which one of the principal tangents will have a four-pointic contact. And if n be the degree of U, that of the surface S intersecting U in the curve in question will be $11n - 24$. Further, it has been shown that at a finite number of points the contact will be five-pointic. The number of these points has not yet been completely determined; but Clebsch has shown (Crelle, vol. lviii. p. 93) that it does not exceed $n(11n - 24)(14n - 30)$. Similarly it appears that on every surface a curve may be drawn, at every point of which one of the principal tangent conics has a seven-pointic contact, and that at a finite number of points the contact will become eight-pointic. But into the discussion of these latter problems I do not propose to enter in the present communication.

March 17, 1870.

Capt. RICHARDS, R.N., Vice-President, in the Chair.

The following communications were read:—

I. "On the Law which regulates the Relative Magnitude of the Areas of the four Orifices of the Heart." By HERBERT DAVIES, M.D., F.R.C.P., Senior Physician to the London Hospital, and formerly Fellow of Queens' College, Cambridge. Communicated by W. H. FLOWER, Hunterian Professor of Comparative Anatomy. Received January 27, 1870.

I propose in this communication to inquire whether any law can be discovered which determines the relative magnitude of the areas of the

tricuspid, pulmonic, mitral, and aortic orifices—the four principal openings in the heart.

Although to ordinary observation these orifices appear to exhibit no mutual relationship of size, there can be no doubt that an instrument so accurate in the adaptation of its valvular apparatus, and so exact in the working of its different parts, must reveal on close examination the existence of laws which not only determine the force required to be impressed upon the blood traversing its chambers, but also the relative sizes of these apertures to one another.

The facts and inferences which I shall adduce on this subject will tend, I believe, to throw some light upon the mechanism of the heart in its healthy state, and will explain also some points of practical interest in the pathology of that organ.

To M. Bizot in France, and Drs. Peacock and Reid in this country, are we mainly indebted for the most careful and trustworthy measurements of the circumferences of the orifices. Their measurements have been recorded to the minuteness of the thousandth part of an inch, and yet it will, I believe, be readily admitted that the results in the form given by these distinguished observers help us but little in obtaining any definite idea of the mutual relationship of the areas of the orifices, and are destitute of any practical value in our study of the mechanism of the heart itself.

Had the observations been pushed further, or, rather, had the figures been worked out into some distinct and definite shape, these observers could not have failed to discover an interesting and important law presiding over the areas of these orifices, and they would thus have been enabled to utilize a multitude of measurements which had been obtained by considerable labour and patient research.

Taking the measurements given by Dr. Peacock in the Croonian Lectures for 1865, we find the mean circumferences of the four orifices, expressed in English inches, to be as follows:—

	Males.	Females.
Tricuspid	4·74	4·562
Pulmonic	3·552	3·493
Mitral	4	3·996
Aortic	3·14	3·019

I will now place these valuable facts into another shape by calculating from these measurements of *circumference* the *areas* of the respective openings.

The circumference of a circular opening being known, its radius is determined from the formula

$$\text{circumference} = 2\pi r,$$

where $\pi = 3{\cdot}1415$; and the radius being thus determined, the *area* of the opening is calculated from the formula

$$\text{area} = \pi r^2.$$

The mean areas of the four orifices thus obtained are found to be as follows:—

	Males.	Females.
Tricuspid............	1·78 sq. in.	1·6 sq. in.
Pulmonic............	1	·97
Mitral..............	1·27	1·27
Aortic..............	·78	·67

Or, for facility of recollection, we may consider the respective mean areas in the male to be:—

Tricuspid...........................	$1\frac{3}{4}$ sq. in.
Pulmonic...........................	1
Mitral.............................	$1\frac{1}{4}$
Aortic.............................	$\frac{3}{4}$

whence it is obvious that the apertures differ very considerably in area from each other, the tricuspid having the largest area, its orifice being more than double the size of the aortic opening.

Irregular, however, as these areas may appear to be in magnitude with respect to each other, we shall find, on pushing our observations further, that there is a distinct and constant law presiding over them, and this law is discovered when we compare the *ratios* of the areas of corresponding orifices. Thus,

$$\frac{\text{Area of tricuspid}}{\text{Area of mitral}}=\frac{1\cdot78}{1\cdot27}=1\cdot4, \text{ nearly};$$

$$\frac{\text{Area of pulmonic}}{\text{Area of aortic}}=\frac{1}{\cdot78}=1\cdot3, \text{ nearly};$$

or, in other words, the area of the tricuspid appears from these calculations to bear nearly the same relation to the area of the mitral which the area of the pulmonic does to that of the aortic orifice, *i. e.* were the tricuspid, for example, twice the size of the mitral orifice in area, the pulmonic would be twice the size of the aortic orifice in area, the two ratios differing from each other only by one-tenth.

Again, if we adopt Dr. Reid's measurements of the circumference of the healthy cardiac orifices (these measurements being given in Dr. Peacock's work), we shall find this law to be more conclusively proved.

According to Dr. Reid the measurements of the circumferences are as follows:—

	Male.	Female.
Tricuspid	5·3 in.	4·9 in.
Pulmonic	3·7	3·5
Mitral	4·6	4·2
Aortic	3·2	3·

from which data we find the areas to be :—

Tricuspid	2·24 sq. in.	1·9 sq. in.
Pulmonic	1·01	1
Mitral..............	1·7	1·4
Aortic..............	·8	·71

And if we make the same comparison of areas as we did in Dr. Peacock's measurements, we find :—

Males.

$$\frac{\text{Area of tricuspid}}{\text{Area of mitral}}=\frac{2{\cdot}24}{1{\cdot}7}=1{\cdot}31$$

$$\frac{\text{Area of pulmonic}}{\text{Area of aortic}}=\frac{1{\cdot}01}{{\cdot}8}=1{\cdot}26$$

$$\text{Difference of the ratios} = {\cdot}05$$

Females.

$$\frac{\text{Area of tricuspid}}{\text{Area of mitral}}=\frac{1{\cdot}9}{1{\cdot}4}=1{\cdot}36$$

$$\frac{\text{Area of pulmonic}}{\text{Area of aortic}}=\frac{1}{{\cdot}71}=1{\cdot}40$$

$$\text{Difference of the ratios} = {\cdot}04$$

It is well known that no measurements can be taken of such orifices as those of the heart without liability to error; but no one can observe the close identity of the respective ratios without concluding that the ratios are really identical, and that the small differences in the calculated results depend entirely upon the impossibility of obtaining absolutely correct measurements of the boundaries of such openings. It is clear, therefore, that in whatever proportion the tricuspid is larger than the mitral, in exactly the same proportion is the pulmonic larger in area than the aortic orifice. This rule applies, of course, to the human heart only in its healthy state; but I shall show that its application is of practical value when we consider the organ in its diseased state.

I shall now proceed to prove that the law which I have deduced from independent observations made in the healthy human heart is of far wider application, for I have found by my own measurements that a comparison of the areas of the same orifices in animals reveals the same result.

The following are the facts at which I have arrived by careful and repeated measurements of the cardiac apertures in different animals.

The measurements are individual, and not mean, and therefore less liable to error.

[redacted]

Horse.

	Circumference.	Area.
Tricuspid	9·25 in.	6·8 sq. in.
Pulmonic	6·5	3·6
Mitral	8·2	5·3
Aortic	5·9	2·8

$$\frac{\text{Tricuspid}}{\text{Mitral}}=\frac{6\cdot 8}{5\cdot 3}=1\cdot 283$$

$$\frac{\text{Pulmonic}}{\text{Aortic}}=\frac{3\cdot 6}{2\cdot 8}=1\cdot 285$$

Difference of the ratios = ·002

Donkey.

Tricuspid	6·2 in.	3·06 sq. in.
Pulmonic	4·1	1·34
Mitral	5·5	2·40
Aortic	3·7	1·09

$$\frac{\text{Tricuspid}}{\text{Mitral}}=\frac{3\cdot 06}{2\cdot 40}=1\cdot 27$$

$$\frac{\text{Pulmonic}}{\text{Aortic}}=\frac{1\cdot 34}{1\cdot 09}=1\cdot 23$$

Difference of the ratios = ·04

Ox.

Tricuspid	7·5 in.	4·48 sq. in.
Pulmonic	4·8	1·83
Mitral	6·6	3·47
Aortic	4·2	1·40

$$\frac{\text{Tricuspid}}{\text{Mitral}}=\frac{4\cdot 48}{3\cdot 47}=1\cdot 29$$

$$\frac{\text{Pulmonic}}{\text{Aortic}}=\frac{1\cdot 83}{1\cdot 40}=1\cdot 30$$

Difference of the ratios = ·01

Calf.

Tricuspid	5 in.	2 sq. in.
Pulmonic	3·2	·81
Mitral	4·3	1·47
Aortic	2·7	·58

$$\frac{\text{Tricuspid}}{\text{Mitral}} = \frac{2}{1{\cdot}47} = 1{\cdot}36$$

$$\frac{\text{Pulmonic}}{\text{Aortic}} = \frac{{\cdot}81}{{\cdot}58} = 1{\cdot}40$$

Difference of the ratios = ·04

Sheep.

	Circumference.	Area.
Tricuspid	3·7 in.	1·09 sq. in.
Pulmonic	2·5	·49
Mitral	3·2	·81
Aortic	2·1	·35

$$\frac{\text{Tricuspid}}{\text{Mitral}} = \frac{1{\cdot}09}{{\cdot}81} = 1{\cdot}34$$

$$\frac{\text{Pulmonic}}{\text{Aortic}} = \frac{{\cdot}49}{{\cdot}35} = 1{\cdot}40$$

Difference of the ratios = ·06

Sheep.

Tricuspid	4·25 in.	1·435 sq. in.
Pulmonic	2·70	·580
Mitral	3·70	1·090
Aortic	2·30	·420

$$\frac{\text{Tricuspid}}{\text{Mitral}} = \frac{1{\cdot}435}{1{\cdot}090} = 1{\cdot}316$$

$$\frac{\text{Pulmonic}}{\text{Aortic}} = \frac{{\cdot}58}{{\cdot}42} = 1{\cdot}380$$

Difference of the ratios = ·064

Pig.

Tricuspid..............	3·95 in.	1·24 sq. in.
Pulmonic..............	2·55	·51
Mitral	3·50	·97
Aortic	2·25	·40

$$\frac{\text{Tricuspid}}{\text{Mitral}} = \frac{1{\cdot}24}{{\cdot}97} = 1{\cdot}278$$

$$\frac{\text{Pulmonic}}{\text{Aortic}} = \frac{{\cdot}51}{{\cdot}40} = 1{\cdot}275$$

Difference of the ratios = ·003

Pig.

	Circumference.	Area.
Tricuspid	3·6 in.	1·03 sq. in.
Pulmonic	2·5	·49
Mitral	3·2	·81
Aortic	2·1	·38

$$\frac{\text{Tricuspid}}{\text{Mitral}}=\frac{1\cdot 03}{\cdot 81}=1\cdot 27$$

$$\frac{\text{Pulmonic}}{\text{Aortic}}=\frac{\cdot 49}{\cdot 38}=1\cdot 29$$

Difference of the ratios = ·02

Dog.

Tricuspid..............	3·65 in.	1·07 sq. in.
Pulmonic..............	1·9	·287
Mitral................	3·15	·79
Aortic	1·6	·204

$$\frac{\text{Tricuspid}}{\text{Mitral}}=\frac{1\cdot 07}{\cdot 79}=1\cdot 36$$

$$\frac{\text{Pulmonic}}{\text{Aortic}}=\frac{\cdot 287}{\cdot 204}=1\cdot 40$$

Difference of the ratios = ·04

Dog.

Tricuspid................	2·9 in.	·69 sq. in.
Pulmonic................	1·6	·204
Mitral	2·5	·49
Aortic	1·4	·156

$$\frac{\text{Tricuspid}}{\text{Mitral}}=\frac{\cdot 69}{\cdot 49}=1\cdot 40$$

$$\frac{\text{Pulmonic}}{\text{Aortic}}=\frac{\cdot 204}{\cdot 156}=1\cdot 31$$

Difference of the ratios = ·09

'rom these facts we may fairly conclude that in the healthy human heart, most probably in the hearts of most animals, the areas of the four rtures bear an exact mathematical relationship to each other, and conıently that if the areas of any three of the openings be known, the area he fourth orifice can be correctly calculated.

need scarcely dwell upon the importance of a knowledge of this law in nating the amount of contraction or dilatation of orifice which a morbid imen may present. I will, however, now show from my own measure-

ments how this law was applied, and how closely the observed and calculated results agreed in the case of a strong healthy man who died in the London Hospital from the effects of a fractured spine. The heart was perfectly healthy. I carefully measured the pulmonic, mitral, and aortic orifices, calculated the area of the tricuspid, and then measured its circumference. Having worked out its area, I was able to observe what difference existed between the result of actual measurement and the result derived from "the law of the orifices" which I had discovered.

Human Heart.

	Circumference.	Area.
Pulmonic	3·55 in.	1·003 sq. in.
Mitral	4·20	1·405
Aortic	3·10	·765

Now, by the law of the orifices,

$$\frac{\text{Area of tricuspid}}{\text{Area of mitral}}=\frac{\text{Area of pulmonic}}{\text{Area of aortic}};$$

$$\therefore \text{area of tricuspid}=1{\cdot}405\times\frac{1{\cdot}003}{{\cdot}765}$$
$$=1{\cdot}972.$$

By measurement,—

	Circumference.	Area.
Tricuspid	=5·1 in.	2·070 sq. in.

Area of tricuspid by measurement = 2·070 sq. in.
Area of tricuspid by calculation = 1·972

Difference between calculated and observed results .... = ·098

The calculated and observed results differ so little from each other, that this case evidently strongly corroborates the correctness of the law which I believe regulates the relative magnitude of the areas of the four cardiac openings.

If, moreover, we scrutinize the measurements, we shall observe an equally important fact, that the ratio of the areas of any two corresponding orifices is almost constant in the same, and, I may almost add, in all animals, man included.

Thus the area of the tricuspid is nearly 1·3 times the area of the mitral orifice, and the area of the pulmonic of course bears the same proportion to that of the aortic opening. By measuring, therefore, the two orifices of the right (supposed healthy), we are enabled by this law to deduce approximately the magnitude of the areas of those of the left heart, and *vice versâ*. One healthy orifice being known, the area of the corresponding opening in the other side of the heart can be approximately calculated; and should the latter be diseased, its deviation from the normal area can be

determined, and the amount of abnormal contraction or dilatation fairly estimated.

To illustrate the value of this approximative law, I will exemplify, in a case of mitral constriction detailed by Dr. Walshe (Diseases of the Heart p. 373), the mode in which the amount of constriction may be calculated.

Mitral Constriction.

	Circumference.	Area.
Tricuspid..........	$4\frac{7}{8}=4{\cdot}875$ in.	$1{\cdot}9$ sq. in.
Pulmonic.....	$3\frac{1}{8}=3{\cdot}125$	$\cdot77$
Mitral	$1\frac{7}{8}=1{\cdot}875$	$\cdot28$
Aortic	$2\frac{3}{8}=2{\cdot}375$	$\cdot45$

$$\frac{\text{Tricuspid}}{\text{Mitral}}=\frac{1{\cdot}9}{\cdot28}=7, \text{ nearly.}$$

$$\frac{\text{Pulmonic}}{\text{Aortic}}=\frac{\cdot77}{\cdot45}=1{\cdot}7, \text{ nearly.}$$

Hence the tricuspid (by reason of the extreme narrowing of the mitral opening) is seven times larger in area than the latter orifice, in place of being only 1·3 to 1·4 times larger in area. If we suppose the tricuspid to be nearly normal, then as

$$\frac{\text{Area of tricuspid}}{\text{Area of mitral}}=1{\cdot}3, \text{ nearly;}$$

$$\therefore \text{ area of mitral (healthy)} = \frac{\text{area of tricuspid}}{1{\cdot}3}$$

$$=\frac{1{\cdot}9}{1{\cdot}3}=1{\cdot}45 \text{ sq. in.}$$

Hence the amount of the contraction of

$$\begin{aligned} \text{the mitral orifice} &= 1{\cdot}45, \text{ the normal size} \\ &- \ \ \cdot28, \text{ its actual size.} \\ &= 1{\cdot}17 \text{ sq. in.} \end{aligned}$$

"The diseased aperture just admitted the end of the index figure; its edge was rugose, and the valve was funnel-shaped towards the ventricle. The left auricle was much hypertrophied, its walls in some parts being ¼ inch in thickness, and its endocardium creaked on being touched." The pulmonic is evidently large in proportion to the aortic opening (the ratio being 1·7 instead of 1·3 to 1·4); and there was no doubt considerable hypertrophy and dilatation of the right ventricle. The increase in the area of the pulmonic aperture was the direct result of this condition of the right side of the heart. The tricuspid was also probably somewhat dilated, as the "valves looked insufficient to fill the widened orifice," and the jugular veins appeared during life to be swollen and pulsatory; but the absolute size of the tricuspid shows that the dilatation was not excessive. The area of the aortic opening appears to be below the mean amount. Was

this the result of the small supply of blood which the left ventricle received and impelled into the general system? In any case a knowledge of the existence of this law enables us to read the measurements of the orifices and their respective ratios with increased interest.

It would be interesting to pursue the application of this law in the study of the various forms of valvular disease. I purpose, however, to return to this subject at the end of this paper, and shall seek now to trace out the reasons why the four orifices present such differences in the magnitude of their areas.

And as the foundations of our arguments we must admit the truth of the two following propositions:—

1st. That the ventricles and auricles act exactly synchronously respectively; and 2ndly, that equal volumes of blood pass in exactly equal and the same times respectively through any two corresponding orifices of the healthy heart.

1. "If we examine," says M. Marey "the lines traced by the right and left ventricles, we find a most perfect synchronism in the respective commencements and terminations of their contraction."

"The examination also of a heart exposed during life confirms the deduction; for if we grasp the auricles or the ventricles, we cannot detect the smallest interval between the contractions of parallel cavities."

Again. Stethoscopic examination of the heart demonstrates the existence of only one first sound and of only one second sound, although the causes producing each of those sounds are twofold, inasmuch as they really reside in two (right and left hearts), placed in close and intimate apposition to one another. Under rare circumstances the sound which results from the closure of the semilunar valves has been found reduplicated; but although such an event may occur from the non-synchronous fall of the valves, it is clear that an unimpeded and uninterrupted circulation could not be maintained unless the two sides of the heart, or really the two hearts, contracted and dilated exactly synchronously. Whether the organ acts violently or feebly, with regularity or intermittently, the auscultator detects but two sounds; and even when its valves are diseased, its orifices irremediably altered in diameter, and its muscular walls hypertrophied or atrophied, we find the same law of synchronism presiding over the heart and its sounds, normal or abnormal.

Lastly. An examination by dissection of the fibres which compose the walls of the ventricles, conclusively proves that these chambers must inevitably act exactly synchronously. In Dr. Pettigrew's masterly account of the arrangement of the muscular fibres in the ventricles of vertebrate animals, we find the following remarks made upon this point:—"The fibres of the right and left ventricles anteriorly and septally are to a certain extent independent of each other; whereas posteriorly many of them are common to both ventricles; i. e. *the fibres pass from the one ventricle to the other.*" The drawings 49 and 50 in the memoir clearly prove how "the common

fibres pass from the left to the right ventricle and dip in or bend at the track of the anterior coronary artery to become continuous with fibres having a similar direction in the septum"*.

2. In the next place, it must be admitted that equal volumes of blood pass in exactly equal and the same times through any two corresponding orifices of the heart; for if, for example, we could suppose the quantity thrown out through the pulmonic orifice into the lungs to be *persistently* greater than the amount thrown out in *the same time* through the aortic opening into the general circulation, it would inevitably follow that overwhelming pulmonary engorgement, cessation of flow from the right heart, and death would rapidly ensue. The alternative supposition of the right ventricle persistently discharging into the lung-capillaries an amount of blood actually less than the quantity as persistently set forth by the left ventricle into the systemic circulation, involves a physical contradiction unnecessary to refute. Whatever, therefore, may be the actual capacities of the ventricles, or the quantities which under pressure they may be made to contain, this law must be always paramount to enable the healthy heart to act freely and without the production of a congested or overloaded condition of the pulmonic or systemic circulations; *the quantities of blood entering the ventricles synchronously must be equal,* and the quantities leaving them synchronously must also be equal; and to prevent the occurrence or production of cardiac congestion the quantity of blood received by the ventricles in diastole must equal the quantity expelled by the ventricles in systole, small deviations being allowed within certain limits of health. We shall see the bearing of these latter remarks when we consider the mode in which hearts much diseased in their orifices and valvular apparatus are often enabled to carry on a tolerably unembarrassed circulation, and with but little functional disturbance experienced by the individual so circumstanced.

The anatomy of the organ fully corroborates the principle we are seeking to establish; for we are told that "the capacities of the ventricles are probably equal" (Cruveilhier); and again, "there are reasons for believing that during life any difference between the capacities of the ventricles is very trifling, if it exist at all"†.

And lastly, "the whole, or very nearly the whole of the blood contained in the ventricles is discharged from them at each systole; for the left ventricle is frequently found quite empty after death; and if a transverse section be made through the heart in a state of well-marked rigor mortis (which may be considered as representing its ordinary state of complete contraction), the ventricular cavity is found to be completely obliterated."

From these considerations we may, I believe, fairly assume that

$$(1)\quad \begin{cases} \text{Equal times of ventricular contraction,} \\ \text{Equal times of ventricular dilatation,} \end{cases}$$

* Phil. Trans. part 3, 1864.

† Quain's 'Anatomy,' by Dr. Sharpey, vol. iii. p. 255.

(2) { Equal or almost equal volumes of blood received in diastole,
Equal or almost equal volumes of blood expelled in systole,

(3) Equal or almost equal capacities of ventricles,

are the main characteristics of a heart which is normal in structure and perfect in function.

(1) In employing the words equal times with reference to the periods respectively occupied by the contraction and dilatation of the ventricles, I would wish to refer for a moment to the statements made by our leading authorities as to the average duration of the systole and diastole of the healthy heart.

Dr. Carpenter states that the ventricular contraction occupies $\frac{2}{5}$ and the ventricular dilatation $\frac{3}{5}$ of the time which elapses between two consecutive beats of the pulse. Dr. Walshe informs us that the time from the commencement of the first to the beginning of the second sound is, on an average, one half of the time from pulse to pulse. Dr. Burdon Sanderson, in his Handbook of the Sphygmograph, says, "There are several facts not difficult of observation which show that the time occupied by the heart in contracting is very much shorter than is commonly supposed. The first sound being synchronous with the commencement of the contraction of the ventricles and the closure of the mitral valve, and the second with the closure of the aortic valves, it is clear that the interval between these two events expresses the duration of the contraction of the heart. Now the most unpractised auscultator can readily satisfy himself, while listening to the sounds of a heart contracting sixty times in a minute, that the time between the first and second sounds is not equal to that which separates the second from the first; and that it cannot be admitted for a moment (as stated in our leading physiological text books) that a heart occupies half of a second in contracting."

This statement is borne out in the last edition of Kirkes's 'Physiology,' edited by Morrant Baker, in which the periods of ventricular contraction and dilatation are considered to be in the ratio of 4 to 7. Chauveau's experiments on the living horse and the sphygmographic tracings of the radial pulse in man, clearly indicate that the times of ventricular contraction and dilatation are very different in duration; and the inferences which are deducible from the study of the comparative areas of the four orifices will fully substantiate the statement that the systole of the ventricles "is a much shorter proceeding than is usually supposed."

(2) And again, with regard to the words "equal volumes of blood" used above, I need scarcely remark that the same volume (quantity, ounces, cubic inches) of blood is not persistently and at all times received by and thrown out of the heart at every complete revolution of the organ. The reverse is, in fact, nearer the truth; for the ventricles (though of course always full from the impossibility of a vacuum existing in their interior) vary considerably from time to time in their degree of fulness and expansion. In profound sleep, or in the perfect rest and muscular relaxation of

the recumbent posture, the flow of blood through the heart is entirely and solely under the control of the heart itself (some allowance being made for the effects of the respiratory movements which "act on the whole advantageously to the circulation"), the right being filled by the contractile energy of the left side of the organ. In our waking moments, however, during exertion, every movement of the body tends to force the blood in the veins in an onward course towards the right chambers of the heart, which would become gorged from over-distension did not the healthy right ventricle assume corresponding energy and force and expel the blood with increased rapidity into the capillaries of the lungs. An increase in the number and depth of the respiratory movements ensues, accelerating the passage of the blood through the lungs to the left side of the heart, which, by an instinctively increased reaction upon its contents, propels the blood forcibly into the systemic circulation. The so-called vital capillary force or interaction between blood and tissue may assist in forwarding the current, but its amount is evidently excessively small in comparison with the enormous contractile energy of the two ventricles. Violent and sudden exertion may for a short time disturb the balance between the two hearts (the cavæ and right auricle in one side, and the pulmonary vessels and left auricle in the other side being, for a time, the safety reservoirs or receptacula of the blood waiting to be forwarded); but with bodily rest equilibrium becomes rapidly reestablished, and equal volumes of blood are again poured forth in equal and the same times from the two ventricles of the heart.

Returning from this digression to the immediate subject of this paper, we have to consider the cause of the differences in the areas of the four principal orifices of the heart.

The right and left sides of that organ are, to all intents and purposes, two distinct and perfect hearts, discharging individually their own proper functions, but associated in one common interest by certain bands of muscular fibres and intercommunicating nerve-ganglia. Now if these two hearts had exactly equal tasks to perform and were simply designed to propel the contents of their ventricles to equal distances and with equal velocities, if, in a word, they had been intended to overcome equal obstacles in the pulmonic and systemic circulations respectively, their walls would have been undoubtedly constructed of equal thickness, and the corresponding orifices of the two sides would have been of equal areas, the tricuspid being equal to the mitral and the pulmonic to the aortic aperture. But as the left ventricle has to propel the blood to far greater distances, and to overcome obstacles much greater than those found in the pulmonic circulation, the velocity and force of the stream sent from the left must be evidently greater than the velocity and force of the blood thrown out by the right ventricle. To secure this result, I need scarcely say that the left is rendered considerably thicker and stronger than the right ventricle by the greater development of its walls; *but here* we must bear in mind the *cardinal fact* (the key to the entire question) that, whatever be the velocity

and force of the streams issuing from the two ventricles, the *quantities* of blood expelled by the synchronous contraction of the two chambers must be exactly the same, or else accumulation in the pulmonic or systemic systems would ensue, and the machine be brought to a standstill.

As, therefore, the two ventricles contracting with *unequal forces* have to expel *equal quantities* of blood in *equal and the same time* to *unequal distances* and to overcome *unequal resistances*, the perfect synchronism of the ventricular contractions can be only obtained by an exact graduation of the areas of the orifices of the aortic and pulmonary artery to the muscular forces respectively impressed upon the contents of the two ventricles in systole, and consequently to the velocities of the streams issuing from those chambers. The area of the aortic must be therefore smaller than the area of the pulmonic, and *in such proportion* that the normal average contents (say, three ounces) of the left ventricle shall occupy exactly the same time in passing through the aortic as is required by the three ounces of the right ventricle in passing through the pulmonic opening. The greater muscular power of the left, as compared with that of the right ventricle, causes a corresponding greater velocity and force of the column of blood issuing from its outlet, while the smaller area of the aortic, as compared with that of the pulmonic opening, exactly equalizes the times occupied by the contractions of the two chambers. Without such an arrangement in the comparative areas of the two outlets, it is clear that the stronger left would completely empty itself before the right ventricle had accomplished the same function, and the synchronous action of the two hearts would be thus rendered impossible. Equal quantities of blood are, however, in the way described, made to pass exactly synchronously through the aortic and pulmonic openings, but with, of course, unequal velocities, the blood-particles which traverse the narrow aortic travelling with greater speed than those which pass through the larger pulmonic orifice. Mathematically expressed, the velocities of the streams through the orifices are inversely as the areas of those orifices, or

$$\frac{\text{velocity through aortic opening}}{\text{velocity through pulmonic opening}} = \frac{\text{area of pulmonic opening}}{\text{area of aortic opening}}.$$

And if we assume the mean measurements of the orifices found in the former part of this paper to be correct,

$$\left.\begin{array}{c}\text{the velocity through}\\ \text{aortic opening}\end{array}\right\} = \frac{1 \text{ sq. inch}}{\cdot 75 \text{ sq. inch}} \times \text{velocity through pulmonic opening}$$

$$= 1 \cdot 3 \text{ time the velocity through pulmonic opening}$$

$$= 1\tfrac{1}{3} \text{ time the velocity through pulmonic opening},$$

or, in other words, the velocities of the currents through the aortic and pulmonic orifices are in the ratio of 4 to 3.

The arguments which I have advanced respecting the aortic and pulmonic will be equally applicable to the tricuspid and mitral openings; for:—

1st. The two ventricles are exactly synchronous in their diastole, re-

ceiving their respective charges of blood from the auricles in exactly equal and the same times.

2nd. Equal volumes of blood enter the two ventricles during their diastole, or else accumulation and stagnation would ensue: I am speaking here of healthy ventricles.

3rd. The ventricles are of equal capacities; but

4th. As the currents which traverse the tricuspid and mitral orifices are of unequal velocities, the areas of those openings must be of such magnitudes that equal volumes of blood must pass through them in exactly equal and the same times. The tricuspid having a slower velocity than the mitral current, will necessitate the area of the tricuspid being proportionally larger than the area of the mitral orifice. In a word, the synchronous dilatation of chambers, admitting equal volumes of blood must entail such a relation of area between the two inlets that

$$\frac{\text{the velocity through tricuspid}}{\text{the velocity through mitral}}=\frac{\text{area of mitral}}{\text{area of tricuspid}}.$$

And if we assume the measurements previously found to be correct,

$$\text{the velocity through tricuspid}=\frac{1\cdot 25}{1\cdot 75}\text{ velocity through mitral,}$$

$$=\tfrac{5}{7}\text{ velocity through mitral,}$$

i. e. the velocities of the currents of blood in diastole through the tricuspid and mitral orifices are in the ratio of 5 to 7.

It may be fairly asked what proofs can be given that the velocities of the currents of blood which traverse the tricuspid and mitral orifices are unequal, and that the mitral incoming stream possesses a stronger ventricular dilating power than the current which enters the tricuspid to expand and fill the right ventricle. I shall refer to this point shortly; but whatever may be the value of the reasons which will be adduced in support of the above view, there can be no doubt, *in fact*, that the two orifices in healthy hearts always differ in size, and the synchronous expansion of ventricles with unequal inlets must inevitably lead to this result—that the larger must admit a current of correspondingly smaller velocity than that which traverses the smaller opening; or, mathematically expressed, the velocities of the incoming tricuspid and mitral streams must be inversely as the areas of the orifices.

From the data at which we have arrived, and estimating the mean amount of the ventricular contents at three ounces (or five cubic inches, nearly), although it must be confessed that this is an uncertain estimate, we may readily calculate the average velocities of the currents which traverse the four orifices. We shall consider the pulse to beat at the rate of 70 per minute, and the periods of ventricular contraction and dilatation to be in the ratio of 1 to 2, *i. e.* the ventricular contraction occupying one-third of the time between two pulses; *i. e.*

$$=\tfrac{1}{3}\text{ of }\tfrac{1}{70}'=\tfrac{1}{210}'.$$

1. *Aortic Orifice.*

$$\text{Velocity through aortic orifice} = \frac{\text{volume expelled in } \frac{1}{310}'}{\text{area of aortic orifice}}$$

$$= \frac{5 \text{ cub. inches}}{\cdot 75 \text{ sq. in.}} \text{ in } \frac{1}{310}'$$

$$= 23\cdot 1 \text{ inches in one second}$$

$$= 2310 \text{ yards per hour.}$$

2. *Pulmonic Orifice.*

$$\text{Velocity through pulmonic orifice} = \tfrac{3}{4} \text{ velocity through aortic opening}$$

$$= 17\cdot 3 \text{ inches in one second}$$

$$= 1725 \text{ yards per hour.}$$

3. *Tricuspid Orifice.*

$$\text{Velocity through tricuspid} = \frac{5 \text{ cub. inches}}{1\cdot 75 \text{ sq. in.}}$$

$$= \frac{1}{\cdot 35} \text{ inch in } \frac{1}{1\cdot 75}\, 1'$$

$$= 5 \text{ inches in one second}$$

$$= 500 \text{ yards per hour.}$$

4. *Mitral Orifice.*

$$\text{Velocity through mitral} = \tfrac{7}{5} \text{ velocity through tricuspid}$$

$$= 7 \text{ inches in one second}$$

$$= 700 \text{ yards per hour.}$$

The mean velocities of the currents of blood traversing a healthy heart, with the dimensions of the areas as given above, are as follows:—

		yards.		mile.
Aortic	=	2310	=	1·3 per hour
Pulmonic	=	1725	=	1 nearly
Mitral	=	700	=	·4
Tricuspid	=	500	=	·28

In such a heart we see, therefore, that the blood enters the tricuspid orifice at the rate of nearly $\frac{1}{4}$ mile per hour, and leaves it through the aortic orifice at the rate of nearly $1\frac{1}{4}$ mile per hour; and that the velocity, therefore, of the tricuspid incoming current is only one-fifth of the velocity of the stream which passes through the aortic orifice.

Without entering into arithmetical details, such a result as the above is easily arrived at when we bear in mind the facts that the same quantity of blood passes through the two openings, but that while the tricuspid is, according to Dr. Peacock, $2\frac{1}{2}$, and according to Dr. Reid nearly three times larger than the aortic orifice, the flow of the three ounces through the former occupies nearly twice the time required by

the passage of the same quantity of blood through the latter opening. The tricuspid is nearly three times larger than the aortic aperture, and is open for the transmission of the same volume of blood more than double the length of time occupied by the latter opening. Hence the comparative slowness of the incoming tricuspid current.

These speculations upon the absolute and relative velocities of the currents of blood through the heart are not without practical value, inasmuch as they have a direct bearing upon the question of the amount of pressure exerted by that fluid in each chamber of the organ, and are links in the chain of reasoning respecting the comparative areas of the four orifices. The first-recorded experiments to determine this pressure were made by Dr. Stephen Hales, F.R.S., and were published by him in his 'Statical Essays' in 1732. Thus, when tubes were fixed into the crural artery and jugular vein of different animals, the heights to which the blood rose were found to be as follows:—

	Artery.	Vein.
Horse	114 inches.	12 inches.
Sheep	77½	5½
Dog	48	4½

These experiments were, of course, rather roughly made and without modern appliances; but they serve to show that the pressure of the blood in the jugular vein is only one-ninth to one-fourteenth of the pressure observed in the arterial side of the circulation. Valentin, by means of the hæmadynamometer, estimated the pressure in the jugular vein to be one-tenth to one-twelfth of the pressure in the carotid artery, and "in the upper part of the inferior vena cava could scarcely detect the existence of any pressure, nearly the whole force from the heart having been apparently consumed during the passage of the blood through the capillaries" (Kirkes and Paget).

It is thus sufficiently clear, experimentally, that the velocity and momentum of the blood which enters the right auricle and finds its way into the right ventricle must be very small in comparison with the rapidity and momentum of the current issuing from the left ventricle; and we can therefore, from this fact, understand that the tricuspid is constructed of much greater area than the aortic opening, in order that its much larger orifice may compensate for the comparatively sluggish stream which it has to transmit. It is evident enough why the blood which has returned to the right heart possesses so small an amount of velocity and momentum. In its passage through the systemic circulation it has encountered and overcome an amount of obstruction which, by the time it has arrived in the right auricle, has deprived it of the greater portion of the velocity and momentum which it had derived from the contractile energy of the left ventricle, assisted, as that power has been, by the muscular pressure on the veins of the body. The columns of blood from the superior and infe-

rior venæ cavæ enter the auricle, therefore, slowly, and with small force, but with an amount of velocity and momentum exactly adapted to and sufficient for the expansion of the right ventricle. It cannot be for one moment maintained that the right is weaker than the left heart in proportion to the work to be done by the respective sides, for each organ is exactly adapted to the task which it has to perform, and the perfection of the mechanism is as manifest in one as in the other side of the heart. The right side is undoubtedly exposed to sudden and great variations in the amount of blood-pressure to which it is from time to time subjected; but there can be no reason for believing that provision has not been made for such variations *within due limits*. In fact daily experience shows us how the right side will maintain its vigour unimpaired, although severely and often tried by the alterations in the blood-pressure resulting from rapid walking, running, pulling at the oar, and the usual athletic exercises. The slowness of the current which returns to the right side, the large area of the tricuspid orifice, and the comparatively long period of time during which the ventricle is open to receive its contents, evidently confirm the view that the right ventricle offers but little resistance to the incoming current, and that a stream of small velocity and energy is amply sufficient to fill and complete the expansion of that chamber. The force which is exerted by the contraction of the auricle is small, and in operation for a short period of time ($\frac{1}{8}$ to $\frac{1}{10}$ of a minute), and is chiefly of use, I believe, in completing the closure of the tricuspid valve in the manner described by Baumgärtner, Valentin, and Halford. It must be also borne in mind that the particles of the blood-stream which have entered the tricuspid orifice in a direction nearly at right angles to the axis of the pulmonary artery must, when the ventricle has become filled, change their direction of motion to find their way in systole out of the ventricle. At the end, therefore, of the diastole I imagine that the whole of the contents of the ventricle is at rest (momentarily, but not less really so), and ready to take up a new movement in a course nearly at right angles to its line of entrance from the auricle. If this view, which has escaped the attention of physiologists, be correct, we observe an additional reason for the blood which enters the ventricle possessing an amount of velocity and energy just sufficient, and no more, to complete the dilatation of the chamber, and having performed its task, to assume for a moment an attitude of repose before the contraction of the ventricle sends it forth in a different direction. All force in the human body is economized, the means is strictly adapted to the end: the left ventricle puts forth power sufficient to carry the blood through the systemic capillaries to the right side of the heart; the blood enters the right auricle with an amount of pressure sufficient, with the aid of the auricular contraction, to fill the ventricle, and should any excess of momentum exist, it is probably annihilated by the incoming current meeting the dense interlacement of the fibres of the columnæ carneæ which form such prominent parts of the interior of both ventricles. It is

interesting to observe that this interlacement is most dense near the apex, where the incoming current impinges with greatest force, and where the excess of momentum of the blood can be easier annihilated before it changes its direction of motion to escape in systole from the chamber. The remarks which belong to the right are equally applicable to the left ventricle, and lead to the conclusion that the current entering through the mitral orifice, as soon as the chamber is filled, loses its motion for a moment before the contractile force of the ventricle launches it forth in a totally different direction through the aortic opening into the systemic circulation.

I will now return to the statement which I had left unproved, that the velocities of the synchronous tricuspid and mitral currents are unequal, and that the latter possesses a stronger ventricle-dilating power than the former.

The argument to establish this point is very brief.

The two ventricles being of unequal thickness and containing consequently unequal quantities and weights of muscular fibre, will necessarily require currents of blood of unequal momenta to overcome their respective *inertia*, fill their chambers, and complete their dilatation in exactly equal and the same times. That is,

the momentum of the mitral is greater than the momentum of the tricuspid current;

or, in other words,

the volume of the mitral column multiplied by its velocity *is greater than* the volume of the tricuspid column multiplied by its velocity;

but the volume of each current *is the same*; hence, eliminating *volume* from each side of the above, it is evident that

the velocity of the mitral is greater than the velocity of the tricuspid current, the conclusion which was to be demonstrated*.

In concluding this paper, I would very briefly recapitulate the conclusions at which I have arrived. I have proved, from the measurements of the orifices made by Drs. Peacock and Reid, that the areas of the openings in man are subject to a constant law, summarily expressed thus:—

$$\frac{T}{M}=\frac{P}{A}.$$

And the same result I have also obtained from my own measurements of

* I have employed the word momentum as one in common use; but the term *vis viva* would have been more correct, inasmuch as the latter expression represents the mass of a body in motion multiplied by the *square* of its velocity. In this case, therefore, while the velocities of the tricuspid and mitral moving masses of blood are in the ratio of 5 to 7, the energy or *vis viva* of the tricuspid is to the energy or *vis viva* of the mitral current or mass as 25 to 49. This observation does not affect the reasoning employed above, but shows the greatness of the disparity between the ventricle-dilating powers of the two incoming currents.

the healthy heart. Furthermore, I have proved, from my own measurements, that the same law probably regulates the areas of the orifices in animals generally; and I have cited several examples in corroboration of the statement. From the existence of this law, it is clear that if the areas of any three healthy orifices be known, the area of the fourth can be determined by calculation.

I have then drawn attention to the curious and important fact which appears to be almost general in animals, that

$$\frac{T}{M} = 1{\cdot}3 \text{ to } 1{\cdot}4, \text{ nearly};$$

and

$$\frac{P}{A} = 1{\cdot}3 \text{ to } 1{\cdot}4, \text{ nearly};$$

consequently that the dimensions of the openings of one side of the heart being given, the areas of the corresponding orifices on the other side of the organ may be obtained by arithmetical process.

Having shown from measurements that the orifices arranged in the order of their magnitude are as follows,

1. Tricuspid,
2. Mitral,
3. Pulmonic,
4. Aortic,

I have sought to determine the reasons for this arrangement, to which however, I shall not again refer.

I propose on some future occasion to show how widely this "*law of the orifices*" which I have discovered is applicable to the heart in its diseased state, and how it serves to explain many important and interesting points relative to the organ. I shall conclude with citing a few instances in which its application throws some light on the effects of pulmonary disease upon the areas of the orifices.

1. *Phthisis* (Dr. Peacock).

	Circumference.	Area.
Tricuspid	51 lines.	207 square lines.
Pulmonic	39	121
Mitral	18	183
Aortic	36	103

$$\frac{T}{M} = \frac{207}{183} = 1{\cdot}13$$

$$\frac{P}{A} = \frac{121}{103} = 1{\cdot}17$$

Difference of the ratios $= \cdot 04$

2. *Phthisis* (Dr. Peacock).

Tricuspid	43 lines.	147 square lines.
Pulmonic	29	67
Mitral	37	109
Aortic..............	26	54

$$\frac{T}{M}=\frac{147}{109}=1{\cdot}35$$

$$\frac{P}{A}=\frac{67}{54}=1{\cdot}25$$

Difference of the ratios = ·10

In these two cases the orifices evidently closely exhibit the usual normal relation to each other; and as the blood in phthisis emaciates and diminishes in quantity like the other parts of the body, we should not expect in a pure case of phthisis an amount of pulmonary obstruction sufficient to produce marked alterations in the areas of the openings.

3. *Bronchitis* (Dr. Peacock).

Tricuspid	60 lines.	286 square lines.
Pulmonic	45	193·07
Mitral	54	232·1
Aortic	26	103·18

$$\frac{T}{M}=\frac{286{\cdot}6}{232{\cdot}1}=1{\cdot}235$$

$$\frac{P}{A}=\frac{193{\cdot}07}{103{\cdot}18}=1{\cdot}870$$

Difference of the ratios = ·635

The relation is clearly abnormal.

T and M are nearly in normal relation to each other, but P is abnormally large in relation to A. This heart was enlarged (weighed 14 ounces) from hypertrophy and dilatation of its right side, and the pulmonic orifice, from the abnormal increase of the pressure of the current of blood sent through it by the thickened right ventricle, became considerably dilated.

4. *Bronchitis* (Dr. Davies).

Tricuspid	5 in.	2 sq. in.
Pulmonic	3·5	·975
Mitral..................	3·8	1·15
Aortic..................	3·1	·79

$$\frac{T}{M}=\frac{2}{1{\cdot}15}=1{\cdot}74$$

$$\frac{P}{A}=\frac{{\cdot}975}{{\cdot}79}=1{\cdot}24$$

Difference of the ratios = ·50

The tricuspid is evidently abnormally large in relation to the mitral orifice.

If we suppose the other three orifices to be nearly normal (and it is evident that the pulmonic and aortic bear their normal relation to one another), we can calculate the excess of dilatation exhibited by the tricuspid opening

$$\frac{T}{M}=\frac{P}{A};$$

$$\therefore T=1\cdot15\times\frac{\cdot975}{\cdot79}=1\cdot42 \text{ sq. inch.}$$

Hence the tricuspid is ·58 inch in excess of its normal area, or more than one-third of its proper size in excess.

5. *Bronchitis* (Dr. Peacock).

The heart in this case weighed eleven ounces. As this does not much exceed its usual weight, it is clear that the pulmonic obstruction was slight. We should therefore expect to find but little deviation from the "law of the orifices."

Tricuspid	62 lines.	306 sq. lines.
Pulmonic..............	45	161
Mitral	54	232
Aortic	39	121

$$\frac{T}{M}=\frac{306}{232}=1\cdot32$$

$$\frac{P}{A}=\frac{161}{121}=1\cdot33$$

$$\text{Difference of the ratios} = \overline{\cdot01}$$

This result fully bears out the inferences above made.

II. "On the Estimation of Ammonia in Atmospheric Air." By Horace T. Brown, Esq. Communicated by Dr. Frankland.

Received February 19, 1870.

In the attempts that have been hitherto made to estimate the ammonia present in atmospheric air, the results arrived at by the various experimenters have differed so widely that it is still a matter of uncertainty what the quantity really is. That it is a very small amount all agree, but the extreme results on record vary as much as from 13·5 to ·01 part of carbonate of ammonium per 100,000 of air. It may therefore not be without interest to give an account of a simple method affording very concordant and, I believe, accurate results, at the same time being easy of performance and requiring but little time for an experiment.

The apparatus used consists of two glass tubes, each of about 1 metre

in length and 12 millims. bore. These are connected air-tight by means of a smaller glass tube, and inclined at an angle of 5° or 6° with the horizon. Into each of the larger tubes are introduced 100 cub. centims. of a mixture of perfectly pure water and two drops of dilute sulphuric acid (sp. gr. 1·18). Through this acidulated water a measured quantity of the air under examination is slowly drawn, in small bubbles, by means of an aspirator.

No porous substance must be used to filter the air, for reasons to be stated hereafter. The air is conducted into the absorption liquid through a small piece of quill tubing drawn out to a small aperture at the end immersed. This tube must be kept quite dry throughout the experiment. Great care must be taken to cleanse perfectly every part of the apparatus with water free from ammonia, and the caoutchouc plugs, or corks, used must be boiled for a short time in a dilute solution of caustic soda.

The stream of air is so regulated as to allow about 1 litre to pass through the apparatus in an hour.

By directing the point of the delivery-tube laterally, each bubble has imparted to it on rising an oscillatory movement which facilitates complete absorption of the ammonia.

When from 10 to 20 litres of air have passed, the liquid is emptied from the tubes into upright glass cylinders, an excess of a perfectly pure solution of potash added, and then 3 cub. centims. of a Nessler solution. The standard of comparison is made in the ordinary way, only using acidulated in place of pure water, and neutralizing with potash after adding the standard solution of ammonium salt. Beyond somewhat retarding the point of maximum coloration, a little potassium sulphate does not interfere with the delicacy of Nessler's reaction.

If the experiment has been conducted with proper care, at least $\frac{4}{5}$ of the total ammonia ought to be found in the first tube. Four or five litres of air are generally quite sufficient to give a decided reaction, but it is better to use not less than 10 litres, as before mentioned*.

Very many experiments have been made by this method, both on air from the town of Burton-on-Trent, and that of the adjoining country. The air from the town, as might be expected, varies somewhat in composition; much more so than that taken from the open country, as may be seen from the following Tables, in which are given some of the numerous results obtained.

The ammonia is calculated in every case as carbonate $\left((NH_4)_2\,CO_3\right)$; for although nitric acid is sometimes found in air, yet its presence must be looked upon as accidental.

* When the air to be examined is highly charged with ammonia, as that from stables &c., a perfectly dry bottle of 3 or 4 litres capacity should be carefully filled with a pair of bellows, 100 cub. centims. of acidulated water introduced, and, after closing securely, the whole well agitated at intervals for three or four hours. The liquid is then poured out, and the NH_3 estimated by the Nessler solution as usual.

In the immediate vicinity of towns some of the ammonia must also be in the form of sulphate, sulphite, or ammonium chloride.

(1) *Air taken from town.* (Taken at a height of 2 metres from ground.)

Date of Experiment.	$(NH_4)_2CO_3$ as grammes per 100,000 litres of air at 0° C. and 760 mm. barom.		$(NH_4)_2CO_3$ in parts by weight per 100,000 of air.
1869. September 30	1·1294	......	·8732
October 4	·62117	......	·4801
„ 6	·5251	......	·4059
„ 8	·62117	......	·4801
November 26	1·0729	......	·8293
„ 28	1·1000	......	8503

(2) *Air from country.* (Taken at a height of 2 metres.)

Date of Experiment.	$(NH_4)_2CO_3$ as grammes per 100,000 litres of air at 0° C. and 760 mm. barom.		$(NH_4)_2CO_3$ in parts per 100,000 of air.
1869. December 6	·7620	......	·5890
„ 8	·7826	......	·6085
„ 9	·6601	......	·5102
„ 11	·6635	......	·5121
1870. February 12	·7639	......	·5904

The direction of the wind does not seem to have any influence on the ammonia found; immediately after heavy rain, however, the quantity falls somewhat below the average, but the air is again restored to its normal condition after a lapse of two or three hours.

Attempts were made to make the method more delicate still by absorbing the ammonia in pure water and then distilling, but the nitrogenous organic matter suspended in the air was found to interfere with the results.

When the air is passed through cotton-wool before entering the absorption-tubes, it is found to be entirely deprived of its ammonia by the filter. This is also the case with air artificially charged with ammonia *to a large extent*. This absorption is not due to the presence of hygroscopic moisture, since cotton-wool, when absolutely dry, is capable of taking up 115 *times its own bulk* of dry ammonia (confined over mercury) at 10°·5 C. and 755·7 millims. barom., the gas being again slowly evolved when the wool is left in contact with the air at 100° C.

All other porous substances that were tried for filtering agents were found to possess this property more or less; even freshly ignited pumice-stone is not entirely without absorptive effect upon the gas.

March 24, 1870.

Lieut.-General Sir EDWARD SABINE, K.C.B., President, in the Chair.

The following communication was read :—

"On the *Madreporaria* dredged up in the Expedition of H.M.S. 'Porcupine.'" By P. Martin Duncan, M.B. Lond., F.R.S., Sec. Geol. Soc., Professor of Geology in King's College, London. Received February 26, 1870.

Professor Wyville Thomson, Dr. Carpenter, and Mr. Gwyn Jeffreys have placed the collection of stony corals dredged up by them in the 'Porcupine' Expedition in my hands for determination. They have kindly afforded me all the information I required concerning the localities, depths, and temperatures in which the specimens were found.

My report has been rendered rather more elaborate than I had intended, in consequence of the great consideration of Professor A. Agassiz and Count de Pourtales in forwarding me their reports* and specimens relating to the deep sea-dredging off Florida and the Havana.

They have enabled me to offer a comparison between the British and American species, which I had not hoped to do before the publication of this communication.

Contents.

I. List of the species, localities, depths, temperatures.
II. Critical notice of the species.
III. Special and general conclusions.

I. Twelve species of Madreporaria were dredged up, and the majority came from midway between Cape Wrath and the Faroe Islands. Others were also found off the west coast of Ireland. Many varieties of the species were also obtained, and some forms which hitherto have been considered specifically distinct from others, but which now cease to be so†. [See Table, p. 290.]

List of species known only on the area dredged, or in the neighbouring seas.

1. Amphihelia atlantica, *nobis*.
2. —— ornata, *nobis*.
3. Allopora oculina, *Ehrenberg*.

* Contributions to the Fauna of the Gulf-stream at great depths, by L. F. de Pourtales, 1st & 2nd series, 1868. Bull. Mus. Comp. Zool. Harvard College, Cambridge, Mass., Nos. 6 & 7.

† One specimen came from the 'Lightning' Expedition. It must be remembered that all th deep-sea corals known to British naturalists were not dredged up. The *Stylaster rosea*, for instance, was not amongst the collection.

List of Madreporaria.

Name.	No.	Latitude.	Longitude.	Depth.	Temperature.	Remarks.
		° ′	° ′	fathoms.	°	
1. Caryophyllia borealis, *Fleming*	2	51 57 N.	10 23 W.	30–40	52·00	Specimens very numerous.
Syn. C. clavus	...	......	......	......	......	The species is found in the coralliferous British seas and
C. Smithii	88	59 26 N.	8 23 W.	705	42·65	Mediterranean. Fossils in Miocene and Pliocene of Sicily.
C. cyathus and many varieties						At great depths in Mediterranean (recent).
2. Ceratocyathus ornatus, *Seguenza*	88	......	......	705	42·65	One specimen. Not known elsewhere; the genus is, with this exception, extinct. The species is fossil in the Sicilian Miocene.
3. Flabellum laciniatum, *Ed. & H.*	3	51 51 N.	11 50 W.	370	......	Specimens numerous.
Syn. Ulocyathus arcticus, *Sars* ...	25	56 41 N.	13 39 W.	164	46·5	This is a well-known Norwegian recent form.
4. Lophohelia prolifera, *Pallas*, sp.	5	52 4 N.	12 8 W.	364	48·8	Specimens numerous.
Syn. All the species hitherto published, viz.:—						The variability of this species at different depths is so great that all the known species must in consequence be con-
L. anthophyllites, *Ed. & H.* ..	13	53 42 N.	13 55 W.	208	49·6	sidered varieties of one form. Recent in Norwegian seas,
L. subcostata, *Ed. & H.*	14	53 49 N.	13 15 W.	173	49·6	Mediterranean, and off the Shetlands. Fossil in Miocene
L. affinis, *Pourtales*	15	54 5 N.	12 7 W.	422	47·0	and Pliocene deposits of Sicily. A variety is found off the
L. Defrancei, *Defrance*	25	56 41 N.	13 39 W.	164	46·5	American coast. A considerable number of specimens was
L. gracilis, *Seguenza*	54	59 56 N.	6 27 W.	363	31·5	found in the "cold area" at depths from 500–600 fathoms.
and several varieties.						
5. Amphihelia profunda, *Pourtales*, sp.	54	59 56 N.	6 27 W.	363	31·5	Many specimens.
6. —— oculata, *Linnæus*, sp.	54	......	......	......	......	The necessity for absorbing *Diplohelia* is stated in the following pages.
7. —— miocenica, *Seguenza*	54	......	......	......	......	The species of *Amphihelia* range from the Miocene to the
8. —— atlantica, *nobis*	54					present day; but only *A. oculata* has hitherto been found
9. —— ornata, *nobis*	54					in recent fauna.
10. Allopora oculina, *Ehrenberg*	54	......	......	......	......	A few specimens.
		59 40 N.	7 10 W.	530	47·0	Dredged in 'Lightning' Expedition. A recent form.
11. Balanophyllia (Thecopsammia) socialis, *Pourtales*, sp.	54	59 56 N.	6 27 W.	363	31·5	These are West-Indian forms, and are included in *Thecopsammia*, a subgenus, by Pourtales.
var. costata						
britannica						
Jeffreysia	65	61 10 N.	2 21 W.	345	29·9	
12. Pliobothrus symmetricus, *Pourtales*	...	......	......	500–600	Cold area.	It is a West-Indian form.

Total species, 12; species absorbed, 9. Good varieties numerous. Greatest depth from which species were dredged, 705 fathoms. Lowest temperature of sea at bottom whence corals were dredged, 29°·9.

List of species common to the area and to the Florida and Havana deep-sea faunas only.

1. Balanophyllia socialis, *Pourtales*, sp.
2. Amphihelia profunda, *Pourtales*, sp.
3. Pliobothrus symmetricus, *Pourtales*, sp.

These forms are not known in the West-Indian Cainozoic fauna, and they have not been discovered in any European deposits.

Lophohelia prolifera (var. *affinis*) is common to the British and Florida deep-sea faunas; it is found fossil in the Sicilian Tertiaries, being moreover a member of the recent fauna of the Mediterranean.

List of species common to the area and to the Mediterranean Sea.

1. Caryophyllia borealis, *Fleming*.
2. Amphihelia oculata, *Linnæus*, sp.
3. Lophohelia prolifera, *Pallas*, sp.

List of species found on the area dredged, and as fossils elsewhere.

1. Caryophyllia borealis, *Fleming*. Sicilian: Miocene and Pliocene.
2. Ceratocyathus ornatus, *Seguenza*. Sicilian: Miocene and Pliocene.
3. Flabellum laciniatum, *Ed. & H.* Sicilian, Calabrian: Miocene and Pliocene.
4. Lophohelia prolifera, *Pallas*, sp. Sicilian: Miocene and Pliocene.
5. Amphihelia miocenica, *Seguenza*. Sicilian: Miocene and Pliocene.

The deep-sea coral-fauna of the area dredged in the 'Porcupine' and 'Lightning' Expeditions is therefore composed of:—

5 species which have lasted since the early Cainozoic period.
1 Mediterranean species not known in Cainozoic deposits.
3 species of the deep-sea fauna of Florida and Havana.
3 indigenous species.
—
12

Two of the fossil species are represented in the recent fauna of the Mediterranean.

If the species which I have absorbed into others (in consequence of the light thrown upon the amount of variation in the deep-sea corals) were counted, the fossil forms would be in all 8.

The greatest depth from which *Madreporaria* were dredged was 705 fathoms, and the lowest temperature of the water in which they lived was 29°·9.

II. *Caryophyllia borealis*, Fleming.—Having collected a very considerable series of the *Caryophylliæ* from the seas around Great Britain, and having been supplied with several specimens of the Mediterranean species, I had some time ago compared the whole with the fossil forms from the Sicilian

tertiary deposits and with each other. The numerous specimens of *Caryophylliæ* dredged up in Dingle Bay were especially interesting after I had arrived at satisfactory conclusions respecting the affinities of the above-mentioned British and Southern-European forms. The Dingle-Bay collection presented all the varieties of shapes (some of which had been deemed of specific value) which I had observed in the separate assemblages of specimens from the Mediterranean, the Sicilian tertiaries, and the British and Scottish seas.

A perfect series of specimens from all these localities can be so arranged as to show a gradual structural transition from form to form; so that the most diversely shaped *Caryophylliæ* can be linked together by intermediate shapes. The *Caryophyllia clavus* and *Caryophyllia cyathus* can be united by intermediate forms, and all of these to *Caryophyllia Smithii* and *Caryophyllia borealis*.

It is impossible to determine which is the oldest form; but they all appear to be reproduced by variation on some part of the area tenanted by the section of the genus. The variability of the *Caryophylliæ* of the Sicilian tertiary deposits is very marked; and it is equally so in the groups which live on disconnected spots in our waters. The Dingle-Bay series presents the greatest amount of variability, and indeed is most instructive; for by applying the range of it to the classification of such genera as *Trochocyathus* and *Montlivaltia* a great absorption of species must ensue.

The Dingle-Bay *Caryophylliæ* are evidently the descendants of those which lived in the Western and Southern-European seas before those great terrestrial elevations took place which were connected with the corresponding subsidence of the circumpolar land and the subsequent emigration of Arctic mollusca. They are not closely allied to the recent West-Indian species; but they occupy a position in the Coral-fauna representative of them. The same remark holds good with reference to the affinities of the recent and the cretaceous *Caryophylliæ*. They are not closely allied, and they belong to different sections of the genus; but they hold the same positions in the economy of the old and new distribution of animal life, and the recent forms are representative of the older. The examination of the Dingle-Bay *Caryophylliæ* tends to prove that a species is really the sum of the variations of a series of forms.

A specimen was dredged up in 705 fathoms, temp. 42°·65 F., and it exactly resembles forms which are frequently found in 90 fathoms, and at a temperature slightly below that of the surface. M. Alphonse Milne-Edwards obtained some *Caryophylliæ* from the cable between Corsica and Algiers in 1110–1550 fathoms. The bathymetrical range of these forms is therefore very great. I have placed the species *borealis* in the first place, and regard the old species *C. clavus*, *C. Smithii*, and *C. cyathus* as varieties of it.

Ceratocyathus ornatus, Seguenza.—A beautiful specimen of this rare form was dredged up from a depth of 705 fathoms with some *Caryophylliæ*

and a small *Isis*. The species is hitherto unknown except in the Sicilian miocene*.

Flabellum laciniatum, Ed. & H.—This is the *Ulocyathus arcticus* of the late Prof. Sars. Many specimens were dredged up; but most of them were broken, in consequence of the extreme fragility and delicacy of the theca. There are no pali; therefore Sars's terminology is not in accordance with the received system. The form was familiar to me from Seguenza's drawing of a dilapidated *Flabellum* (which is always found broken*); and it is now evident that *Ulocyathus* must give place to *Flabellum*. The species links *Flabellum* to *Desmophyllum*: it is not known in the recent Mediterranean fauna.

Lophohelia prolifera, Pallas, sp., is apparently a common coral in the north-western British seas.

					Temperature.
It was dredged up in No.	5	at a depth of	364	fathoms ..	48°·8
	13	”	208	” ..	49·6
	14	”	173	” ..	49·6
	15	”	422	” ..	47·0
	25	”	164	” ..	46·5
	54	”	363	” ..	31·5

and also at a depth of from 350 to 600 fathoms in the cold area to the north-west.

All the specimens show great density of the calcareous skeleton; and active nutrition may be inferred on account of the repeated gemmation, the large size of the calices, and the numerical development of the septa. Great variability occurs in the corallites forming a stem; and the shape of the calices is very diverse.

It is very interesting to find some specimens bearing elongate and more or less claviform corallites with the peculiar gemmation of *Lophohelia anthophyllites*, Ellis and Solander, on some portions of their stem, and the usual-shaped corallites of *Lophohelia prolifera* on others.

A separate corallum, which must be referred to *Lophohelia anthophyllites*, Ellis and Solander, was dredged up at No. 54.

The variation of the gemmules of several specimens is sufficiently great to absorb *Lophohelia subcostata*, Ed. & Haime; for fragments of the corallum of *Lophohelia prolifera* exist which possess all its so-called specific peculiarities.

A careful examination of *Lophohelia Defrancei*, Defrance, sp., from the Messinese Pliocene and Miocene deposits, and a comparison of its structure with the numerous specimens dredged up in the ‘Porcupine’ Expedition, lead me to believe that it is identical with *Lophohelia prolifera*.

* Seguenza, “Disquisiz. Paleont. int. ai Corall. Foss.,” Mem. della Reale Accad. dell. Sci. Torino, serie ii. tomo xxi. 1864.

The same identity must be asserted for *Lophohelia affinis*, Pourtales, which was dredged up in 195 fathoms off Coffin's Patches, Florida.

Lophohelia prolifera exists in the Mediterranean Sea and the sea between Scotland and Norway.

Lophohelia anthophyllites is an East-Indian form; but its absorption into *Lophohelia prolifera* suggests explanations concerning the Cainozoic progenitor, and how it migrated eastwards.

The relation of the recent East-Indian Coral-faunas to those of the European and West-Indian Cainozoic deposits has been noticed and admitted for some years past.

The Cainozoic *Lophohelia* of Sicily is the earliest form of the genus; and those which are found in such remote parts of the world as the East Indies, the Florida coast, the Norwegian coast, and the Mediterranean, and which have been determined to belong to different species, are, from the study of the curious assemblage of variable forms now under consideration, evidently varieties of the old type, *Lophohelia prolifera*. I have therefore absorbed the old species *L. anthophyllites*, *L. subcostata*, *L. affinis*, *L. Defrancei*, and *L. gracilis*.

Two genera of the *Oculinidæ* in the classification of MM. Milne-Edwards and Jules Haime have always been most difficult to distinguish; and now the results of the dredging off the north of Scotland and off Florida and the Havana necessitate the absorption of one of them.

Amphihelia and *Diplohelia*.—The first containing recent species only at the time of the enunciation of the classification just referred to, and the last having fossil species only, were very likely to be considered separate genera. *Diplohelia* had species in the Eocene and in the Cainozoic seas. *Amphihelia* was known to have species in the Mediterranean fauna, and in that of Australia also. Seguenza, however, described some *Amphihelia* and *Diplohelia* from the Sicilian tertiary deposits which were identical so far as generic attributes are considered, the only distinction being a doubtful raggedness of the septal edges. The habit and the method of growth and gemmation of the forms were the same. M. de Pourtales dredged up a branching form from off the Havana in 350 fathoms, and from off Bahia Honde, near Florida, in 324 fathoms, and also in lat. 28° 24′ N., long. 79° 13′ W., in 1050 fathoms (came up with the lead). This he named *Diplohelia profunda*. On referring to Seguenza's plates and descriptions* of the fossil corals from the Sicilian Tertiary deposits, there is no difficulty in deciding upon the very close affinity of the species described by Pourtales and *Diplohelia Meneghiniana*, Seg., and *Diplohelia Doderleiniana*, Seg., fossil forms from the mid-tertiary deposits.

But on comparing these forms with one exquisitely figured by Seguenza, and which he calls *Amphihelia miocenica*, Seg., the generic affinities of all become startlingly evident (tab. xii. figs. 1*b*, 1*c*, 3*b* & 3*c*, *op. cit.*).

The very numerous specimens of small branching *Oculinidæ* which

* Seguenza, *l. c.*

were dredged up in the 'Porcupine' Expedition (No. 54, and to the north-west of that spot in the cold area), at a depth of from 363 to 600 fathoms, present singular variations of structure in the buds and calices upon the same stems. A comparison between them and the well-known recent and fossil *Amphiheliæ*, the fossil and recent *Diploheliæ*, and the smaller specimens of *Lophoheliæ*, leads to the belief that *Amphihelia* is identical generically with *Diplohelia*, and very closely allied to *Lophohelia*. Indeed the distinction between the *Lophoheliæ* and *Amphiheliæ* is of the slightest kind.

The species of the genus *Amphihelia* dredged up in the 'Porcupine' Expedition are five:—

1. Amphihelia (Diplohelia) profunda, *Pourtales*, sp.
2. —— oculata, *Linnæus*, sp.
3. —— miocenica, *Seguenza*.
4. —— atlantica, *nobis*.
5. —— ornata, *nobis*.

The species came from No. 54 dredging, and from the cold area to the north-west in from 500 to 600 fathoms.

The specimens are exceedingly beautiful, strong, and perfect; and there was much difficulty experienced in removing the polypes from the calices.

1. *Amphihelia profunda*, Pourtales, sp., has been noticed. It is a West-Indian form closely allied to a Sicilian miocene species.

2. *Amphihelia oculata*, Linnæus, sp., is well known in the Mediterranean, and has not hitherto been found in the Atlantic.

3. *Amphihelia miocenica*, Seguenza, is a very common species in the deep sea, but is rare in the miocene deposits of Sicily. Its fully developed costal structures distinguish it from the other forms.

4. *Amphihelia atlantica*, nobis, is a new species, large, bushy, and with almost plain cœnenchyma, which is very abundant.

5. *Amphihelia ornata*, nobis, is a new species closely allied to the miocene form, but its ornamentation is most peculiar, and not continuously costulate.

Allopora oculina, Ehrenberg.—Several specimens of this very rare coral were dredged up in No. 54, and one in the 'Lightning' Expedition, not far from the same spot.

The type is in the Berlin Museum; the locality whence it came is unknown.

The distinction between these massive and densely hard corals (whose calices are principally on one side of the cœnenchyma of the stem) and the *Stylasters* is very evident.

M. de Pourtales has described a pretty red-coloured *Allopora miniata* dredged in 100 to 324 fathoms off the Florida reef; but it is very distinct from the species discovered in the late deep-sea dredging expeditions.

Allopora has no fossil representatives.

Balanophyllia (*Thecopsammia*) *socialis*, Pourtales.—Six specimens of a

simple perforate coral were dredged up in lat. 59° 56′ N., long. 6° 27′ W., 363 fathoms, temperature 31°·8 (No. 54), and one in lat. 61° 10′ N., long. 2° 21′ W., 345 fathoms, temp. 29°·9 (No. 65).

The six specimens are of different sizes and ages; and although they present considerable variation in shape and septal development, they evidently belong to one type. The solitary coral from No. 65 is larger than the others, but it belongs to the same species.

Notwithstanding the temperature in which the corals were found, and the depth of the sea, they are strong and well-developed forms, evidencing an active and abundant nutrition.

There is no difficulty in classifying the specimens with the *Thecopsammiæ* of Pourtales.

Thecopsammia socialis, Pourtales, was dredged up in from 100 to 300 fathoms, off Sombrero, near Florida, in the course of the Gulf-stream.

I have been able to compare the specimens dredged up in the 'Porcupine' Expedition with M. Pourtales's types, and, after making due allowance for variation, I have no doubt about including the British forms under his specific term. These varieties of the Floridan type, found at greater depths, and doubtless in much colder water, present evidences of greater vigour than the American forms. They are larger and denser, and their septa are better developed. Moreover some of them, although they possess all the other characteristics of the genus as diagnosed by Pourtales, present indubitable costæ, especially inferiorly. This clinging to the Balanophyllian type is not witnessed in the Floridan forms; but it is too important to be passed over, especially as it renders the generic distinction between many well-known *Balanophylliæ* and the new *Thecopsammiæ* very unstable. The *Thecopsammiæ*, from the peculiarities of their wall, epitheca, and septa, well merit the distinction of a subgenus; and therefore I propose to restore the species associated under the term to the genus *Balanophyllia*, in the subgenus *Thecopsammia*.

Balanophyllia (*Thecopsammia*) *socialis*, Pourtales, var. *costata*. No. 54, 'Porcupine' Expedition.

—— (——) ——, var. *britannica*. No. 54, 'Porcupine' Expedition.

—— (——) ——, var. *Jeffreysia*. No. 65.

All these varieties refer to specimens which were fixed by their bases to stones.

The varieties and the original types are very isolated forms in the great genus *Balanophyllia*. They have only a very remote affinity with the West-Indian recent *Balanophylliæ*, with those of the Crag, the Faluns, and the Eastern Tertiaries.

The British forms appear to have emigrated from the south-west; and probably the original type wandered through the agency of the Gulf-stream, which carried the ova and deposited them in our northern sea, where they have propagated, varied, and thriven.

Pliobothrus symmetricus, Pourtales.—A specimen of this doubtful coral (which had been described by M. de Pourtales from the results of dredging in from 100 to 200 fathoms) was sent to me by Dr. Carpenter. It came from the cold area, in from 500 to 600 fathoms.

There is no doubt that this very polyzoic-looking mass belongs to the American type. The tabulæ are hardly worthy to be called such; and I place the form amongst the Zoantharia provisionally.

III. The species of *Madreporaria* belong to genera which do not contribute and have not contributed to form coral-reef faunas. None of them are reef-builders; but all are essentially formed to live where rapid growth and delicately cellular structures are not required. The forms are strong, solid, and large; and their rapid and repeated gemmation proves that their nutritive processes went on actively and continuously.

All the species are very much disposed to produce variations; and this is especially true as regards those which have outlived the long age of the Crag, the glacial period, and the subsequent time of elevations and subsidences. The least-variable species are those which are not known on other areas.

Two of the three species which are common to the West-Indian deep-sea fauna and that of our north-western coasts are also very variable.

The persistence of *Madreporaria* from the earlier Cainozoic period to the present time has been an established fact for several years. Some of the forms which are common to the deep sea of the British area and to the so-called miocene of Sicily are still existing in the Mediterranean. None, however, of the species of Corals found in the British Crag are represented in the deep-sea fauna.

The existence of Mediterranean forms in the North-west British area is in keeping with the discoveries of Forbes. It has, however, a double significance, and bears upon the presence of West-Indian forms on the North-west British marine area. There was a community of species between the Mediterranean and the West Indies in the Cainozoic period, especially of Echinodermata, Mollusca, Madreporaria, and Foraminifera. After the great alterations of the mutual relations of land and sea which took place before the cold affected the fauna of the Franco-Italian seas, this community of species diminished; but it lasted through all the period of Northern glacialization, and is proved still to exist slightly by comparing the Algæ, the Corals, the Echinodermata, and the Mollusca.

The presence of two very characteristic Floridan species, and one less so, off the north of Scotland, is particularly interesting, because they all live in the cold area and flourish there, whilst they appear to be less vigorous in the warmer Gulf-stream near Florida.

It is impossible to fail to recognize the operation of this stream in producing the emigration of these three species, which are essentially American.

The solidity and the power of gemmation of the corals within the cold area

appear to be greater than elsewhere. Depth has not much effect upon the nutrition of the *Madreporaria*; for those dredged up at 600 fathoms are quite as hard and solid as those found at 300 fathoms.

All the calices were stuffed with small Foraminifera, and there was evidently a great abundance of food.

There were numerous Polyzoa, Sponges, Foraminifera, Diatomaceæ, and delicate bivalves associated with or fixed upon the corals at all depths. Moreover, at from 300 to 400 fathoms, some *Amphihelia* had incrusted an Annelid.

Serpulæ, moreover, abound upon the corals; and a pretty *Isis* was associated with them at a depth of 705 fathoms. This is a fauna which, if covered up and presented to the palæontologist, would be, and would have been for some years past, considered a deep-sea one.

It is a fauna which indicates the existence of the same processes of nutrition and of destructive assimilation and reproduction which are recognized in association with corresponding forms at less depths and in higher temperatures.

The great lesson which it reads is, that vital processes can go on in certain animals at prodigious depths, and in much cold, quite as well as in less depths and in considerable heat. It suggests that a great number of the Invertebrata are not much affected by temperature, and that the supply of food is the most important matter in their economy.

The researches of Hooker, who obtained Polyzoa and Foraminifera in soundings at a depth of nearly 400 fathoms off the icy barrier of the South Pacific, of Wallich in the Atlantic, and of Alphonse Milne-Edwards in the Mediterranean have had much influence upon geological thought in this age, which, so far as geologists are concerned, is remarkably averse to theory. For many years before any very deep soundings had been taken with the view of searching the sea-bottom for life, geologists had more or less definite opinions concerning the deposition of organisms in sediments at great depths. Certainly more than thirty years ago deep-sea deposits were separated by geologists from those which they considered to have been formed in shallower seas. The finely divided sediment of strata containing Crinoids, Brachiopods, Foraminifera, and simple Madreporaria was supposed to have been deposited in deeper water than formations containing large pebbles, stones, and the mollusca whose representatives now live in shallows. The relations of such strata to each other during subsidence, the first being found occasionally to overlap the last, proved that there was a deeper sea-fauna in the offing of the old shores which were tenanted by littoral and shallow-water species. The deposition of strata containing Foraminifera, Madreporaria, and Echinodermata, whose limestone is remarkably free from any foreign substances, has been considered to have taken place in very deep water; this theory has been founded upon the observations of the naturalist and mineralogist. Indeed no geologist has hesitated in assigning a great depth to the origin of some

deposits in the Laurentian, Silurian, or in any other formation. The "flysch," a great sediment of the Eocene formation, has been considered to have been formed at a great depth and under great pressure. Its singularly unfossiliferous character was supposed to be due to the absence of life at the depths of the ocean where the sediment collected. But this was a theory of the early days of geology, when the destructive influence of chemical processes in strata upon the remains of organisms in them was hardly admitted.

The great value of such researches as those so ably carried out by Thomson, Carpenter, and Jeffreys is the definite knowledge they impart to the geologist, who is theorizing in the right direction, but whose notions of the depth at which the sediments containing Invertebrata can be deposited are indefinite. These researches contribute to more exact knowledge, and they will materially assist the development of those hypotheses which are current amongst advanced geologists into fixed theories. I do not think that any geological theory worthy of the term, and which has originated from geological induction, will be upset by these careful investigations into the bathymetrical distribution of life and temperature. The theories involving pressure and the intensity of the hardness of deep-sea deposits will suffer from the researches; but many difficulties in the way of the palæontologist will be removed. The researches tend to explain the occurrence of a magnificent deep-sea coral-fauna in the Palæozoic times in high latitudes, and of Jurassic and Cainozoic faunas on the same area, and they favour the doctrines of uniformity. They explain the cosmopolitan nature of many organisms, past and present, which were credited with a deep-sea habitat, and they afford the foundations for a theory upon the world-wide distribution of many forms during every geological formation.

It is not advisable, however, to make too much of the interesting identities and resemblances of some of the deep-sea and abyssal forms with those of such periods as the Cretaceous, for instance. In the early days of geological science there was a favourite theory that at the expiration of a period the whole of the life of the globe was destroyed, and that at the commencement of the succeeding age a new creation took place. There were as many destructions and creations as periods; or, to use the words of an American geologist, there was a succession of platforms. This theory held back the science, just as the theory that the sun revolved round the earth retarded the progress of astronomy. Moreover it had that armour of sanctity to protect it which is so hard to pierce by the most reasonable opposition. Nevertheless every now and then a geologist recognized the same fossils in rocks which belonged to different periods. A magnificent essay by Edward Forbes on the Cretaceous Fossils of Southern India, a wonderful production and far before its age *, gave hope and confidence to the few palæontologists who began to assert that

* Quart. Journ. Geol. Soc. vol. i. p. 79.

periods were perfectly artificial notions—that it did not follow, because one set of deposits was forming in one part of the world, others exactly corresponding to it elsewhere, so far as the organic remains are concerned, were contemporaneous—and that life had progressed on the globe continuously and without a break from the dawn of it to the present time.

The persistence of some species through great vertical ranges of strata, and the relation between the world-wide distribution of forms and this persistence were noticed by D'Archiac, De Verneuil, Forbes, and others. The identity of some species in the remote natural-history provinces of the existing state of things was established in spite of the dogmatic opposition of authorities; and then geologists accepted the theories that there were several natural-history provinces during every artificial period, that some species lived longer and wandered more than others, and that some have lasted even from the Palæozoic age to the present.

Persistence of type was the title of a lecture delivered by Professor Huxley* many years ago; and this persistence has been admitted by every palæontologist who has had the opportunity of examining large series of fossils from every formation from all parts of the world.

Geological ages are characterized by a number of organisms which are not found in others, and by the grouping of numerous species which are allied to those of preceding and succeeding times, but which are not identical. Certain portions of the world's surface were tenanted by particular groups of forms during every geological age; and there was a similarity of arrangement in this grouping under the same external physical conditions. To use Huxley's term, the "homotaxis" of certain natural-history provinces during the successive geological ages has been very exact. The species differed; but there was a philosophy in the consecutive arrangements of high-land and low-land faunas and floras, and of those of shallow seas, deep seas, oceans, and reef-areas. The oceanic † conditions, for instance, can be traced by organic remains from the Laurentian to the present time, and the deep-sea corals now under consideration are representative of those of older deep seas.

It is not a matter for surprise, then, that, there being such a thing as persistence of type and of species, some very old forms should have lived on through the ages whilst their surroundings were changed over and over again. But this persistence does not indicate that there have not been sufficient physical and biological changes during its lasting to alter the face of all things enough to give geologists the right of asserting the succession of several periods. The occurrence of early Cainozoic Madreporaria in the deep sea to the north-west of Great Britain only proves that certain forms of life have persisted during the vast changes in the physical geography of the world which were initiated by the upheaval of the Alps, the Himalayas, and large masses of the Andes. To say that we are therefore

* Royal Institution. See also Pres. Address, Geol. Soc., 1870.

† P. M. Duncan, Quart. Journ. Geol. Soc. No. 101.

still in the Cainozoic or Cretaceous age would hardly be consistent with the necessary terminology of geological science.

During the end of the Miocene age and the whole of the Pliocene the Sicilian area was occupied by a deep sea. The distinction between the faunas of those times and the present becomes less, year after year, as science progresses; and it is evident that a great number of existing species of nearly every class flourished before the occurrence of the great changes in physical geology which have become the artificial breaks of tertiary geologists. That the Cainozoic deep-sea corals should resemble, and in some instances should be identical in species with, the forms now inhabiting vast depths, is therefore quite in accordance with the philosophy of modern geology. Before the deposition of the Cainozoic strata, and whilst the deep-sea deposits of the Eocene age were collecting in the Franco-British area, there was a Madreporarian fauna there which was singularly like unto that which followed it, both as regards the shape of the forms and their genera. Still earlier, during the slow subsidence of the great Upper Cretaceous deep-sea area, there was a coral-fauna in the north and west of Europe, of which the existing is very representative. The simple forms predominate in both faunas. *Caryophyllia* is a dominant genus in either; and a branching *Synhelia* of the old fauna is replaced in the present state of things by a branching *Lophohelia*. The similarity of deep-sea coral-faunas might be carried still further back in the world's history; but it must be enough for my purpose to assert the representative character and the homotaxis of the Upper Cretaceous, the Tertiary, and the existing deep-sea coral-faunas. This character is enhanced by the persistence of types; but still the representative faunas are separable by vast intervals of time.

March 31, 1870.

Lieut.-General Sir EDWARD SABINE, K.C.B., President, in the Chair.

The following communications were read:—

I. "On the Relation between the Sun's Altitude and the Chemical Intensity of Total Daylight in a Cloudless Sky." By HENRY E. ROSCOE, F.R.S., and T. E. THORPE, Ph.D. Received March 3, 1870.

(Abstract.)

In this communication the authors give the results of a series of determinations of the chemical intensity of total daylight made in the autumn of 1867 on the flat tableland on the southern side of the Tagus, about 8½ miles to the south-east of Lisbon, under a cloudless sky, with the object of ascertaining the relation existing between the solar altitude and the

chemical intensity. The method of measurement adopted was that described in a previous communication to the Society*, founded upon the exact estimation of the tint which standard sensitive paper assumes when exposed for a given time to the action of daylight. The experiments were made as follows:—

1. The chemical action of total daylight was observed in the ordinary manner.

2. The chemical action of the diffused daylight was then observed by throwing on to the exposed paper the shadow of a small blackened brass ball, placed at such a distance that its apparent diameter, seen from the position of the paper, was slightly larger than that of the sun's disk.

3. Observation No. 1 was repeated.

4. Observation No. 2 was repeated.

The means of observations 1 and 3 and of 2 and 4 were then taken. The sun's altitude was determined by a sextant and artificial horizon, immediately before and immediately after the observations of chemical intensity, the altitude at the time of observation being ascertained by interpolation.

It was first shown that an accidental variation in the position of the brass ball within limits of distance from the paper, varying from 140 millims. to 230 millims., was without any appreciable effect on the results. One of the 134 sets of observations was made as nearly as possible every hour, and they thus naturally fall into seven groups, viz.:—

(1) Six hours from noon, (2) five hours from noon, (3) four hours from noon, (4) three hours from noon, (5) two hours from noon, (6) one hour from noon, (7) noon.

Each of the first six of these groups contains two separate sets of observations,—(1) those made before noon, (2) those made after noon. It has already been pointed out†, from experiments made at Kew, that the mean chemical intensity of total daylight for hours equidistant from noon is the same. The results of the present series of experiments prove that this conclusion holds good generally; and a Table is given showing the close approximation of the numbers obtained at hours equidistant from noon.

Curves are given showing the daily march of chemical intensity at Lisbon in August, compared with that at Kew for the preceding August, and at Pará for the preceding April. The value of the mean chemical intensity at Kew is represented by the number 94·5, that at Lisbon by 110, and that at Pará by 313·3, light of the intensity 1 acting for 24 hours being taken as 1000.

The following Table gives the results of the observations arranged according to the sun's altitude.

* Roscoe, Bakerian Lecture, 1865.

† Phil. Trans. 1867, p. 558.

of observations.	Mean altitude.	Chemical Intensity. Sun.	Sky.	Total.
15	9° 51′	0·000	0·038	0·038
18	19 41	0·023	0·063	0·085
22	31 14	0·052	0·100	0·152
22	42 13	0·100	0·115	0·215
19	53 09	0·136	0·126	0·262
24	61 08	0·195	0·132	0·327
11	64 14	0·221	0·138	0·359

rves are given showing the relation between the direct sunlight (column and diffuse daylight (column 4) in terms of the altitude. The curve of ect sunlight cuts the base line at 10°, showing that the conclusion formerly ived at by one of the authors is correct, and that at altitudes below 10° direct sunlight is robbed of almost all its chemically active rays. The ation between the total chemical intensity and the solar altitude is shown be represented graphically by a straight line for altitudes above 10°, the ition of the experimentally determined points lying closely on to the aight line.

A similar relation has already* been shown to exist (by a far less complete ies of experiments than the present) for Kew, Heidelberg, and Pará; that although the chemical intensity for the same altitude at different ces and at different times of the year varies according to the varying nsparency of the atmosphere, yet the relation at the same place between tude and intensity is always represented by a straight line. This varia- n in the direction of the straight line is due to the opalescence of the nosphere; and the authors show that, for equal altitudes, the higher inten- is always found where the mean temperature of the air is greater, as in nmer, when observations at the same place at different seasons are com- ed, or as the equator is approached, when the actions at different places examined. The differences in the observed actions for equal altitudes, ich may amount to more than 100 per cent. at different places, and to rly as much at the same place at different times of the year, serve as ect measurements of the transparency of the atmosphere.

The authors conclude by calling attention to the close agreement between curve of daily intensity obtained by the above-mentioned method at bon, and that calculated for Naples by a totally different method.

* Phil. Trans. 1867, p. 555.

II. "On the Acids contained in Crab-oil." By WILLIAM J. WONFOR, Student in the Laboratory of the Government School of Science, Dublin. Communicated by Dr. MAXWELL SIMPSON. Received March 7, 1870.

Crab-oil is obtained from the nuts of a tree named by botanists *Hylocarpus carapa*, and also *Carapa Guianensis*. The tree grows abundantly in the forests of British Guiana; the oil is prepared by the Indians, who bring it to George Town for sale. The oil is obtained from the kernels by boiling them for some time, and then placing them in heaps and leaving them for some days; they are then skinned, and afterwards triturated in wooden mortars until reduced to a paste, which is spread on inclined boards and exposed to the sun; the oil is thus melted out, and trickles into receiving-vessels.

As no investigation, so far as I have been able to ascertain, has ever been made of the acids contained in this oil, Professor Galloway, to whom I am indebted for the samples of the oil, recommended me to examine them; and the examination was conducted under his direction.

The oil was in the state in which it is sold by the Indians; it possessed the appearance of a semifluid butyraceous mass, evolving a peculiar penetrating odour; its melting-point was 55° C. To obtain the acids, the oil was saponified with a solution of potassic hydrate, and the soap thus obtained dissolved in a large quantity of distilled water; to the solution sodic chloride was added in considerable excess; the soap which separated was washed and afterwards dissolved, and the solution treated with hydrochloric acid; the liberated fatty acids were collected and pressed, then melted in boiling water, and frequently washed to remove all traces of sodic chloride; the acids were again saponified, and again treated with sodic chloride, but the soda-soap was on this occasion decomposed with tartaric acid. The mixed acids had a melting-point of 40° C.

The acids were dissolved in boiling alcohol of 89 per cent.; the solution, on cooling, deposited a white radiated crystalline mass, which was repeatedly recrystallized from alcohol until it acquired a constant melting-point; it was then saponified with a solution of potassic carbonate, and the solution of the mixed potash salts was evaporated to dryness on the water-bath; the fat salt was then dissolved in absolute alcohol. The alcoholic solution, unless extremely dilute, does not crystallize on cooling, but merely forms a strong jelly, which was, after pressing, dissolved in water, and the fat acid separated by a strong solution of tartaric acid; the separated acid was washed with boiling water until all potassic tartrate and tartaric acid were removed; it was subsequently twice crystallized from absolute alcohol: its melting-point was then found to be 57° C. The acid, when pure, presents the appearance of a white glistening radiated crystalline mass: two combustions were made; the acid employed in the two analyses was obtained from two different saponifications:—

I. ·259 grm. gave ·7115 CO_2 and 295 H_2O=·194 C and ·0327 H.
II. ·1731 grm. gave ·4748 CO_2 and ·195 H_2O=·1295 C and ·02168 H.
Percentage composition :—

	I.	II.	Mean.
Carbon	74·900	74·812	74·856
Hydrogen	12·624	12·516	12·570
Oxygen	12·476	12·672	12·574
	100·000	100·000	100·000

These analyses, it will be seen, agree very closely with the formula for palmitic acid, $C_{16}\,H_{32}\,O_4$.

	At. weight.	Calculated percentage composition.
C_{16}	192	75·00
H_{32}	32	12·50
O_2	32	12·50
	256	100·00

Preparation of the Soda-salt.—The acid was saponified with a dilute solution of sodic carbonate, the jelly-like mass was pressed and dried, and the fat salt dissolved out with absolute alcohol; the alcoholic solution, when cold, gelatinized; the gelatinous mass was pressed, dried, and dissolved in alcohol, and filtered: this salt was not analyzed.

Preparation of the Silver-salt.—The soda-salt was dissolved in hot water and precipitated by argentic nitrate; the precipitate was washed in the dark; the analysis of this salt yielded the following results :—

I. ·2255 grm. gave ·067 grm. Ag.

II. ·5088 grm. gave ·152 grm. Ag.

III. ·6044 grm. gave 1·1572 grm. CO_2 and ·4555 grm. H_2O= ·3156 grm. C and ·05061 H.

IV. ·3267 grm. gave ·634 grm. CO_2 and ·257 grm. H_2O=·1729 C and ·02855 H.

Percentage composition :—

	I.	II.	Mean.
Carbon	52·217	52·923	52·570
Hydrogen	8·373	8·739	8·556
Oxygen	9·698	8·464	9·081
Silver	29·712	29·874	29·793
	100·000	100·000	100·000

The following are the calculated numbers for argentic palmitate :—

	At. weight.	Percentage composition.
C_{16}	192	52·89
H_{31}	31	8·54
O_2	32	8·82
Ag	108	29·75
	363	100·00

Preparation of the Ether.—Dry hydrochloric acid gas was passed to saturation through a warm solution of the acid in absolute alcohol; the solution was then diluted with water, which caused the ether to separate as a yellowish oil, which, as it became cold, assumed the appearance of a waxy body; it was boiled with water, and afterwards agitated with a hot dilute solution of sodic carbonate; it was again dissolved in alcohol, and precipitated from this solution by water; it was then collected and dried; its analysis yielded the following results:—

I. ·2197 grm. gave ·6112 grm. of CO_2 and ·25 grm. H_2O = ·1667 C and ·0278 H.

II. ·204 grm. gave ·567 grm. of CO_2 and ·233 grm. of H_2O = ·15464 C and ·02589 H.

Percentage composition:—

	I.	II.	Mean.
Carbon	75·876	75·803	75·839
Hydrogen	12·643	12·691	12·667
Oxygen	11·481	11·506	11·494
	100·000	100·000	100·000

The following are the calculated numbers for ethylic palmitate—$C_{16}H_{31}(C_2H_5)O_2$:—

	At. weight.	Percentage composition.
C_{18}	216	76·05
H_{36}	36	12·68
O_2	32	11·27
	284	100·00

Preparation of the Baric Salt.—A hot alcoholic solution of the acid was saturated with ammonia in slight excess, and boiled with a solution of baric acetate; the precipitate falls as a white flocculent mass, which, when thoroughly washed, dried, and powdered, has the appearance of a glistening spongy powder.

I. ·276 grm. gave ·0625 grm. of Ba O = 23·64 per cent.
II. ·752 „ ·17906 „ = 23·81 „
Theory requires...... 23·65

I did not consider it necessary to make a carbon and hydrogen determi-

ion of the baric salt, or to examine any other salts of the acid, as the lysis of the acid, the silver-salt, and the ether, along with the determina- of the baryta in the baric salt, I considered sufficiently indicated that acid under examination was palmitic acid, although I could never ain, even after fractional precipitation, a higher melting-point for the l than 57° C.

'he difference in the melting-points of the acid mass before it was ted with alcohol, and the melting-point of the palmitic acid, indicated t at least one other acid was present, but in very minute quantity.

attempted to determine the nature of the acid of lower melting-point exposing the residues obtained from the first three crystallizations of hard acid to cold in a bath of sodic sulphate and hydrochloric acid, all hard acid which crystallized out being rejected; the portion which re- ned fluid was saponified with potassic carbonate, and the solution of the ash soap was subjected to fractional precipitation by means of plumbic ate; the second and smaller precipitate was collected and washed, and ted for some time at a moderate temperature with dilute sulphuric ; this caused the separation of a reddish oily-looking liquid which collected and dissolved in boiling alcohol; it was afterwards saponified potassic carbonate, and the silver-salt prepared from that salt. I obtained sufficient of the silver-salt from about 2 lbs. of oil to make determination of the silver and one of the carbon and hydrogen, and these determinations I did not obtain concordant results, and want of erial compelled me to relinquish the further examination of the acid.

Presents received March 10, 1870.

1sactions.

ordeaux:—Académie Impériale des Sciences, Belles-Lettres et Arts. Actes. 3^e série, 30^e année 1868, 3^{me} trimestre. 8vo. *Paris* 1868. The Academy.

isbon:—Museu Nacional. Secção Zoologica. Catalogo das Collecções Ornithologicas. Junho de 1869. 8vo. *Lisboa* 1869. The Museum.

ewcastle-on-Tyne:—Iron and Steel Institute. Transactions, Session 1869. (Nos. 1–3.) 8vo. *Newcastle-on-Tyne* 1869. The Secretary.

aris:—Ecole des Mines. Annales des Mines. 2, 3, 4 livraisons de 1869. 8vo. *Paris.* The School.

Ecole Normale Supérieure. Annales Scientifiques, par L. Pasteur. Tome VI. Nos. 2–6. 4to. *Paris* 1869. The School.

2 A 2

Transactions (*continued*).

Siena:—R. Accademia de' Fisiocritici. Classe delle Scienze Fisiche. Rivista Scientifica. Anno 1. Fasc. 2–3. 8vo. *Siena* 1869. The Academy.

Turin:—Regio Osservatorio dell' Università. Bollettino Meteorologico ed Astronomico. Anno 4, 1869. 4to. The Observatory.

Zürich:—Meteorologische Centralanstalt der schweizerischen naturforschenden Gesellschaft. Schweizerische Meteorologische Beobachtungen. Sept.–Dec. 1868, Jan.–Feb. 1869. 4to. *Zürich*. The Institution.

Aoust (l'Abbé.) Théorie des Coordonnées Curvilignes quelconques. 3 pts. 4to. *Rome et Milan* 1864–69. Sur les Coordonnées Curvilignes planes quelconques. 4to. *Paris* 1859. Sur l'Analyse des Courbes rapportées à un système quelconque de Coordonnées. 4to. *Paris* 1869. The Author.

Plateau (J.) Recherches expérimentales et théoriques sur les Figures d'Equilibre d'une masse liquide sans pesanteur. Séries 9–11. 4to *Bruxelles* 1868. The Author

Wheatley (H. B.) Round about Piccadilly and Pall Mall; or a Ramble from the Haymarket to Hyde Park. 8vo. *London* 1870. The Author

Wolf (R.) Matériaux divers pour l'histoire des Mathématiques. 4to *Rome* 1869. The Author

March 17, 1870.

Transactions.

Edinburgh:—Geological Society. Transactions. Vol. I. Part 3. 8vo *Edinburgh* 1870. The Society

London:—Meteorological Office. Weather Reports, July 1st to December 31, 1869. fol. *London*. Minutes of the Proceedings of the Meteorological Committee, 1869. fol. *London* 1870. The Office

St. Petersburg:—Académie Impériale des Sciences. Mémoires. Tome XIII. No. 8; Tome XIV. Nos. 1–7. 4to. *St. Pétersbourg* 1869. Bulletin. Tome XIV. Nos. 1, 2. 4to. *St. Pétersbourg* 1869. The Academy

Turin:—R. Accademia delle Scienze. Atti. Vol. IV. Disp. 1–7. 8vo *Torino* 1869. Sunti dei Lavori Scientifici letti e discussi nella Classe di Scienze Morali, Storiche e Filologiche dal 1859 al 1865 scritti da Gasparo Gorresio. 8vo. *Torino* 1868. The Academy

---

Brown (Robert.) On the Geographical distribution and physical charac

teristics of the Coal-fields of the North Pacific Coast. 8vo. *Edinburgh* 1869. The Author.

Ghirardini (A.) Studj sulla Lingua Umana, sopra alcune antiche Inscrizione e sulla Ortografia Italiana. 8vo. *Milano* 1869. The Author.

Lamy (A.) Sur une nouvelle espèce de Thermomètres. 4to. *Paris* 1870. The Author.

Lartet (E.) and Christy (H.) Reliquiæ Aquitanicæ, being contributions to the Archæology and Palæontology of Périgord; edited by T. Rupert Jones. Part 10. 4to. *London* 1870. The Executors of Henry Christy, Esq.

Price (J. E.) A Description of the Roman Tessellated Pavement found in Bucklersbury, with Observations on analogous discoveries. 4to. *Westminster* 1870.
The Library Committee of the Corporation of London.

Smyth (C. Piazzi, F.R.S.) A Poor Man's Photography at the Great Pyramid in the year 1865, compared with that of the Ordnance Survey Establishment subsidized by London Wealth. 8vo. *London* 1870. The Author.

Traill (G. W.) An Elementary Treatise on Quartz and Opal, including their varieties. New edition. 8vo. *Edinburgh* 1870. The Author.

American Ephemeris and Nautical Almanac for the year 1871. 8vo. *Washington* 1869. The Secretary of the United States' Navy.

March 24, 1870.

Transactions.

Berlin:—Physikalische Gesellschaft. Die Fortschritte der Physik im Jahre 1866. XXII. Jahrgang. 8vo. *Berlin* 1869. The Society.

Berliner Astronomisches Jahrbuch für 1872, mit Ephemeriden der Planeten (1)–(108) für 1870; herausgegeben von W. Foerster. 8vo. *Berlin* 1870. The Berlin Observatory.

Calcutta:—Asiatic Society of Bengal. Journal, 1869. Part 1, No. 3; Part 2, No. 4. Proceedings, No. 10. 8vo. *Calcutta* 1869. The Society.

Leeds:—Geological and Polytechnic Society of the West Riding of Yorkshire. Report of the Proceedings, 1869. 8vo. *Leeds* 1870. The Society.

London:—Royal United Service Institution. Journal. Vol. XIII. Nos. 54–56. 8vo. *London* 1869. The Institution.

Transactions (*continued*).

St. Petersburg:—Central Physical Observatory. Repertorium für Meteorologie redigirt von Dr. Heinrich Wild. Band I. Heft 1. 4to. *St. Petersburg* 1869. Vorschläge betreffend die Reorganisation des Meteorologischen Beobachtungs-systemes in Russland. 8vo. *St. Petersburg* 1869. The Observatory.

Bianconi (J. J.) Specimina Zoologica Mosambicana. Fasc. 19 et 20. 4to. *Bononiæ* 1867. Comparazione dell' Organo fossorio della Talpa e della Grillotalpa. 4to. *Bologna* 1869. The Author.

Chimmo (Wm.) Bed of the Atlantic. 4to. *Weymouth* 1870. The Author.

Favre (Alphonse.) De l'Existence de l'Homme à l'époque tertiaire. 8vo. *Lausanne* 1870. H. B. de Saussure et les Alpes. 8vo. *Lausanne* 1870. The Author.

Melde (F.) Experimentaluntersuchungen über Blasenbildung in kreisförmig cylindrischen Röhren. Zweiter Abschnitt. Quecksilberblasen. 8vo. *Marburg* 1870. The Author.

Williams (C. J. B., F.R.S.) Authentic Narrative of the Case of the late Earl St. Maur. 8vo. *London* 1870. The Author.

Symons's Monthly Meteorological Magazine. Nos. 42–45, 47–49. 8vo. *London* 1869–70. The Editor.

Natural History, or second division of "The English Cyclopædia." Supplement. roy. 8vo. *London* 1870. Mr. Alex. Ramsay, jun.

The First Proofs of the Universal Catalogue of Books on Art, compiled for the use of the National Art Library, and the Schools of Art in the United Kingdom. Vol. I. A to K. 4to. *London* 1870. The Lords of the Committee of Council on Education.

March 31, 1870.

Transactions.

Berwickshire Naturalists' Club. Proceedings. Vol. VII. No. 1. 8vo. *Alnwick* 1869. The Club.

Leipzig:—Astronomische Gesellschaft. Vierteljahrsschrift. Jahrgang IV. Heft 4. Supplementheft 2. Jahrgang V. Heft 1. 8vo. *Leipzig* 1869–70. The Society.

London:—British Museum. Catalogue of the Specimens of *Dermaptera saltatoria*, by Francis Walker. Part 3. 8vo. *London* 1870. A Guide to the Printed Books exhibited to the Public. 12mo. *London* 1870. The Trustees.

Transactions (*continued*).

Royal Agricultural Society. Journal. Second Series. Vol. VI. Part 1. 8vo. *London* 1870. The Society.

Melbourne:—Royal Society of Victoria. Transactions and Proceedings. Vol. IX. Part 1. 8vo. *Melbourne* 1868. The Society.

Philadelphia:—Academy of Natural Sciences. Journal. New Series. Vol. VI. Part 4. Vol. VII. (Leidy's Extinct Mammalian Fauna of Dakota and Nebraska.) 4to. *Philadelphia* 1869. Proceedings, 1869. Nos. 1, 2. 8vo. *Philadelphia.* American Journal of Conchology. Vol. V. Parts 1, 2. 8vo. *Philadelphia* 1869–70. The Academy.

Haidinger (W. v., For. Memb. R. S.) Ein vorhomerischer Fall von zwei Meteoreisenmassen bei Troja, 1864. Der Meteorit von Goalpara in Assam, nebst Bemerkungen über die Rotation der Meteoriten in ihrem Zuge, 1869. Bemerkungen zu Herrn Dr. Stanislas Meunier's Note über den Victorit oder Enstatit von Deesa, 1870. Review of the Royal Society Catalogue of Scientific Papers, 1870. 8vo. *Vienna.* The Author.

Miers (J., F.R.S.) Contributions to Botany, Iconographic and Descriptive, detailing the characters of Plants that are either new or imperfectly described. Vol. II. 4to. *London* 1860–69. The Author.

Zantedeschi (Fr.) Delle Oscillazioni calorifiche orarie, diurne, mensili ed annue del 1867. 8vo. *Venezia* 1870. The Author.

April 7, 1870.

Dr. WILLIAM ALLEN MILLER, Treasurer and Vice-President, in the Chair.

The following communications were read:—

I. "On Supra-annual Cycles of Temperature in the Earth's Surface-crust." By Prof. C. Piazzi Smyth, F.R.S. Received March 4, 1870.

(Abstract.)

The author presents and discusses the completely reduced observations, from 1837 to 1869 inclusive, of the four great earth-thermometers sunk into the rock of the Calton Hill, at the Royal Observatory, Edinburgh, by the late Principal Forbes, pursuant to a vote by the British Association for the Advancement of Science.

Leaving on one side the several natural-philosophy data which have been investigated from smaller portions of the same series of observations

both by Principal Forbes and Sir William Thomson, the author applies himself solely to trace the existence of other cycles than the ordinary annual one, in the rise and fall of the different thermometers.

Of such cycles, and of more than one year's duration, he considers that he has discovered three; and of these the most marked has a period of 11·1 years, or practically the same as Schwabe's numbers for new groups of solar spots. Several numerical circumstances, however, which the author details, show that the sun-spots cannot be the actual cause of the observed waves of terrestrial temperature, and he suggests what may be, concluding with two examples of the practical use to which a knowledge of the temperature cycles as observed may at once be turned, no matter to what cosmical origin their existence may be owing.

II. "On the Constituent Minerals of the Granites of Scotland, as compared with those of Donegal." By the Rev. Samuel Haughton, F.R.S., M.D. Dubl., D.C.L. Oxon., Fellow of Trinity College, Dublin. Received March 31, 1870.

During the past summer (1869) I completed my investigation of the constituent minerals of the Scotch Granites, and secured specimens, from the analysis of which I obtained the following results:—

I. *Orthoclase.*

	No. 1.	No. 2.	No. 3.	No. 4.
Silica	65·40	64·44	64·48	64·48
Alumina	19·04	18·64	20·00	20·00
Peroxide of iron	trace.	0·80	none.	none.
Lime	0·22	0·66	1·01	0·78
Magnesia	trace.	trace.	trace.	none.
Soda	3·63	2·73	1·72	2·19
Potash	11·26	12·15	12·81	12·10
Water	0·20	0·80	0·64	0·08
	99·75	100·22	100·66	99·63

No. 1. Stirling Hill, Peterhead. Occurs in an eruptive Granite, in veins, in well-developed reddish-pink opaque crystals, encrusted with crystals of Albite.

No. 2. Rubislaw, Aberdeen. Large beautiful reddish-pink opaque crystals in veins, associated with white Mica. The Granite of Rubislaw is of metamorphic origin, and different in character from the eruptive Granite of Peterhead. No Albite has been found in it.

No. 3. Peterculter, Aberdeen. In metamorphic Granite; white, translucent, large crystals.

No. 4. Callernish, extreme west of Lewis. In metamorphic Granite; in large grey crystals, with a slight shade of pink, translucent.

The oxygen ratio of these felspars is as follows:—

	No. 1.	No. 2.	No. 3.	No. 4.
Silica	33·956	33·456	33·478	33·477
Alumina &c. ..	8·898	8·950	9·348	9·348
Lime	0·061	0·187	0·286	0·221
Soda	0·929	0·699	0·440	0·561
Potash	1·908	2·059	2·171	2·051
	45·752	45·351	45·723	45·658

From this Table we find the oxygen ratios:—

	No. 1.	No. 2.	No. 3.	No. 4.
Silica	11·37	11·35	11·55	11·82
Peroxides	3·06	3·04	3·22	3·30
Protoxides	1·00	1·00	1·00	1·00

The Granites of central and western Scotland are metamorphic rocks, like those of Donegal and Norway, with which they are geologically identical; and truly eruptive Granite occurs at only a few localities, as, for example, near Peterhead.

The second felspar associated with Orthoclase in the Metamorphic Granites is Oligoclase, as in Donegal; while the second felspar associated with Orthoclase in the eruptive Granites is Albite, as in Mourne, Leinster, and Cornwall. The fact thus indicated by the Scotch Granites is completely in accordance with the mode of occurrence of Oligoclase and Albite in the Irish Granites.

II. *Oligoclase.*

	No. 1.	No. 2.
Silica	62·00	61·88
Alumina	23·20	24·80
Magnesia	——	Trace.
Lime	4·71	4·93
Soda	9·20	8·12
Potash	0·43	0·98
	99·54	100·71

No. 1. This Oligoclase occurs in the Granite of Craigie-Buckler, near Aberdeen; it is white and opaque, and so much resembles Cleavelandite in appearance as to have been mistaken for that variety of Albite; its analysis proves it to be Oligoclase. The crystals do not exhibit striation.

No. 2. From the Granite of Rhiconich, in the west of Sutherlandshire;

it is greyish white, semitranslucent, in large striated crystals, and resembles the Oligoclase of Ytterby, in Sweden.

The oxygen ratios of the Oligoclase are as follow:—

	No. 1.	No. 2.
Silica	32·191	32·128
Alumina	10·843	11·590
Lime	1·339	1·400
Soda	2·360	2·082
Potash	0·072	0·165
	46·805	47·365

Hence we obtain:—

	No. 1.	No. 2.
Silica	8·54	8·82
Peroxides	2·88	3·18
Protoxides	1·00	1·00

These oxygen ratios prove the felspars to be Oligoclase.

III. *Albite.*

Silica	68·00
Alumina	20·00
Lime	0·35
Magnesia	trace.
Soda	10·88
Potash	0·68
	99·91

This Albite occurs at Stirling Hill, near Peterhead, in eruptive Granite, and is found associated with red Orthoclase in veins; it encrusts the large crystals of Orthoclase, and is semitranslucent, and is generally stained on the surface by peroxide of iron.

Oxygen Ratios.

Silica	35·306	11·77
Alumina	9·348	3·11
Lime	0·099	
Soda	2·790	1·00
Potash	0·114	

This mineral is evidently a typical Albite.

There are two kinds of Mica found in the Scotch Granites, and both Micas resemble very closely the corresponding minerals of the Donegal Granites.

IV. *White Mica.*

Silica	44·40
Fluosilicon (Si F_3)	0·16
Alumina	37·36
Peroxide of iron	2·04
Lime	0·78
Magnesia	0·57
Soda	0·93
Potash	9·87
Protoxide of manganese	0·24
Water	1·84
	98·19

The specimen of Mica here analyzed came from veins in the Granite quarry of Rubislaw, near Aberdeen, and occurs in large plates, associated with red Orthoclase. It was carefully examined for lithia, but no trace of this alkali could be found in it.

The angles of the rhombic plates were 60° and 120° exactly, and the angle between its optic axes was found to be 72° 30′.

The black Mica in large crystals is very rare, but it seems abundantly disseminated in minute scales through most of the Scotch Granites. The following analysis was made on specimens found near Aberdeen by Prof. Nicol, and kindly forwarded to me by him, for the purposes of this paper:—

V. *Black Mica.*

Silica	36·50
Alumina	16·50
Peroxide of iron	18·49
Lime	1·11
Magnesia	7·44
Soda	0·92
Potash	8·77
Protoxide of iron	6·76
Protoxide of manganese	1·80
Water	1·60
	99·89

This mica was carefully examined for fluorine, and found not to contain any.

III. Researches on Vanadium.—Part III. Preliminary Notice. By Henry E. Roscoe, B.A., F.R.S. Received April 7, 1870.

I. Metallic Vanadium*.

In the second part of "Researches on Vanadium," it was stated that the metal absorbs hydrogen. This conclusion has been fully borne out by subsequent experiment; and it appears that the amount of absorbed or combined hydrogen taken up by the metal varies according to the state of division, first, of the chloride (VCl_2) from which the metal is prepared, and secondly, and especially, of the metal itself. The metal containing absorbed hydrogen slowly takes up oxygen on exposure to dry air, water being formed and the metal undergoing oxidation to the lowest oxide, V_2O. At this point the oxidation stops, but in moist air it proceeds still further.

The difficulty of obtaining metallic vanadium free from admixture of oxide has been again rendered evident. Perfectly pure tetrachloride was prepared in quantity, and from this pure dichloride was made. On heating this to whiteness in dry hydrogen for 48 hours a substance was obtained which gained on oxidation 70·7 per cent. (vanadium requiring 77·79 percentage increase), and therefore still contained a slight admixture of oxide.

The reducing action of sodium on the solid chlorides was next examined; in this case the reduction takes place quietly in an atmosphere of hydrogen at a red heat, and is best conducted in strong iron tubes. Explosions occur when sodium acts on the liquid tetrachloride. The substance thus obtained was found, after lixiviation, to be free from chlorine, and on washing it separated into two portions—(1) a light and finely divided black powder (trioxide), which remains in suspension, and is soluble in hydrochloric acid, and (2) a heavier grey powder, insoluble in hydrochloric acid, which soon deposits, and can, by repeated washing, be completely freed from the lighter trioxide. This bright grey powder consists of metallic vanadium, mixed with more or less oxide. If this metallic powder, after drying *in vacuô*, be reduced at a low red heat in a current of pure hydrogen, it takes fire spontaneously, even when cold, on exposure to air or oxygen, water being formed, whilst the vanadium undergoes oxidation, forming the blue oxide, V_2O_4. A portion of metal exposed for some weeks to the air also slowly absorbed oxygen, passing into the oxide, V_2O.

II. Vanadium and Bromine.

1. *Vanadium Tribromide*, VBr_3, molec. wt. = 291·3.—When excess of bromine is passed over vanadium mononitride heated to redness, a vivid action occurs, and dense dark-brown vapours are formed, condensing in the cooler portions of the tube to a greyish-black, opaque, amorphous mass of the tribromide. The tribromide is a very unstable compound, losing bro-

* Phil. Trans. 1869, p. 679.

mine even when kept sealed up in glass tubes; it is very deliquescent, and on heating in the air rapidly loses all its bromine and takes up oxygen, with formation of vanadic acid. On being thrown into water, the tribromide readily dissolves, forming a brown liquid (in this respect resembling the trichloride), which, on addition of a few drops of hydrochloric acid, turns of a bright green colour, showing the presence of a solution of an hypovanadic salt. No free bromine or hydrobromic acid is given off on dissolving the tribromide in water. That a more volatile higher bromide was not formed in this reaction was shown, inasmuch as, on distilling the excess of liquid which had collected in the receiver, it was found to consist of free bromine, containing mere traces of the tribromide mechanically carried over. The tribromide is likewise formed when bromine is passed over a red-hot mixture of vanadium trioxide and pure charcoal, as in the preparation of the tetrachloride; but this method is not one to be recommended, as the tube becomes constantly stopped up by the formation of the solid tribromide.

The analysis of the tribromide was made by dissolving the compound in water, and precipitating the bromine with excess of nitrate of silver, the vanadium being estimated as V_2O_5, either in the filtrate from the bromide of silver or in a separate portion. The bromine in the above determinations, obtained by precipitation as silver-salt, was invariably found to be too high, whilst the vanadium nearly agreed with the theoretical percentage. This is due to the fact pointed out by Stas, in his 'Recherches,' p. 156, that bromide of silver, when boiled, encloses mechanically a portion of the precipitant, which then cannot be washed out. The loss of weight obtained by reducing the bromide to metallic silver in a current of hydrogen, taken as bromine, gave more nearly agreeing numbers:—

		Calculated.	
			Mean of 6 determinations.
Vanadium....	$V = 51{\cdot}3$	17·61	18·44
			Mean of 3 determinations.
Bromine	$Br_3 = 240{\cdot}0$	82·39	80·86
	291·3	100·00	99·30

2. *Vanadium Oxytribromide*, or *Vanadyl Tribromide*, $VOBr_3$, molec. wt. $=307{\cdot}3$.—The oxytribromide is a dark-red transparent liquid, evolving white fumes on contact with the air, obtained by passing pure and dry bromine over vanadium trioxide (V_2O_3) heated to redness. Moisture prevents the formation of the oxytribromide; and it not only undergoes sudden decomposition when heated to 180°, but also slowly decomposes at the ordinary atmospheric temperatures. The boiling-point of the tribromide can, however, be brought below its temperature of decomposition by distillation *in vacuô*, and the liquid can then be freed completely from bromine by passing a current of dry air through the liquid. Under a pressure of 100 millims. the oxytribromide boils from 130° to 135°, and may be

distilled almost without decomposition. Vanadium oxytribromide dissolves in water, yielding a yellow-coloured solution, in which both vanadium and bromine were determined, after reduction with sulphurous acid :—

		Calculated.	Mean of several analyses.
V	= 51·3	16·69	16·75
Br_3	= 240·0	78·10	79·20
O	= 16·0	5·21	——
	307·3	100·00	

The specific gravity of the oxytribromide at 0° is 2·967.

3. *Vanadium Oxydibromide,* or *Vanadyl Dibromide,* $VOBr_2$, molec. wt. = 227·3.—This is a solid substance, of a yellowish-brown colour, obtained by the sudden decomposition of the foregoing compound at temperatures above 100°, or by its slow decomposition at the ordinary temperature.

The oxydibromide is very deliquescent, dissolving in water, with formation of a blue solution of a vanadious salt. When heated in the air it loses all its bromine, and is converted into V_2O_5.

Analysis gave :—

		Calculated.	Mean of several analyses.
V	= 51·3	22·57	22·45
Br_2	= 160·0	70·39	70·93
O	= 16·0	7·104	——
	227·3	100·00	

III. Vanadium and Iodine.

Iodine-vapour does not attack either the trioxide or the nitride at red heat; both these substances remain unchanged, and no trace of vanadium can be detected in the iodine which has passed over them.

IV. The Metallic Vanadates.

In the first part of these Researches (Phil. Trans. 1868) it was pointed out (1) that the salts analyzed by Berzelius must be considered as meta- or monobasic vanadates, (2) that the so-called bivanadates analyzed by Von Hauer are anhydro-salts, and (3) that the ortho- or tribasic vanadates contain 3 atoms of monad metal, the sodium salt being formed artificially by fusing 1 molecule of vanadium pentoxide with 3 molecules of carbonate of soda, when 3 molecules of carbon dioxide are expelled, whilst the ortho-salts occur native in many minerals. The present communication contains a description of these classes of salts, as well as of a new class of salts, the tetrabasic or pyro-vanadates.

Sodium Vanadates.

1. *Ortho-* or *Tri-Sodium Vanadate,* $Na_3VO_4 + 16H_2O_2$.—When a mixture of 3 molecules of Na_2CO_3 and 1 molecule of V_2O_5 is fused until no

further evolution of CO_2 is observed, a tribasic vanadate remains as a white crystalline mass. This mass dissolves easily in water, and on addition of absolute alcohol to the solution two layers of liquid are formed; the lower one solidifies after a time, forming an aggregation of needle-shaped crystals, which possess a strongly alkaline reaction. These having been washed with alcohol, and dried on a porous plate over sulphuric acid *in vacuô*, were analyzed with the following results:—

		Calculated.	Found.
Na_3	= 69·0	14·6	13·8
V	= 51·3	10·86	10·86
O_4	= 64·0	13·56	——
$16H_2O$	=288·0	60·97	60·44
	472·3	99·99	

The sodium in this and in the following compounds was separated from the vanadium by precipitating the vanadic acid as the perfectly insoluble basic lead salt hereafter described. This was dried at 100° and weighed, then dissolved in nitric acid and decomposed by sulphuric acid, and the solution of V_2O_5 in excess of this acid gave on evaporation a finely crystalline mass. The filtrate from the lead precipitate freed from lead yielded on evaporation sodium sulphate. Full analytical details of this method, as well as of the other by precipitation as the insoluble ammonium metavanadate, are given in the memoir. By frequent crystallizations the trisodium vanadate is slowly decomposed into the tetrasodium salt, caustic soda being formed. This singular reaction was most carefully examined and the amount of sodium hydroxide liberated determined volumetrically.

2. *Tetrasodium Vanadate*, $Na_4V_2O_7+18H_2O$.—This salt crystallizes in beautiful six-sided tables; it is easily soluble in water, insoluble in alcohol, and is precipitated by the latter liquid from aqueous solution in white scales of a silky lustre. As long as the salt contains free alkali or tribasic salt, it forms, on precipitation with alcohol, oily drops which solidify after some time. The tetrasodium vanadate is always formed by the first fusion of vanadic acid with excess of carbonate of soda, and can be easily prepared in the pure state by recrystallization.

		Calculated.	Found (mean of many determinations).
Na_4	= 92·0	14·58	14·61
V_2	= 102·6	16·27	15·97
O_7	= 112·0	17·27	——
$18H_2O$	= 324·0	51·38	51·80
	630·6	99·99	

The salt loses 17 molecules of water at 100°.

The corresponding Calcium and Barium Vanadates, $Ca_2V_2O_7$, and $Ba_2V_2O_7$, are white precipitates obtained by adding the chlorides to a

solution of tetrasodium vanadate. If calcium chloride be added to a solution of the trisodium salt, dicalcium vanadate is precipitated, the solution becoming strongly alkaline from formation of calcium hydroxide and absorbing carbonic acid from the air. Complete analysis showed that the calcium salt contains 2½ molecules of water of crystallization, whilst the barium salt is anhydrous.

Lead Vanadates.

1. *Tribasic or Ortho-Lead Vanadate*, $Pb_3\,2(VO_4)$.—Obtained as a light yellow insoluble powder on precipitating the tribasic sodium salt with a soluble lead salt; it yielded on analysis 11·75 per cent. of vanadium, the calculated quantity being 12·04 per cent.

2. *Vanadinite, the Double Orthovanadate and Chloride of Lead*, $3Pb_3\,VO_4 + Pb\,Cl_2$, can be artificially prepared by fusing for a few hours a mixture of vanadic acid, oxide of lead, and chloride of lead, in the above proportions, together with an excess of sodium chloride. After cooling, a greyish crystalline mass is left, containing cavities filled with long crystals having the same colour as the mass, which under the microscope could be distinguished as six-sided prisms. The crystalline powder is then boiled with water until no further traces of soluble chlorides are extracted.

The following analysis shows that this substance has the same composition as the vanadinites from Zimapan and Windischkappel, analyzed by Berzelius and Rammelsberg* :—

	Calculated. $3\,(Pb_3\,VO_4)\,Pb\,Cl_2$.	Natural vanadinite. Zimapan, Berzelius.	Windischkappel, Rammelsberg.	Artificial vanadinite.
Lead........	73·08	70·4	71·20	71·96
Vanadium....	10·86	——	9·77	11·11
Chlorine	2·50	2·54	2·23	2·31
Oxygen......	13·55	——	——	——

The specific gravity of the artificial vanadinite at 12° C. is 6·707, that of the natural being 6·886.

3. *Basic Di-Lead Vanadate*, $2(Pb_2\,V_2\,O_7) + Pb\,O$.—This salt is precipitated as a pale yellow powder when acetate of lead is added to a solution of disodium vanadate, the liquid acquiring an acid reaction. It is completely insoluble in water and in dilute acetic acid, but dissolves readily in nitric acid.

		Calculated.	Mean found.
Pb_5 ......=	1035·0	69·92	70·18
V_4 ....=	205·2	13·86	13·3
O_{15} ....=	240·0	16·22	——
	1480·2		

* Pyromorphite and apatite have already been artificially prepared by Deville and Caron, and also by Debray, whilst mimetesite has been obtained artificially by Lechartier.

Silver Vanadates.

1. *The Ortho-Silver Vanadate*, $Ag_3 VO_4$, is obtained as an orange-coloured precipitate by mixing a freshly prepared solution of the trisodium salt with a solution of silver nitrate, in which every trace of free acid has been neutralized; unless these precautions are attended to, the precipitate consists of a mixture of the ortho- and pyro-salt. The trisilver vanadate is insoluble in water, but readily dissolves in ammonia and nitric acid. Analysis gave the following results:—

		Calculated.	Found (mean).
Ag_3	=324·0	73·75	73·83
V	= 51·3	11·67	11·76
O_4	= 64·0	14·58	——
	439·3	100·00	

2. *The Tetrabasic Silver Vanadate*, $Ag_4 P_2 O_7$, is prepared by mixing a solution of the corresponding sodium salt with a neutral solution of nitrate of silver. It falls as a yellow dense crystalline precipitate, resembling in colour the ordinary phosphate of silver. On dissolving the salt in nitric acid, the silver is precipitated as chloride, and the vanadium determined as $V_2 O_5$.

Analysis gave:—

		Calculated.	Found.
Ag_4	=432	66·81	66·45
V_2	=102·6	15·87	15·97
O_7	=112·0	17·32	——
	646·6	100·00	

The reactions of the tri- and tetrabasic vanadates of the other metals are then described.

The author has to thank Messrs. Oelhofer and Finkelstein for the valuable assistance which they have given him in the above investigation.

The Society adjourned over the Easter Recess to Thursday, April 28.

April 28, 1870.

Dr. WILLIAM ALLEN MILLER, Treasurer and Vice-President, in the Chair.

Principal Dawson, LL.D., of M'Gill College, Montreal, was admitted into the Society.

The following communications were read:—

I. "On the Organs of Vision in the Common Mole." By Robert James Lee. Communicated by Samuel Solly, F.R.S. Received March 30, 1870.

The eye of the Common Mole and the structures connected with it undergo some remarkable changes during the growth of the animal. The gentleman who does me the honour to present the results of an investigation into that subject to the Royal Society was desirous that it should be undertaken in order to ascertain the cause of the anomalous condition in which the organ of vision is found in the adult Mole.

It was the suggestion of Mr. Solly that an examination of the eye of the young or fœtal Mole might assist in the explanation; for Mr. Solly had reflected much on the subject, and entertained reasons for believing that such an inquiry would be attended with a satisfactory result.

It is known that there is distinct evidence of the existence of an eye and other parts concerned in the endowment of sight in many of the various species of the Mole genus. To what extent, however, the defective state of the organs permit of sight, or whether the animal is totally blind, are questions still undecided.

That the organs of vision in the young Mole would be found in a more perfect state than in mature age was what Mr. Solly anticipated, while he conjectured, for physiological reasons, that the cause of the difference between them would be found to be a process of atrophy or degeneration in the various structures essential for the enjoyment of sight.

The specimens sent me for the purpose of examination consisted of a female Mole, which appeared from its dimensions to have attained the full period of development, if it had not somewhat exceeded it, and of six unborn young about an inch and a quarter long, and, as far as I could judge, beyond the middle of the period of gestation.

Before entering into anatomical details, I venture to review briefly the researches which have been made by anatomists into this subject. A summary of the views entertained by those who preceded him is given by Gottfried Treviranus, in his work published in 1820, 'Vermischte Schriften Anatomischen und Physiologischen Inhalts,' in the chapter on the Nerves of Sense in Mammalian Animals. From this account it appears that it was Zinn who first described an optic nerve in the Mole, and declared it to be a branch of that division of the fifth pair of nerves which is distributed to the nose.

The description by Zinn was published in the fourth volume of the Commentaries of the Royal Society of Gottingen. "The optic nerve," he says, "is long and of considerable tenuity. Its origin is the same as that of the very large nerve which passes to the proboscis. It takes a long oblique course, lying above the muscles of the nose, and passing in an outward and backward direction, surrounded by dense structures, is finally

inserted into the posterior part of the globe of the eye in the line of the axis of vision."

In 1813 Tiedemann published a description of the optic nerve and the fifth pair, which differed in a very important respect from the account given by Zinn; for he says that although the optic nerves are small and difficult to distinguish, yet they exist as separate nerves, and present the same general character as in most of the mammalia. Tiedemann carried his investigations still further, and declared the absence of the third, fourth, and sixth pairs of nerves. He described certain filaments, which he stated to be unconnected with the optic nerve, and to be similar to those branches which are found in the tissues around the eye in other animals. The absence of the third, fourth, and sixth pairs of nerves was subsequently asserted also by Carus; but his account of the origin and termination of the optic nerves is not quite intelligible to me. It appears, however, that the conclusions of Treviranus and Carus agreed that there was some connexion between the optic nerve and the fifth, which sufficed to supply the Mole to a certain degree with the sense of sight. Indeed the chief physiological fact which Treviranus endeavoured to establish in the chapter of the work alluded to, was that the nerves of one particular and special sense were capable, under certain circumstances, of becoming endowed with the properties of nerves of another and different sense. "The fifth pair of nerves in some mammalia supply the place of the most important nerve of sense" is the introductory sense in the chapter; or, in other words, that a nerve of touch and feeling may become a nerve of sight, that is, sensitive to the rays of light; and he concludes the chapter thus:—"I cannot but agree with Carus that the optic nerve and the fifth branch enter into connexion in the eye to produce the retina." This opinion met with opposition from Prof. Müller, who controverted it by the statement that true optic nerves had been exhibited to him by Dr. Henle (Baly's translation, p. 842).

From a remark of M. P. G. Pelletan, in his 'Mémoire sur la Spécialité des Nerves des Sens,' quoted by Mr. Solly in his work on the Brain, it would appear that that anatomist had made a very careful examination of the organs of vision, both in the adult and fœtal Mole, for he "recommends the dissection of either fœtal Moles, or very young ones, in whom the optic foramen is still distinct." The importance of this remark consists in the proof that Pelletan had observed that the optic foramina undergo some change subsequent to the birth of the animal.

Von Siebold has published the results of investigations into the difference between the eyes of certain species of *Talpa*. "The eyes are rudimentary," he says, "in the Mole and *Spalax typhlus*, which live underground; and above all in *Talpa cæca* and the Chrysochlores are the eyes rudimentary. They are a little more developed in the Musaraignes and the Common Mole. According to Ollivier (Bulletin de la Société Philomathique, vol. ii.

No. 38, p. 105) all the ordinary elements of the eye are found in *Spalax typhlus*."

Leydig, in his 'Handbuch der Histologie,' has some important remarks on the eyes of blind animals, and has described, in Muller's 'Archives,' 1854, p. 346, the cellular structure of the lens of the Mole's eye, as presenting the character of embryonic structure, from which he concludes that the lens remains in its primitive embryonic condition.

Mr. Solly's investigations were directed to the state of the optic commissures at the base of the brain. "In the Mole," he says, "in which the optic nerves are so extremely minute that they have often escaped detection, and are by many authors described as entirely wanting, these commissural fibres are found distinctly crossing the base of the skull opposite the usual situation of the optic commissure; while the small black speck, evidently the rudiment of the eye, is supplied by a minute branch from the fifth pair" (p. 289, *op. cit.*).

In Prof. Owen's work on the 'Comparative Anatomy and Physiology of Vertebrates' (vol. iv. p. 246), the organ of sight, like that of smell, is stated to be "wanting in a few mammals, the eyeball being reduced to the size and condition of the ocèllus in *Amblyopsis*, and to its simple primitive office of taking cognizance of light, a filament of the fifth aiding a remnant of the proper optic nerve. The Moles, especially the Italian kind, *Talpa cæca*, and Mole-rats, exemplify this condition, in which, as in *Spalax typhlus*, the skin passes over the ocellus without any palpebral opening or loss of hair."

Mr. Herbert Mayo has given a similar description in his 'Physiology,' and has supplemented it by a drawing, in which the fifth nerve is represented as sending a filament directly to the globe of the eye.

From the above enumeration of the views entertained by anatomists regarding the eye and optic nerve of the Mole, it is apparent that attention has been directed by some to the eye in particular, and to the structures intimately connected with it, while others have arrived at their conclusions from examination of the interior of the skull and the optic region of the brain.

It remained therefore to ascertain the condition of the optic nerve in the posterior part of the orbit, especially that portion of the nerve which lies in the optic foramen, and thus endeavour to connect the appearances described in the eye with those observed at the base of the brain.

It is proposed to give an account of the dissection of the full-grown Mole, in order to contrast the state of the eye, the optic nerve, and the cranium with that which those parts present in the fœtal Mole, following such an arrangement of the facts that the important points of difference shall be apparent without separate comparison.

The eye of the Common Mole presents the appearance of a minute black and shining bead, closely attached to the skin of the head, and concealed

by the hair so completely that it is difficult sometimes to discover it. In removing the skin the small globe is easily detached at the same time, and no indication remains of the exact position in which it was situated. This shows that in the Mole the cavity of the orbit is wanting, and that the structures usually found in the vicinity of the eye are in a different condition from that which they present in other mammalia. It is necessary, therefore, to divide the skin around the base of the eye in order to preserve the connexion between the globe and the subjacent tissues.

Beneath the eye, and forming a basis on which it rests, is "a firm mass of cellular fibrous tissue which assumes on dissection a fusiform shape, with an attenuated portion passing towards the base of the skull." The filament becomes so exceedingly delicate in the deeper part of the orbit that the difficulty of ascertaining its precise condition is probably the reason of the difference of opinion on the subject.

In Mr. Solly's specimen there was found to be no attachment whatever of the filament to the base of the skull; but in a former dissection of a smaller, and probably younger specimen, the continuity between the bone and the tissue was evident.

The filament of tissue above described, and the connexion which it formed between the eye and the skull, induced me to examine it microscopically, in order to ascertain whether it contained nervous fibres, or possessed any of the characters of the optic nerve.

It exhibited a tendency to divide in a longitudinal direction when needles were applied to it, and presented the appearance of cellular tissue, without, however, any trace of nerve-fibre. It will be seen nevertheless, from the description of the optic nerve in the fœtal Mole, that this delicate thread is the only vestige which remains of that important part of the organs of vision in the full-grown Mole.

With regard to some minute branches of nerves and blood-vessels which pass into the tissue forming the base of the eye, both on its outer and inner side, it is not in my power to say definitely from whence they come, as their minute size prevented me from tracing them in the deeper part of the orbit to their points of exit from the skull.

The eye of the full-grown Mole presents a surface uniformly black and glistening, in which there is no indication of a cornea and sclerotic distinct from one another, nor any evidence of an iris or pupillary aperture. Within the globe, when ruptured with the points of needles, a layer of black pigmentary particles was found to line the internal surface of the dense structure which corresponds to the sclerotic.

In addition there was a confused mixture of grey and white granular substance, in which there was no distinct evidence of remains of the usual contents of the globe of the eye, though, as will be seen, those structures exist in fœtal life.

The specimens were sent to me preserved in alcohol, consequently the brain was firm, and easy to be removed entire from the cranium.

On raising the anterior lobes gently from the base of the skull, it was ascertained that no nerves connected the brain with the bone anterior to the fifth pair. The base of the brain also exhibited an entire absence of the optic nerves beyond a vestige in a very minute chiasma, as described by Mr. Solly.

On examining the internal surface of the base of the skull, the usual foramina for the optic nerves are found to be wanting, a condition which is observed with facility in the dried specimens in the Museum of the Royal College of Surgeons. Among these there is one in which there is a vestige of an optic foramen on the left side of the head, while on the opposite side the surface is smooth and perfect.

In the arrangement of the details which have been given above of the appearances observed in the course of the examination, attention has been directed to three points in particular, namely to the condition of that part of the optic nerve which is situated externally to the skull, and which exists as a mere thread of connective tissue; secondly, to the eye itself, and the structures within, so far as it was necessary to consider them in their efficiency for optical purposes; thirdly, to the internal surface of the skull in its relation to the part of the brain from which the optic nerves take their origin.

The following description of the various structures in the fœtal Mole will be more general than the above account of them in the full-grown Mole, as five specimens instead of one were examined.

On the removal of the skin and a layer of muscular tissue subjacent, a part of the globe of the eye is exposed. When the whole side of the face and the temporal region are dissected, the eye is found to be in close proximity to the large branch of the facial nerve, as is represented in the drawing accompanying this account.

The eye has the usual appearance presented by the organ in most fœtal mammalia; in form globular, and in size proportionate to the head of the animal; the cornea translucent; the sclerotic perfectly distinct, and of dense white tissue; the iris apparent through the cornea, with a clear pupillary aperture.

Between the eye and the facial nerve a small portion of the optic nerve is seen in the superficial dissection, and appears to form an upright peduncle for the globe.

It is necessary to divide the seventh pair in order to examine the deeper parts of the orbit. When the dissection is completed, and the optic nerve exposed in its whole extent, from the eye to the base of the cranium, the branches of the fifth pair of nerves are brought into view. The main branch of the second division of the fifth nerve lies a little below the optic nerve, parallel with it, and supplies large and numerous branches to the anterior part of the face. There is no necessity to describe minutely the appearance presented in the deep dissection of the orbit, as I observed nothing unusual to require particular notice. There are some minute

muscles attached to the globe which do not admit of separation into distinct parts, but completely surround the posterior half of the globe.

To trace the optic nerve through its foramen to the brain was successfully accomplished in only one dissection. After exposing the optic nerve and the eye completely, all the surrounding parts were removed, and a section made through the skull so as to exhibit a lateral view of the interior of the cranium.

The brain itself was disorganized in all the young specimens; but in the dissection just alluded to the optic nerve was seen to pass through the base of the skull, and to enter the membranes to a short distance, so that it would have been possible, if the brain had remained perfect, to trace it to its origin.

With regard to the eye itself, no difficulty was experienced in separating the iris, choroid, and lens. The other structures usually existing in the eye had been so long subjected to the influence of the alcohol that I could not determine their condition.

It must necessarily happen that many interesting observations are made in the course of an investigation like that which has been briefly described, and many minute details might have been added to this account; but it appeared to me to be desirable to limit the details, as far as possible, to those which were sufficient to establish the remarkable physiological fact that the Mole, at the time of birth, is endowed with organs of vision of considerable perfection, while in mature age it is deprived of the means of sight in consequence of certain changes which take place in the base of the skull, terminating in the destruction of the most important structures on which the enjoyment of the sense of sight depends.

II. "On an Aplanatic Searcher, and its Effects in improving High-Power Definition in the Microscope." By G. W. ROYSTON-PIGOTT, M.A., M.D. Cantab., M.R.C.P., F.R.A.S., F.C.P.S., formerly Fellow of St. Peter's College, Cambridge. Communicated by Prof. STOKES, Sec. R. S. Received March 31, 1870.

(Abstract.)

The Aplanatic Searcher is intended to improve the penetration, amplify magnifying-power, intensify definition, and raise the objective somewhat further from its dangerous proximity to the delicate covering-glass indispensable to the observation of objects under very high powers.

The inquiry into the practicability of improving the performance of microscopic object-glasses of the very finest known quality was suggested by an accidental resolution in 1862 of the Podura markings into black beads. This led to a search for the cause of defective definition, if any existed. A variety of first-class objectives, from the $\frac{1}{16}$ to the $\frac{1}{4}$, failed to show the beading, although most carefully constructed by Messrs. Powell and Lealand.

Experiments having been instituted on the nature of the errors, it was found that the instrument required a better distribution of power; instead of depending upon the deepest eyepieces and most powerful objectives hitherto constructed, that better effects could be produced by regulating a more gradual bending or refraction of the excentrical rays emanating from a brilliant microscopic origin of light.

It then appeared that delusive images, which the writer has ventured to name *eidōla**, exist in close proximity to the best focal point (where the least circle of confusion finds its locus).

(I.) That these images, possessing extraordinary characters, exist principally above or below the best focal point, according as the objective spherical aberration is positive or negative.

(II.) That test-images may be formed of a high order of delicacy and accurate portraiture in *miniature*, by employing an objective of twice the focal depth, or, rather, half the focal length of the observing objective.

(III.) That such test-images (which may be obtained conveniently two thousand times less than a known original) are formed (under precautions) with a remarkable freedom from aberration, which appears to be reduced in the miniature to a *minimum*.

(IV.) The beauty or indistinctness with which they are displayed (especially on the immersion system) is a marvellous test of the correction of the observing objective, but an indifferent one of the image-forming objective used to produce the testing miniature.

These results enable the observer to compare the known with the unknown. By observing a variety of brilliant images of known objects, as gauze, lace, an ivory thermometer, and sparkles of mercury, all formed in the focus of the objective to be tested with the microscope properly adjusted so that the axes of the two objectives may be coincident, and their corrections suitably manipulated, it is practicable to compare known delusions with suspected phenomena.

It was then observed (by means of such appliances) that the aberration developed by high-power eyepieces and a lengthened tube followed a peculiar law.

A. A lengthened tube increased aberration faster than it gained power (roughly the aberration varied as v^2, while the power varied as v).

B. As the image was formed by the objective at points nearer to it than the *standard distance of nine inches*, for which the best English glasses are corrected, the writer found the aberration diminished faster than the power was lost, by shortening the body of the instrument.

C. The aberration became negatively affected, and required a positive compensation.

D. Frequent consideration of the equations for aplanatism suggested the

* From εἴδωλον, a false spectral image.

idea of searching the axis of the instrument for aplanatic foci, and that many such foci would probably be found to exist in proportion to the number of terms in the equations (involving curvatures and positions).

E. The law was then ascertained that power could be raised, and definition intensified, by positively correcting the searching lenses in proportion as they approached the objective, at the same time applying a similar correction to the observing objective.

The chief results hitherto obtained may be thus summarized.

The writer measured the distance gained by the aplanatic searcher, whilst observing with a half-inch objective with a power of seven hundred diameters, and found it *two-tenths of an inch increase*; so that optical penetration was attainable with this high power through plate-glass nearly one quarter of an inch thick, whilst *visual* focal depth was proportionably increased.

The aplanatic searcher increases the power of the microscope from two and a half to five times the usual power obtained with a third or C eyepiece of one inch focal length. The eighth thus acquires the power of a twenty-fifth, the penetration of a one-fourth. And at the same time the lowest possible eyepiece (3-inch focus) is substituted for the deep eyepiece formed of minute lenses, and guarded with a minutely perforated cap. The writer lately exhibited to Messrs. Powell and Lealand a brilliant definition, under a power of four thousand diameters, with their new "eighth immersion" lens, by means of the searcher and low eyepiece.

The traverse of the aplanatic searcher introduces remarkable chromatic corrections displayed in the unexpected colouring developed in microscopic test-objects*.

The singular properties or, rather, phenomena shown by eidola, enable the practised observer in many cases to distinguish between true and delusive appearances, especially when aided by the aberrameter applied to the objective to display excentrical aberration by cutting off excentrical rays.

Eidola are symmetrically placed on each side of the best focal point, as ascertained by the aberrameter when the compensations have attained a delicate balance of opposite corrections.

If the beading, for instance, of a test-object exists in two contiguous parallel planes, the eidola of one set is commingled with the true image of the other. But the upper or lower set may be separately displayed, either by depressing the false eidola of the lower stratum, or elevating the eidola of the upper; for when the eidola of two contiguous strata are intermingled, correct definition is impossible so long as the aperture of the objective remains considerable.

One other result accrues: when an objective, otherwise excellent, cannot

* Alluded to by Mr. Reade, F.R.S., in the 'Popular Science Review' for April 1870.

be further corrected, the component glasses being already closely screwed up together, a further correction can be applied by means of the adjustments of the aplanatic searcher itself, all of which are essentially conjugate with the actions of the objective and the variable positions of its component lenses; so that if δx be the traversing movements of the objective lenses, δv that of the searcher, F the focal distance of the image from the objective when δx vanishes, f the focal distance of the virtual image formed by the facet lenses of the objective,

$$\frac{\delta v}{\delta x}=-\left(\frac{F}{f}\right)^2.$$

The *appendix* refers to plates illustrating the mechanical arrangements for the discrimination of eidola and true images, and for traversing the lenses of the aplanatic searcher.

The plates also show the course of the optical pencils, spurious disks of residuary aberration and imperfect definition, as well as some examples of "high-power resolution" of the Podura and Lepisma beading, as well as the amount of amplification obtained by Camera-Lucida outline drawings of a given scale.

III. "On a Cause of Error in Electroscopic Experiments." By Sir Charles Wheatstone, F.R.S. Received April 26, 1870.

To arrive at accurate conclusions from the indications of an electroscope or electrometer, it is necessary to be aware of all the sources of error which may occasion these indications to be misinterpreted.

In the course of some experiments on electrical conduction and induction which I have recently resumed, I was frequently delayed by what at first appeared to be very puzzling results. Occasionally I found that I could not discharge the electrometer with my finger, or only to a certain degree, and that it was necessary, before commencing another experiment, to place myself in communication with a gas-pipe which entered the room. How I became charged I could not at that time explain; the following chain of observations and experiments, however, soon led me to the true solution.

I was sitting at a table not far from the fireplace with the electrometer (one of Peltier's construction) before me, and was engaged in experimenting with disks of various substances. To ensure that the one I had in hand, which was of tortoiseshell, should be perfectly dry, I rose and held it for a minute before the fire; returning and placing it on the plate of the electrometer, I was surprised to find that it had apparently acquired a strong charge, deflecting the index of the electrometer beyond 90°. I found that the same thing took place with every disk I thus presented to the fire, whether of metal or any other substance. My first impression was that the disk had been rendered electrical by heat, though it would have been extraordinary that, if so, such a result had not been observed before; but

on placing it in contact with a vessel of boiling water, or heating it by a gas-lamp, no such effect was produced. I next conjectured that the phenomenon might arise from a difference in the electrical state of the air in the room and that at the top of the chimney ; and to put this to the proof, I adjourned to the adjacent room where there was no fire, and bringing my disk to the fireplace I obtained precisely the same result. That this conjecture, however, was not tenable was soon evident, because I was able to produce the same deviation of the needle of the electrometer by bringing my disk near any part of the wall of the room. This seemed to indicate that different parts of the room were in different electrical states ; but this again was disproved by finding that when the position of the electrometer and the place where the disk was supposed to be charged were interchanged, the charge of the electrometer was still always negative. The last resource was to assume that my body had become charged by walking across the carpeted room, though the effect was produced even by the most careful treading. This ultimately proved to be the case ; for resuming my seat at the table and scraping my foot on the rug, I was able at will to move the index to its greatest extent.

Before I proceed further I may state that a gold-leaf electrometer shows the phenomena as readily.

When I first observed these effects the weather was frosty ; but they present themselves, as I have subsequently found, almost equally well in all states of the weather, provided the room be perfectly dry.

I will now proceed to state the conditions which are necessary for the complete success of the experiments, and the absence of which has prevented them from being hitherto observed in the striking manner in which they have appeared to me.

The most essential condition appears to be that the boot or shoe of the experimenter must have a thin sole and be perfectly dry ; a surface polished by wear seems to augment the effect. By rubbing the sole of the boot against the carpet or rug, the electricities are separated, the carpet assumes the positive state and the sole the negative state ; the former being a tolerable insulator, prevents the positive electricity from running away to the earth, while the sole of the foot, being a much better conductor, readily allows the charge of negative electricity to pass into the body.

So effective is the excitation, that if three persons hold each other by the hands, and the first rubs the carpet with his foot while the third touches the plate of the electrometer with his finger, a strong charge is communicated to the instrument.

Even approaching the electrometer by the hand or body, it becomes charged by induction at some distance.

A stronger effect is produced on the index of the instrument if, after rubbing the foot against the carpet, it be immediately raised from it. When the two are in contact, the electricities are in some degree coerced or dissimulated ; but when they are separated, the whole of the negative

electricity becomes free and expands itself in the body. A single stamp on the carpet followed by an immediate removal of the foot causes the index of the electrometer to advance several degrees, and by a reiteration of such stamps the index advances 30° or 40°.

The opposite electrical states of the carpet and the sole of the boot were thus shown: after rubbing, I removed the boot from the carpet, and placed on the latter a proof-plate (*i. e.* a small disk of metal with an insulating handle), and then transferred it to the plate of the electrometer; strong positive electricity was manifested. Performing the same operation with the sole of the boot a very small charge was carried, by reason of its ready escape into the body.

The negative charge assumed by sole-leather when rubbed with animal hair was thus rendered evident. I placed on the plate of the electrometer a disk of sole-leather and brushed it lightly with a thick camel's-hair pencil; a negative charge was communicated to the electrometer, which charge was principally one of conduction, on account of the very imperfect insulating power of the leather.

Various materials, as India-rubber, gutta percha, &c., were substituted for the sole of the boot; metal plates were also tried; all communicated negative electricity to the body. Woollen stockings are a great impediment to the transmission of electricity from the boot; when these experiments were made I wore cotton ones.

When I substituted for the electrometer a long wire galvanometer, such as is usually employed in physiological experiments, the needle was made to advance several degrees.

At the Meeting of the British Association at Dublin in 1857, Professor Loomis, of New York, attracted great attention by his account of some remarkable electrical phenomena observed in certain houses in that city. It appears that in unusually cold and dry winters, in rooms provided with thick carpets and heated by stoves or hot-air apparatus to 70°, electrical phenomena of great intensity are sometimes produced. A lady walking along a carpeted floor drew a spark one quarter of an inch in length between two metal balls, one attached to a gas-pipe, the other touched by her hand; she also fired ether, ignited a gaslight, charged a Leyden jar, and repelled and attracted pith-balls similarly or dissimilarly electrified. Some of these statements were received with great incredulity at the time both here and abroad, but they have since been abundantly confirmed by the Professor himself and by others. (See Silliman's American Journal of Science, July 1858.)

My experiments show that these phenomena are exceptional only in degree. The striking effects observed by Professor Loomis were feeble unless the thermometer was below the freezing-point, and most energetic when near zero, the thermometer in the room standing at 70°. Those observed by myself succeed in almost any weather, when all the necessary conditions are fulfilled. Some of these conditions must frequently be

present, and experimentalists cannot be too much on their guard against the occurrence of these abnormal effects. I think I have done a service to them, especially to those engaged in the delicate investigations of animal electricity, by drawing their attention to the subject.

May 5, 1870.

Lieut.-General Sir EDWARD SABINE, K.C.B., President, in the Chair.

In conformity with the Statutes, the names of the Candidates recommended for election into the Society were read from the Chair, as follows:—

William Froude, C.E.
Edward Headlam Greenhow, M.D.
James Jago, M.D.
Nevil Story Maskelyne, M.A.
Maxwell Tylden-Masters, M.D.
Alfred Newton, M.A.
Andrew Noble, Esq.
Capt. Sherard Osborn, R.N.
Rev. Stephen Parkinson, B.D.
Capt. Robert Mann Parsons, R.E.
William Henry Ransom, M.D.
Robert H. Scott, Esq.
George Frederic Verdon, C.B.
Augustus Voelcker, Ph.D.
Samuel Wilks, M.D.

THE BAKERIAN LECTURE was delivered by JOHN W. DAWSON, LL.D., F.R.S., &c., Principal and Vice-Chancellor of M'Gill College, Montreal, "On the Pre-Carboniferous Floras of North-Eastern America, with especial reference to that of the Erian (Devonian) Period." The following is an Abstract.

The attention of the author was first directed to the Devonian as distinguished from the Carboniferous flora by the discovery, on the part of Sir W. E. Logan, in 1843, of some remarkable remains of plants in the Sandstones of Gaspé, Canada. In 1859, after visiting Gaspé to study these plants *in situ*, the author published descriptions of them, and more particularly of the two characteristic Lower-Devonian genera *Prototaxites* and *Psilophyton*, in the Journal of the Geological Society.

Subsequently additional material was obtained by personal investigation of the Devonian of Maine and New Brunswick, and, through the kindness of Prof. James Hall, from that of New York. These additional plants were also published in the Journal of the Geological Society.

Still more recently, a thorough re-examination of the Gaspé beds, the systematic exploration of the plant-bearing beds near St. John by Prof. Hartt, and fresh collections made by Prof. Hall have enabled the author to prepare a catalogue of 121 species, and to attempt a thorough revision

of the Erian flora, and an investigation of its conditions of growth and relations to the Carboniferous flora.

The term "Erian" is applied to the formations included between the top of the Upper Silurian and the base of the Carboniferous, on account of the uncertainties which have attended the subdivision and limitation of the Devonian of Europe, and also on account of the immense area occupied by these beds on the south and west of Lake Erie, and their admirable development with regard to subdivisions and fossils. The name "Erie Division" was also that originally applied to this typical series by the geologists of the Survey of New York.

A large part of the paper was occupied with the revision of the Erian flora, including the description of twenty-three new species, and more ample descriptions of others previously known only in fragments. Large trunks of *Prototaxites*, from the base of the Lower Devonian, were described, and full details given of the form, structures, and fructification of two species of *Psilophyton*. The new genus *Ormoxylon* was described. The genus *Cyclostigma* was noticed, as represented by two species in America, and its foliage and fruit described for the first time. The genera of the Erian Ferns were examined and corrected, and several interesting trunks and stipes belonging to Tree-ferns were described. The fruits of the genus *Cardiocarpum* were illustrated with reference to their structure. The occurrence of *Lepidophloios*, *Calamodendron*, and other forms in the Middle Devonian was noticed for the first time.

The third part of the memoir was occupied with comparisons and general conclusions. At the close of the Upper-Silurian period there was a great subsidence of the land in Eastern America, proved by the wide extent of the marine beds of the Lower Helderberg (Ludlow) group. It was on the small areas of Lower-Silurian and Laurentian land remaining after this subsidence that the oldest land plants known in the region flourished. Re-elevation occurred early in the Devonian period, and the known flora receives considerable extension in the shallow-water beds of the Lower Erian. The subsidence indicated by the great Corniferous limestone interrupted these conditions on the west side of the Appalachians, but not on their eastern side. At the close of this we find the rich Middle-Devonian flora, which diminishes toward the close of the period; and, after the physical disturbances which on the east side of the Appalachians terminated the Erian age, it is followed by the meagre and quite dissimilar flora of the Lower Carboniferous; and this, after the subsidence indicated by the Carboniferous limestone, is followed by the Coal-formation flora.

If we compare the Erian and Carboniferous floras, we find that the leading genera of the latter are represented in the former, but, for the most part, under distinct specific forms, that the Erian possesses some genera of its own, and that many Carboniferous genera have not yet been recognized in the Erian. There is also great local diversity in the Erian flora, conveying the impression that the conditions affecting the growth of

plants were more varied, and the facilities for migration of species less extensive, than in the Carboniferous.

In comparing the Erian flora of America with the Devonian of Europe, we meet with the difficulty that little is known of the plants of the Lower and Middle Devonian in Europe. There are, however, specimens in the Museum of the Geological Survey which show, in connexion with facts which can be gleaned from the works of continental writers, that *Psilophyton* occupied the same important place in Europe which it did in America; and in the Upper Devonian the generic forms are very similar, though the species are, for the most part, different.

In Eastern America no land flora is known below the Upper Silurian; and even in that series the plants found are confined to the genus *Psilophyton*. Independently, however, of the somewhat doubtful Lower-Silurian plants stated to have been found in Europe, there are indications, in the Lower-Erian flora, that it must have been the successor of a Silurian flora as yet almost unknown to us; and the line of separation between this old flora and that of the Devonian proper seems to be at the base of the Middle Devonian.

In applying these facts and considerations to the questions relating to the introduction and extinction of species, and the actual relations of successive floras, it was proposed to compare what might be called specific types,—that is, forms which in any given period could not be rationally supposed to be genetically related. Of such specific types, at least fifty may be reckoned in the Erian flora; of these, only three or four are represented in the Carboniferous by identical species, while about one half are represented by allied species. The remainder have no representatives.

A Table of specific types of the Erian was given, and its bearing shown on the questions above referred to; and the hope was expressed that by separating such types from doubtful species and varietal forms, some progress might be made towards understanding, at least, the times and conditions in which specific types were introduced and perished, and the range of varietal forms through which they passed.

Presents received April 7, 1870.

Transactions.

Berwickshire Naturalists' Club. Proceedings. Vol. V. No. 3. 8vo. *Alnwick* 1865. The Club.

Lyons:—Académie Impériale des Sciences, Belles-Lettres et Arts. Mémoires. Classe des Sciences. Tome XVII. 8vo. *Paris* 1869–70. The Academy.

Naples:—Società Reale. Rendiconto delle tornate e dei lavori dell' Accademia di Scienze Morali e Politiche. Anno 8. Settembre–Dicembre 1869. 8vo. *Napoli* 1869. The Academy.

Transactions (*continued*).

Rotterdam :—Bataafsch Genootschap der Proefondervindelijke Wijsbegeerte. Feestrede door K. M. Giltay. 4to. *Rotterdam* 1869. The Society.

Turin :—Regio Osservatorio dell' Università. Bollettino Meteorologico ed Astronomico. Anno 3. 1868. 4to. *Torino.* The Observatory.

Washington :—War Department, Surgeon-General's Office. Circular No. 2. (A Report on Excisions of the Head of the Femur for Gunshot Injury.) 4to. *Washington* 1869. The Surgeon-General U.S. Army.

Caligny (A. de) Liste des Mémoires et Notes présentés à l'Académie des Sciences ou publiés dans divers recueils. 4to. *Versailles* 1869. Rapport sur "Expériences faites à l'écluse de l'Aubois, pour déterminer l'effet utile de l'Appareil à l'aide duquel M. de Caligny diminue dans une proportion considérable la consommation d'eau dans les canaux de Navigation." 4to. *Paris* 1869. Sur l'effet de l'Appareil à tube oscillant, d'après les expériences du jury de l'Exposition Universelle de 1867. 4to. *Paris* 1869. Note sur la fondation de l'ancien port de Cherbourg. 8vo. *Paris* 1868. The Author.

Settimanni (César) D'une seconde nouvelle méthode pour déterminer la Parallaxe du Soleil. 8vo. *Florence* 1870. The Author.

Canadian Naturalist and Quarterly Journal of Science. New series. Vol. IV. Nos. 3, 4. 8vo. *Montreal* 1869. The Natural-History Society of Montreal.

April 28, 1870.

Transactions.

Liverpool :—Historic Society of Lancashire and Cheshire. Transactions. New series. Vol. IX. Session 1868–69. 8vo. *Liverpool* 1869. The Society.

London :—British Association. Report of the Twenty-ninth Meeting, held at Exeter in August 1869. 8vo. *London* 1870. The Association.

Mathematical Society. Proceedings. Nos. 20, 21. Index, &c. 8vo. *London* 1869. The Society.

Royal Geographical Society. Journal. Vol. XXXIX. 8vo. *London* 1869. Proceedings. Vol. XIV. No. 1. 8vo. *London* 1870. The Society.

Le Roy (A.) Liber Memorialis. L'Université de Liége depuis sa fondation. roy. 8vo. *Liége* 1869. The University.

Michalowski (F.) Origines Celtiques. 8vo. *Saint Étienne* 1870. The Author.

Miquel (F. A. G.) Catalogus Musei Botanici Lugduni-Batavi. Pars Prima, Flora Japonica. 8vo. *Hagæ Comitis.* Annales Musei Botanici. Tom. IV. Fasc. 6–10. fol. *Amst.* 1869. The Museum.

Miers (J., F.R.S.) Contributions to Botany, Iconographic and Descriptive. Vol. I. 4to. *London* 1851–61. The Author.

Owen (Prof., F.R.S.) Monograph on the British Fossil Cetacea from the Red Crag. No. 1 containing Genus *Ziphius.* 4to. *London* 1870. Monograph of the Fossil Reptilia of the Liassic Formations. Part second, *Pterosauria.* 4to. *London* 1870. The Author.

Plateau (J.) Mémoire sur les Phénomènes que présente une Masse liquide libre et soustraite à l'action de la Pesanteur. Première Partie. Recherches expérimentales et théoriques sur les figures d'Equilibre d'une Masse liquide sans Pesanteur. Séries 2–6. 4to. *Bruxelles* 1842–61. The Author.

Plateau (Félix.) Recherches sur les Crustacés d'Eau douce de Belgique. Parties 2, 3. 4to. *Bruxelles* 1868. Matériaux pour la Faune Belge. Crustacés Isopodes Terrestres. 8vo. *Bruxelles* 1870. The Author.

Richards (Capt., F.R.S.) and Lieut.-Col. Clarke. Report on the Maritime Canal connecting the Mediterranean at Port Saïd with the Red Sea at Suez. fol. *London* 1870. Captain Richards, R.N., F.R.S.

Seven Photographs of the Total Eclipse of the Sun, August 7, 1869, Surgeon-General's Office, Army Medical Museum, Washington. The Surgeon-General.

Reports on Observations of the Total Eclipse of the Sun, August 7, 1869, conducted under the direction of Commodore B. F. Sands. 4to. *Washington* 1869. The U.S. Naval Observatory.

May 5, 1870.

Transactions.

Baltimore:—Peabody Institute. Address of the President to the Board of Trustees on the Organization and Government of the Institute. 8vo. 1870. Discourse on the Life and Character of George Peabody, by S. T. Wallis. 8vo. 1870. The Institute.

Brussels:—Académie Royale. Mémoires Couronnés et Mémoires des Savants Étrangers. Tome XXXIV. 4to. *Bruxelles* 1870. Mémoires Couronnés et Autres Mémoires. Collection in 8vo. Tome XXI. 8vo. *Bruxelles* 1870. Bulletins. 2e série. Tomes XXVII.,

Transactions (*continued*).
XXVIII., XXIX. Nos. 1–3. 8vo. 1869–70. Annuaire 1870. 12mo. 1870. Nederlandsche Gedichten uit de Veertiende eeuw van Jan Boendale, Hein van Aken en anderen, door F. A. Snellaert. 8vo. *Brussel* 1869. The Academy.

Emden:—Naturforschende Gesellschaft. Vierzigster Jahresbericht für 1854. 8vo. *Emden* 1855. Kleine Schriften, V.–XIV. 8vo & 4to. *Emden* 1858–69. Die Gewitter des Jahres 1855; Ein Beitrag, von M. A. F. Prestel. 8vo. *Emden* 1856. The Society.

Vienna:—K. k. Geologische Reichsanstalt. Jahrbuch. Jahrgang 1869. XIX. Band. Nos. 14–18. 8vo. *Wien* 1869. The Institution.

Breen (Hugh) On the Corrections of Bouvard's Elements of the Orbits of Jupiter and Saturn. 4to. *London* 1870. The Author.

Dempsey (J. M.) Our Ocean Highways: a condensed Universal Route Book by Sea, by Land, and by Rail. 8vo. *London* 1870. The Editor.

Quetelet (A., For. Mem. R. S.) Physique Sociale ou Essai sur le développement des facultés de l'Homme. Tome II. 8vo. *Bruxelles* 1869. Notice sur le Congrès Statistique de Florence en 1867. 4to. Observations des Phénomènes Périodiques pendant les Années 1867 et 1868. 4to. Sur les Orages observées en Belgique 1868–69. 8vo. Notices sur les Aurores Boréales des 15 Avril et 13 Mai, 1869. 8vo. Note sur l'Aurore Boréale du 6 Octobre et les Orages de 1869. 8vo. Sur les Etoiles Filantes du Mois d'Aout 1869, observées à Bruxelles. 8vo. Statistique Internationale de l'Europe, plan adopté par les délégués officiels. 8vo. Congrès international de statistique des délégués des différents pays. 8vo. Sur le phénomène de la Menstruation. 8vo. Annales de l'Observatoire Royal de Bruxelles. Tome XIX. 4to. 1869. Annuaire de l'Observatoire 1870. 37e année. 12mo. 1869.

Reade (Rev. J. B., F.R.S.) Address delivered before the Royal Microscopical Society at the Anniversary Meeting, Feb. 9, 1870. 8vo. *London* 1870. The Author.

Symons (G. J.) British Rainfall, 1869. On the Distribution of Rain over the British Isles during the year 1869. 8vo. *London* 1870. Monthly Meteorological Magazine. Nos. 46, 50, 51. 8vo. *London* 1869–70. Report of the Rainfall Committee for the year 1868–69. The Author.

May 12, 1870.

Dr. WILLIAM ALLEN MILLER, Treasurer and Vice-President, in the Chair.

The Croonian Lecture, by Augustus Waller, M.D., F.R.S., of Geneva, "On the Results of the Method introduced by the Author of investigating the Nervous System, more especially as applied to the elucidation of the Functions of the Pneumogastric and Sympathetic Nerves." Received May 12, 1870.

(Abstract.)

Dr. Waller commenced by stating that he had been long engaged in the investigation of the nervous system by means of the method which he introduced many years ago. After drawing attention to the importance of the functions of the nervous system as the seat of all the higher faculties which distinguish animals from plants and man from the lower animals, he referred briefly to the general constitution and intimate structure of the nervous system. It is known that after a nerve has been disconnected from the central organs, its medullary part undergoes a series of changes. The tubular medulla, or white substance, is disintegrated and finally converted into dark granular matter. On this alteration the author founded his method of investigation, as it enables the inquirer to distinguish the altered from the sound fibres at any point of their course. Dr. Waller soon applied his method to the study of the sympathetic nerve, and was enabled thereby to clear up a great part of the mystery which hung over the origin and functions of this nerve—a nerve which supplies and presides over some of the most important organs in the body, the liver, the intestines, the womb, and especially the blood-vessels.

In this manner, while associated with Dr. Budge, the author determined the part of the spinal cord termed by them the cilio-spinal region, which, through the part of the sympathetic connected with it, governs the dilating fibres of the iris. In the hands of Prof. C. Bernard, Brown-Sequard, Dr. Waller, and others the results obtained in this inquiry have shown the relation of the spinal cord to the important functions which the sympathetic nerve exercises in regulating the supply of blood in the vessels and, as a consequence, in controlling the general nutrition and temperature of the body.

Dr. Waller next applied his method to the elucidation of the functions of the ganglions or swellings found on the origin of many nerves.

On dividing the roots of the spinal nerves, it was found, after a certain lapse of time, that on the posterior root, which is alone possessed of a ganglion, the central segment remaining in connexion with the spinal cord became disorganized and its elements passed into a state of granular degeneration; whereas in the distal segment remaining in connexion with the gan-

glion the nervous elements retained all their normal structure, evidently showing that continuity with the spinal cord does not prevent it from becoming disorganized, whereas its connexion with the intervertebral ganglion suffices to preserve its integrity of structure.

In the divided anterior root the phenomenon takes place in an exactly inverse manner from the former. For in this instance the central portion connected with the spinal cord retains its normal structure, while the distal part becomes disorganized and reduced to a granular state. We therefore arrive at this conclusion, viz. that the spinal cord confers on the anterior root that unknown vital power whereby its elements resist granular disorganization; whereas for the posterior root, on the contrary, this preservative power is no longer an attribute of the spinal cord, but resides in the ganglion.

The author pointed out the important bearings these results had on pathology—that henceforth in diseases of the spinal cord and of the brain, we had to endeavour in our pathological examinations of these parts to ascertain in each case how far the alterations could be referred to the separation of a part from its trophic centre.

Dr. Waller then referred to his researches on the Pneumogastric and Spinal Accessory Nerves.

If, from among the various nerves of the human body, we were called upon to point out that which has most exercised the patience and ingenuity of anatomists and physiologists, we should at once indicate the vagus. Its distribution to the larynx, the lungs, the heart, and the stomach shows us at a glance the important nature of its functions. At its origin it is formed by roots springing from the medulla oblongata, to which is added afterwards a considerable branch from the accessorius, which joins and mingles with the pure vagus with which its fibres become intimately blended. The problem to be solved, therefore, is the precise functions of each or of either (*i. e.* the accessorius or pure vagus) before their anastomosis.

In ordinary circumstances nothing would be more simple than to uncover the nerves and to galvanize each separately, as in the case of an ordinary spinal pair. But here the case is different. In their origin these nerves are so close to the medulla oblongata and the blood-vessels that any such operation is quickly fatal, and the irritation of the minute roots of each nerve in close proximity renders it impossible to obtain any precise results. Professor Bischoff's attempts at division of the roots of the accessorius in the vertebral canal rendered it probable that it gave motor fibres to the vagus which were distributed to the larynx. So far the previsions of Sir Charles Bell were confirmed, who compared the internal branch to the anterior or motor part of a spinal pair, and the true vagus with its ganglion to the posterior root. Professor C. Bernard had, however, succeeded in entirely destroying the power of the accessorius by evulsion of its roots, and had arrived at the conclusion that all the fibres of this nerve are distributed to the laryngeal muscles whose functions are connected with the pro-

duction of vocal sounds, while other fibres from the pure vagus govern certain nutritive or organic functions connected with respiration.

In order to separate the functions of the one from the other, we require to destroy all the fibres of the accessorius and leave the others intact, which has been done most effectually by Dr. Waller's process; first disconnecting the accessorius from the medulla, on Bernard's plan, and afterwards allowing the animal to live sufficiently long for the fatty degeneration to take place, or about seven or eight days. The vagus then being galvanized at every part of its length, it is found impossible to affect either the action of the heart or the stomach, and the only result is to cause slight movements of the larynx.

It is therefore evident that Sir Charles Bell's ideas respecting this nerve are in a great measure demonstrated by this experiment; the only exception being with regard to certain fibres of a motor nature distributed to the larynx, which it may be surmised are derived from some anastomotic source, and therefore not contained in the vagus at its origin. Dr. Waller referred to the recent researches on this subject by Professor Vulpian, MM. Jolliet, Schiff, and Heidenhain, who have confirmed the results above stated.

The Lecturer then proceeded to his observations on the pneumogastric and sympathetic nerves on man in health and in certain affections of the nervous system.

He was first induced to undertake this subject on account of the unsatisfactory results obtained by galvanizing this nerve and the cervical sympathetic on man. In man this operation is frequently resorted to by medical men, but in no case has any one asserted that any of the known symptoms of irritation of those nerves, such as stoppage of the heart's action, dilatation of the pupil or contraction of the vessels, have been produced. The inference is that it is erroneous to suppose that they were in any degree affected by galvanism. By means of mechanical irritation applied over these nerves in the neck, Dr. Waller, in 1862, found that most of the known effects of their irritation, such as dilatation of the pupil &c., can be induced. The principal effects thus induced are nausea, tenderness, or oppression over the præcordia, and stoppage of the heart's action more or less complete; dilatation of the pupil of the same side, and fall of temperature of the cheek and ear, amounting to 2° or 3° Centigrade, as ascertained by one of Geissler's delicate thermometers. All these effects correspond to those produced by galvanism on the denuded nerves. By means of the mechanical irritation of the pneumogastric in cases of vomiting, the vomiting has been instantly stopped, sometimes returning again immediately the irritation was removed; at other times a permanent relief was procured.

He lastly referred to the effects of collapse and syncope produced by the irritation of these nerves. This effect was well known to Aristotle, who attributes it to the compression of the veins, and describes the effects very accurately in the following passage:—

2 D 2

"ὧν ἐπιλαμβανομένων ἐνίοτε ἔξωθεν, ἄνευ πνιγμοῦ καταπίπτουσιν οἱ ἄνθρωποι, μετ' ἀναισθησίας τὰ βλέφαρα συμβεβληκότες."

Dr. Waller has frequently observed the same symptoms, viz. the sudden collapse and temporary insensibility; but in general the effects are confined to a state of depression more or less strong, which may be moderated by graduating the degree of irritation applied. He believes that this fact may be taken advantage of and applied as a means of inducing asthenia or debility for the purpose of facilitating certain operations in surgery, such as the reducing of fractures or even hernia, in lieu of the administration of other anæsthetics, such as chloroform or tobacco, which present a certain degree of danger not attending the compression of the vagus. In confirmation of this idea, he cites a case of reduction of the shoulder-joint in this manner:—

C., a journeyman baker of powerful athletic frame, dislocated the head of the humerus beneath the clavicle by a fall down stairs. While the man was lying on the bed some unavailing attempts were made to reduce the luxation by Dr. Waller himself, Dr. Prevost, and Dr. Julliard. Dr. Julliard, whose patient he was, sent for some chloroform to facilitate the operation by inducing anæsthesia. In the meantime Dr. Waller proposed to make another attempt with the assistance of the compression of the vagus. After removing the pillows at the head and arranging the patient more comfortably, Dr. Waller stood at the head of the bed to apply compression on both sides, while Dr. Julliard and Dr. Prevost performed extension and counter extension. At the end of about two or three minutes, just when the carotids had ceased to be felt beating beneath the fingers, a sudden click indicated the return of the bone into its cavity.

The Lecturer concluded with the following words:—"In terminating his lecture, I cannot refrain from urging on your attention that, if much has been already accomplished by means of this method, there still remains a vast field of inquiry unexplored before us. The nervous system, central and peripheral, is an immense and intricate series of nerve-tubes and of ganglion-cells, and by the method I have laid before you we have already recognized in these elements a great degree of mutual dependence. Within these limits are contained the organic substratum of all that is most noble in our being, of all that elevates the animal above the plant, and that gives man preeminence over the animal. No one can doubt the importance of a thorough knowledge of this system for the efficient treatment of the diseases that affect it. And it may be reasonably hoped that the full development of the method here especially referred to, combined with other modes of investigation, will materially contribute to gain for us a greater insight into the nature of this so highly endowed part of our organism.

"Much that is at present required is a combined and methodical application of the powers and knowledge which we possess; something, in fact, resembling that which has been done by mapping out the surface of our satellite in separate small compartments, each of which is assigned to a

different observer. I cannot help entertaining the hope that something of the sort will sooner or later be undertaken with regard to the investigation of the whole nervous system."

May 19, 1870.

General Sir EDWARD SABINE, K.C.B., President, in the Chair.

The following communications were read :—

I. "A Ninth Memoir on Quantics." By Prof. CAYLEY, F.R.S. Received April 7, 1870.

(Abstract.)

It was shown not long ago by Prof. Gordan that the number of the irreducible covariants of a binary quantic of any order is finite (see his memoir "Beweis das jede Covariante und Invariante einer binären Form eine ganze Function mit numerischen Coefficienten einer endlichen Anzahl solcher Formen ist," Crelle, t. 69 (1869), Memoir dated 8 June 1868), and in particular that for a binary quantic the number of irreducible covariants (including the quantic and the invariants) is $=23$, and that for a binary sextic the number is $=26$. From the theory given in my "Second Memoir on Quantics," Phil. Trans. 1856, I derived the conclusion, which as it now appears was erroneous, that for a binary quintic the number of irreducible covariants was infinite. The theory requires, in fact, a modification, by reason that certain linear relations, which I had assumed to be independent, are really not independent, but, on the contrary, linearly connected together: the interconnexion in question does not occur in regard to the quadric, cubic, or quartic; and for these cases respectively the theory is true as it stands; for the quintic the interconnexion first presents itself in regard to the degree 8 in the coefficients, and order 14 in the variables; viz. the theory gives correctly the number of covariants of any degree not exceeding 7, and also those of the degree 8, and order less than 14; but for the order 14 the theory as it stands gives a non-existent irreducible covariant $(a, \ldots)^8(x, y)^{14}$; viz. we have, according to the theory, $5=(10-6)+1$, that is, of the form in question there are 10 composite covariants connected by 6 syzygies, and therefore equivalent to $10-6,=4$ asyzygetic covariants; but the number of asyzygetic covariants being $=5$, there is left, according to the theory, 1 irreducible covariant of the form in question. The fact is that the 6 syzygies being interconnected and equivalent to 5 independent syzygies only, the composite covariants are equivalent to $10-5,=5$, the full number of the asyzygetic covariants. And similarly the theory as it stands gives a non-existent irreducible covariant $(a, \ldots)^8(x, y)^{20}$. The theory being thus in error, by reason that it

omits to take account of the interconnexion of the syzygies, there is no difficulty in conceiving that the effect is the introduction of an infinite series of non-existent irreducible covariants, which, when the error is corrected, will disappear, and there will be left only a finite series of irreducible covariants.

Although I am not able to make this correction in a general manner so as to show from the theory that the number of the irreducible covariants is finite, and so to present the theory in a complete form, it nevertheless appears that the theory can be made to accord with the facts; and I reproduce the theory, as well to show that this is so as to exhibit certain new formulæ which appear to me to place the theory in its true light. I remark that although I have in my second memoir considered the question of finding the number of irreducible covariants of a given degree θ in the coefficients but of any order whatever in the variables, the better course is to separate these according to their order in the variables, and so consider the question of finding the number of the irreducible covariants of a given degree θ in the coefficients, and of a given order μ in the variables. (This is, of course, what has to be done for the enumeration of the irreducible covariants of a given quantic; and what is done completely for the quadric, the cubic, and the quartic, and for the quintic up to the degree 6 in my Eighth Memoir (Phil. Trans. 1867). The new formulæ exhibit this separation; thus (Second Memoir, No. 49), writing a instead of x, we have for the quadric the expression $\dfrac{1}{(1-a)(1-a^2)}$, showing that we have irreducible covariants of the degrees 1 and 2 respectively, viz. the quadric itself and the discriminant: the new expression is $\dfrac{1}{(1-ax^2)(1-a^2)}$, showing that the covariants in question are of the actual forms $(a,\ldots\between x,y)^2$ and $(a,\ldots)^2$ respectively. Similarly for the cubic, instead of the expression No. 55, $\dfrac{1-a^6}{(1-a)(1-a^2)(1-a^3)(1-a^4)}$, we have $\dfrac{1-a^6x^6}{(1-ax^3)(1-a^2x^2)(1-a^3x^3)(1-a^4)}$, exhibiting the irreducible covariants of the forms $(a,\ldots\between x,y)^3$, $(a,\ldots)^2(x,y)^2$, $(a\ldots)^3(x,y)^3$, and $(a,\ldots)^4$, connected by a syzygy of the form $(a,\ldots)^6(x,y)^6$; and the like for quantics of a higher order.

In the present Ninth Memoir I give the last-mentioned formulæ; I carry on the theory of the quintic, extending the Table No. 82 of the Eighth Memoir up to the degree 8, calculating all the syzygies, and thus establishing the interconnexions, in virtue of which it appears that there are really no irreducible covariants of the forms $(a,\ldots)^8(x,y)^{14}$, and $(a,\ldots^5\between x,y)^{20}$. I reproduce in part Prof. Gordan's theory so far as it applies to the quintic; and I give the expressions of such of the 23 covariants as are not given in my former memoirs; these last were calculated

for me by Mr. W. Barrett Davis, by the aid of a grant from the Donation Fund at the disposal of the Royal Society. The paragraphs of the present memoir are numbered consecutively with those of the former memoirs on Quantics.

II. "On the Cause and Theoretic Value of the Resistance of Flexure in Beams." By W. H. Barlow, F.R.S. Received April 13, 1870.

(Abstract.)

The author refers to his previous papers, read in 1855 and 1857, wherein he described experiments showing the existence of an element of strength in beams, which varied with the degree of flexure, and acts in addition to the resistance of tension and compression of the longitudinal fibres. It was pointed out that the ratio of the actual strength of solid rectangular beams to the strength as computed by the theory of Liebnitz is,

In cast iron, as about $2\frac{1}{4}$ to 1.
In wrought iron as $1\frac{5}{8}$ and $1\frac{3}{4}$ to 1.
And in steel, as $1\frac{5}{8}$ and $1\frac{3}{4}$ to 1.

The theory of Liebnitz assumes a beam to be composed of longitudinal fibres only, contiguous, but unconnected, and exercising no mutual lateral action. But it is remarked that a beam so constituted would possess no power to resist transverse stress, and would only have the properties of a rope.

Cast iron and steel contain no actual fibre, and wrought iron (although some qualities are fibrous) is able to resist strain nearly equally in any direction.

The idea of fibre is convenient as facilitating investigation; but the word fibre, as applied to a homogenous elastic solid, must not be understood as meaning filaments of the material. In effect it represents lines of direction, in which the action of forces can be ascertained and measured; for in torsion-shearing and "*angular deformation*" the fibres are treated by former writers as being at the angle of 45°, because it has been shown that the diagonal resistances have their greatest manifestation at that angle.

Elastic solids being admitted to possess powers of resistance in the direction of the diagonals, attention is called to omission of the effect of resistance in the theory of beams.

The author then states, as the result of his investigation, that compression and extension of the diagonal fibres constitute an element of strength equal to that of the longitudinal fibres, and that *flexure* is the consequence of the relative extensions and compressions in the direct and diagonal fibres, arising out of the amount, position, and direction of applied forces.

Pursuing the subject, it is shown that certain normal relations subsist

between the strains of direct fibres and their relative diagonals, evenly distributed strain being that in which the strain in the direct fibres is accompanied by half the amount of strain in the relative diagonal fibres.

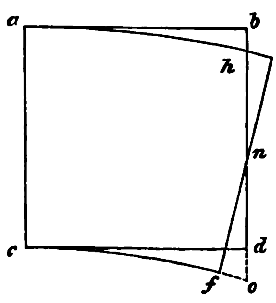
Any disturbance of this relation indicates the presence of another force. Thus tensile forces applied at *right angles* to compressive forces of equal amount, produce no strain in the diagonals. But if forces applied at right angles to each other are both tensile, or both compressive, the strain in the diagonal is as great as that in the direct fibres.

It is also pointed out that in a given fibre *a b c*, the point *b* may be moved with regard to *a* and *c*, thus producing plus and minus strains in the same fibre.

Treating a solid as being made up of a series of laminæ, and showing that every change of figure can be represented by the variation in length of the diagonals, taken in connexion with those of the direct fibres, the author proceeds to trace the effects of the application of tensile and compressive forces acting longitudinally on either side of the neutral plane, and shows that curvature is the result of the relation between the strains in direct fibres and those in the diagonals.

The operation of a single tensile force applied along one side of the plate and a transverse stress are likewise traced out, and the conditions of "*elastic equilibrium*" referred to.

The amount of resistance offered by the diagonal fibres is shown as follows:—



a b c d represents a portion of a beam strained by transverse forces into the circular curve *a e*.

Two resistances arise.

1. That due to the extension and compression of the longitudinal fibres produced by the rotation of *b d* about the neutral axis, which is the resistance considered in the theory of Leibnitz.
2. That due to the extension and compression of the diagonal fibres, caused by the deformation of the square *a b c d* into the figure *a h o c*, which is the resistance of flexure.

It is then shown that in a solid rectangular beam, the second resistance is equal to the first, and that both resistances act independently, and

ısequently that the true theoretic resistance of a solid rectangular beam is ıctly twice that arrived at by the theory of Leibnitz,

The strength so computed is in general accordance with the results of periments in cast iron, wrought iron, steel, and other materials, the ıximum strength being found in cast iron, which is one-eighth above, d the minimum in glass, which is one-fourth below the calculated ength.

The author considers this treatment of the subject as arising necessarily t of Dr. Hook's law "ut tensio sic vis," and that it is in effect com- :ting the application of those principles which were only partially ap- .ed by Leibnitz.

The paper concludes with some practical illustrations (accompanied by ,otographs) of the effect of diagonal action.

The appendix contains the results of experiments on the tensile, com- essive, and transverse resistances of steel.

I. "On Deep-sea Thermometers." By Staff-Commander JOHN E. DAVIS, R.N. Communicated by Capt. RICHARDS, R.N., Hydrographer of the Admiralty. Received April 18, 1870.

(Abstract.)

The results of thermometric observations at great depths in the ocean ıt being of a satisfactory nature, the attention of the Hydrographer the Navy was directed to the defects in the construction of the Six's lf-registering thermometers then in use, and also to the want of know- lge of the effects of compression on the bulb ; and as it was known that delicate thermometer was affected *in vacuo*, it was natural to suppose at an opposite effect would be had by placing them under pressure, and ırticularly such as they would be subjected to at great depths.

Several thermometers, of a superior construction, were made by different akers, and permission was granted to make experiments by pressure in an ydraulic press ; but much delay was caused by not being able to obtain a ess suitable to the requirements, until Mr. Casella, the optician, had a sting-apparatus constructed at his own expense, and the experiments :re commenced.

Previous to the experiments being made, Dr. W. A Miller, V.P.R.S., oposed, or rather revived, a mode of protecting the bulb from compres-)n by encasing the full bulb in glass, the space between the case and e bulb being nearly filled with alcohol*.

A wrought-iron bottle had been made to contain a thermometer, for the ırpose of comparison with those subjected to compression ; but it failed, ıd finally burst under great compression ; it proved, however, of but :tle consequence, as those designed by Dr. Miller showed so little dif- rence under pressure that they were at once accepted as standards.

* *Vide* Proceedings of the Royal Society, vol. xvii. No. 113, June 17, 1869.

Two series of experiments were then most carefully made, at pressures equal to depths of 250, 500, 750, &c. to 2500 fathoms, the results of which satisfactorily proved that the strongest-made unprotected thermometers were liable to considerable error, and therefore that all previous observations made with such instruments were incorrect.

Experiments were also made in the testing-apparatus with Sir Wm. Thomson's enclosed thermometers, to ascertain the calorific effect produced by the sudden compression of water, in order to find what error, if any, was due to compression in the Miller pattern: an error was proved to exist, but small, amounting to no more than 1°·4 under a pressure of 3 tons to the square inch.

The dredging cruize of the 'Porcupine' afforded an opportunity of comparing the results of the experiments made in the hydraulic testing-apparatus, with actual observation in the ocean, and a most careful series of observations were obtained by Staff-Commander E. K. Calver at depths corresponding to the pressure applied in the testing-apparatus; the result was that, although there was a difference in the curves drawn from the two modes of observation, still the general effect was the same, and the *means* of the two were identical.

From these experiments and observations a scale has been made by which observations made by thermometers of similar construction to those with unprotected bulbs can be corrected and utilized, while it is proposed that by means of observations made with the Miller pattern in the positions and at the same depths at which observations have been made with instruments not now procurable for actual experiment, to form a scale for correcting all observations made with that particular type.

In conclusion, it is suggested that to avoid error from the unsatisfactory working of the steel indices, which, from mechanical difficulties in their construction, cannot always be depended on, two instruments should be sent down for every observation; and although their occasional disagreement of record may raise a doubt, a little experience will enable the observer to detect the faulty indicator, while their agreement will create confidence.

A description of such deep-sea metallic thermometers as have been invented is appended.

IV. "On the Chemical Activity of Nitrates." By Edmund J. Mills, D.Sc. Communicated by Prof. A. W. Williamson. Received April 21, 1870.

(Abstract.)

In the course of his researches upon nitro-compounds, the author found it extremely desirable to submit the genetic relations of those bodies to a detailed examination; in other words, to trace the modifications undergone

by nitryl as it is transmitted (from the chloride, hydrate, or free radical) through an adequate succession of combinations. One of the first steps in this direction is the preparation of nitrylic chloride, which can be most easily effected, according to a statement in Watts's 'Dictionary of Chemistry' *, by the action of phosphoric oxychloride on plumbic nitrate—

$$3\,Pb(NO_3)_2 + 2\,PO\,Cl_3 = Pb_3(PO_4)_2 + 6\,NO_2Cl.$$

Among other modes of verifying this equation, the examination of the residue left behind when excess of the oxychloride is heated with plumbic nitrate, and then distilled off in a current of dry air, appeared the most simple and obvious. The results were found not to agree with the equation; and after three nitrates had been tried, a law of chemical activity became evident, rendering the reaction worthy of pursuit for its own sake, although, as an available source of nitrylic chloride, it had failed entirely. The nature and mode of establishment of this law constitute the subjects of the author's memoir.

When a nitrate is treated with phosphoric oxychloride, as has just been mentioned, the residue contains phosphoric oxide and a metallic chloride. Within the limits of experimental error, or subject to other satisfactory explanation, the ratio between these two products is constant for each nitrate; and from that ratio a quotient a can be found as follows :—

$$a = \frac{\dfrac{\text{weight of chlorine}}{\text{Cl}}}{\dfrac{\text{weight of phosphoric oxide}}{P_2O_5}} = \frac{\text{weight of chlorine}}{\text{weight of phosphoric oxide}} \times 4{\cdot}06.$$

This quotient, which is different for each nitrate, is termed the "coefficient of chemical activity" of nitrates, and the method of obtaining it is designated the "method of ratios." The data from which a is deduced, namely, certain weights of argentic chloride and magnesic pyrophosphate, are, if singly considered, new with each experiment; they depend on time, rate of heating, the state of division of the nitrate, and other conditions. But, assuming the results to have been brought about under a law of chemical action, the values of a must be independent of those circumstances, by which the primitive numerator and denominator could have been only *pari passu* affected; they are related only to the actual occurrence of the reaction. This property, in a chemical ratio, has not, it is believed, been previously observed.

After describing the means employed for obtaining a current of dry air, the apparatus required for the reaction, and the individual experiments which were severally made, the following Table of results is given, Σ being the symbolic value of a nitrate, and $Q = \frac{\Sigma}{a}$.

* Vol. iv. p. 77.

	a	Σ	Q
{ Thallous nitrate	8·76	265·30	30·29
{ Argentic nitrate	5·48	169·94	31·01
{ Plumbic nitrate	5·17	165·56	32·02
Rubidic nitrate	2·38	147·40	61·93
Cæsic nitrate	2·21	195·01	88·24
{ Potassic nitrate	1·99	101·14	50·82
{ Sodic nitrate	1·70	85·05	50·03
Lithic nitrate	1·61	69·00	42·86

The above list probably contains all the metallic nitrates that can be completely dried, excepting nitrates derived from amines and amides, which, in the present state of our knowledge of the phosphamides, it was evidently advisable to exclude.

In the silver group, the mean value of Q is 31·11; and the following equation may be accepted therefore:—

$$a=\frac{\Sigma}{31{\cdot}11}.$$

In the potassium group we have likewise

$$a=\frac{\Sigma}{50{\cdot}42}.$$

Hence, within each set of nitrates, chemical activity is in direct proportion to symbolic value. It is further sufficiently apparent that (excepting rubidic nitrate) a and Σ increase and diminish in the same general order. Within the limits of error, the Q column is an incomplete arithmetical series, the most probable value of whose first term is 6·258, so that

$$Q=m\,6{\cdot}258,$$

m being integral. Reasons are then adduced for identifying the number 6·25 with Dulong and Petit's constant of specific heat. Moreover, since the product of specific heat and symbolic value is, generally, n 6·25, and m is greater than n, taking $m=xn$ and $s=$the specific heat of a nitrate, we have

$$Q=xn\,6{\cdot}25,$$

but

$$\Sigma s=n\,6{\cdot}25;$$

$$\therefore\ Q=x\Sigma s,$$

and

$$a=\frac{\Sigma}{Q}=\frac{\Sigma}{x\Sigma s}=\frac{1}{xs},$$

the expression for chemical activity in terms of specific heat. Comparing the coefficients (a, a') for any two nitrates, the following relations are obtained:—

$$\frac{a}{a'}=\frac{m'}{m}\cdot\frac{\Sigma}{\Sigma'}=\frac{x's'}{xs};$$

and it is shown that these formulæ agree sufficiently well with experiment. Where $m=m'$ and $x=x'$, we have the simple expression

$$\frac{a}{a'}=\frac{\Sigma}{\Sigma'}=\frac{s'}{s}.$$

The values of Q are strictly *equivalent* to each other in point of activity. The author believes that a is commensurate with the elective function of chemical attraction, first discovered by Bergman. He terminates the memoir with a reference to some well-known instances of chemical action (such as that of argentic nitrate on a mixture of aqueous potassic chloride, bromide, and iodide), as serving to bestow a presumptive generality on his principal conclusions.

V. "On the relative Duration of the Component Parts of the Radial Sphygmograph Trace in Health." By A. H. Garrod, of St. John's College, Cambridge. Communicated by Dr. Garrod. Received April 23, 1870.

The graphic method of representing the various phenomena occurring in the body during life, which has been so much developed by MM. Marey and Chauveau of Paris, has placed within our reach great facilities for obtaining an accurate knowledge of the relations, in point of time, of mutually dependent physiological events, and the sphygmograph has become, among others, an instrument familiar to most interested in science.

By means of this instrument, a detailed and truthful record can be easily obtained of the modifications in the diameter of any superficial artery, and, as usually constructed, it is intended to be applied to the radial at the wrist.

The traces to be referred to were taken with one of Marey's instruments, as made by Breguet. The recording paper ran its whole length, $4\frac{3}{8}$ inches, in seven seconds, and thus, by counting the number of pulse-beats in each trace, and multiplying the number thus obtained by 8·57143, the rate of the pulse at the time the trace was taken was easily found.

The lever-pen was of thin steel, sharply pointed, and it recorded by scratching on highly-polished paper previously smoked.

It is now generally agreed that in each pulsation of the radial sphygmograph trace, the main rise is the effect of the contracting ventricle sending blood into, and thus filling, the arterial system.

This rise is followed by a continuous fall when the pulse is quick, but when slow, its continuity is interrupted by a slight undulation, convex upwards.

The major fall is followed by a secondary rise, not so considerable as the main one, but more marked than any other, and this secondary rise is evidently due to the closure of the aortic valves preventing further flow of blood heartwards.

The two points therefore, the commencement of the primary and of the secondary rise, may be considered to mark the beginning of the systole of the heart, and the closure of the aortic valve respectively, as far as they influence the artery at the wrist; and the interval between these two events may be called the *first* part of the arterial sphygmograph trace,

while the interval between the beginning of the secondary rise and that of the succeeding primary one constitutes the second part of the same trace.

In 1865, Prof. Donders* published the results of experiments to determine the relative duration of the first and second part of the cardiac revolution with different rapidities of movements of the heart, taking as his data the commencement of the first and second sounds respectively, and he came to the conclusion that, though the second part varied with the rapidity, the first part was almost constant in all cases.

On commencing work with the sphygmograph, the author came to the same conclusion with regard to the trace at the wrist, but, on improving his methods of observation, he has arrived at a different result.

The best means of insuring an accurate measurement of any sphygmograph trace is to project all the points desired to be compared on to one straight line, and this is done by fixing the trace on to a piece of board, which has another pointed lever attached to it, with relations similar to those of the lever and recording apparatus in the original instrument. By this means lines can be scratched on the trace similar to those which would be produced by the instrument itself if the watch-work were not moving, and a result, as shown in Plate II. fig. 1, can be easily produced.

The reason why this means has to be employed is, because the lever in the sphygmograph moves in part of a circle, not directly up and down.

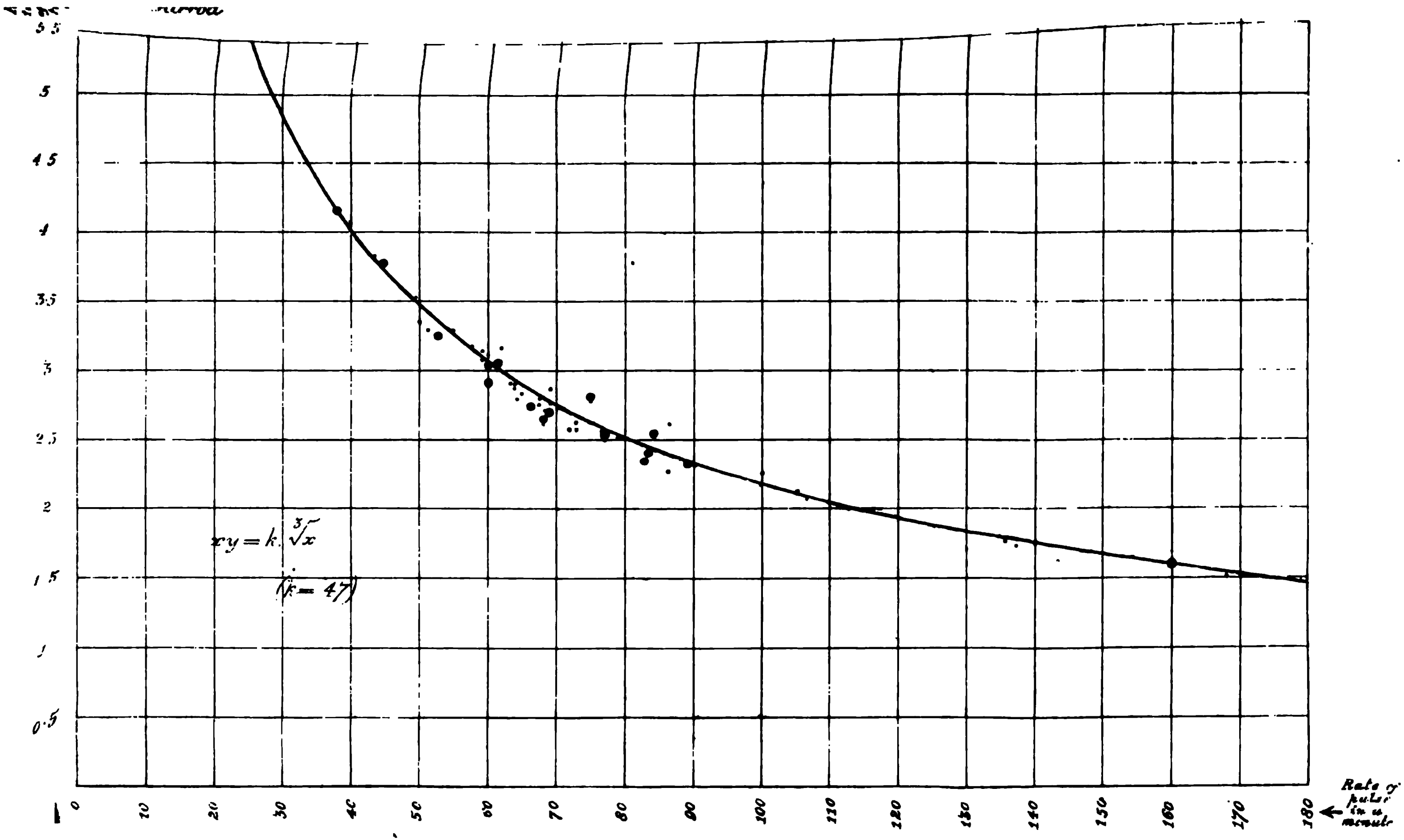
The ratio between the length of the first part of each pulse-beat in a trace and that of the whole beat was measured with a small pair of compasses, and from these the average was obtained, which thus eliminated, in a great degree, the variations produced by the respiratory movements, and also some of the clock-work imperfections.

For example, in fig. 1, the ratios in the several beats are :—

1 : 1·8
: 1·725
: 1·725
: 1·775
: 1·725
: 1·7
: 1·725
: 1·775
: 1·8
: 1·775
: 1·675
: 1·75
: 1·75
: 1·725

with an average of 1 : 1·7443.

* "On the Rhythm of the Sounds of the Heart." By F. C. Donders. Translated in the 'Dublin Quarterly Journal of Medical Science,' Feb. 1868, from the 'Nederlandisch Archief voor Genees- en Natuurkunde,' Utrecht, 1865.



5.5
5
4.5
4
3.5
3
2.5
2
1.5
1
0.5
0
10
20
30
40
50
60
70
80
90
100
110
120
130
140
150
160
170
180
Rate of pulse in a minute
$xy = k\sqrt[3]{x}$
($k = 47$)

Again, in fig. 2, the ratios are:—

$$\begin{aligned} 1 &: 3{\cdot}8 \\ &: 3{\cdot}775 \\ &: 3{\cdot}8 \\ &: 3{\cdot}825 \end{aligned}$$

with an average of 1 : 3·8.

Calling the rate of the pulse x, and the number of times the *first* part is contained in the whole beat y, xy equals the number of times that the *first* part is contained in a minute, and $\frac{1}{xy}$ equals the part of a minute occupied by the *first* part of each pulse-beat.

From several observations, it was found that xy increases with x, not directly as it, but as its cube root, consequently the following equation finds xy in terms of x,

$$xy = k\sqrt[3]{x},$$

k being a constant, equal to 47 (about).

For instance, in fig. 1, $x=137$, $y=1{\cdot}7443$;

and in fig. 2, $x=44$, $y=3{\cdot}8$;

and $137\times 1{\cdot}7443=238{\cdot}9691$,

$44\times 3{\cdot}8=167{\cdot}2$;

and $238{\cdot}9691 : 167{\cdot}2 :: 1{\cdot}43 : 1$,

and $\sqrt[3]{137} : \sqrt[3]{44} ::$

$=5{\cdot}155 : 3{\cdot}54 :: 1{\cdot}456 : 1$,

which shows that in these individual cases xy varies, within the limits of experimental error, as the cube root of x.

If this statement of the ratio of the *first* part of the trace to the whole beat is a correct one, a knowledge of the rapidity of the pulse alone is sufficient to enable the length of the *first* part to be found by multiplying the cube root of the rapidity by the constant quantity 47.

Thus, supposing the pulse beats 64 times in a minute, the cube root of 64 being 4, $4\times 47=188$, and the length of the first part of the beat ought to be $\frac{1}{188}$ of a minute. In one case with $x=64$, xy was found to be 185·75, and in another with $x=63{\cdot}5$, $xy=181{\cdot}77$, both numbers which agree closely with the requirements of the equation.

With $x=140$, and therefore $\sqrt[3]{x}=5{\cdot}2$,

$$5{\cdot}2\times 47=244{\cdot}4;$$

and therefore the first part $=\frac{1}{244}-\frac{1}{245}$ of a minute; in a pulse of that rapidity xy was found $=242{\cdot}9$.

To save the trouble of extracting the cube root for any rapidity, these facts have been thrown into a coordinate form in the accompanying Table, and the observations on which the formula is based are represented by dots on their proper coordinates, the calculated curve, with $k=47$, being represented by a continuous line.

Since the above equation was worked out, a great many other observa-

tions have been made, several of which are recorded on the Table, and in health no cases have been found which depart from the curve more than those indicated on it.

The observations made on the author are represented by simple black dots, those made on others are encircled by a ring; great size of a dot indicates that more than one independent observation has produced exactly similar results.

In none of the cases have measurements been made after violent exercise. Differences in the height and age of the subjects experimented on have not been found to produce any appreciable effect.

The trace from infants has not been examined.

From the equation $xy=\sqrt[3]{x}\,.\,k$ the length of the *second* part of the pulse trace may be represented in terms of x, as $\frac{k-\sqrt[3]{x^2}}{x\,.\,k}$; and as from the nature of y it cannot be less than unity (no pulse having been seen with two contractions or more between two successive closures of the aortic valve), the limit of cardiac rapidity may be deduced to be 322 in a minute ($k=47$); but it is scarcely probable that pulses of such a rate could remain so sufficiently long to be counted.

In many cases of disease implicating the circulatory system, the equation given above indicates that the duration of the *first* part of the heart's action is not normal; thus, in a boy suffering from typhoid fever, on the second day after the pyrexia had ceased, and when the temperature was below the normal, xy was found $=225\cdot25$, where $x=60$, which differs from the equation

$$\sqrt[3]{67}\times 47=190\cdot82,$$

which shows that the length of the *first* part is considerably too short in the former. In the same case, three days later, the patient rapidly improving, with $x\ =\ 56\cdot5$,

$$xy=\ 188,$$

which is much nearer the calculated normal result, 180·5, than on the former occasion, the trace keeping pace with the other physical changes.

It is probable that many other imperfections in the circulatory system can be similarly indicated, and it has been shown above with what facility a diagnosis may be arrived at.

VI. "Spectroscopic Observations of the Sun."—No. VI. By J. Norman Lockyer. Received April 27, 1870.

The weather lately has been fine enough and the sun high enough during my available observation-time to enable me to resume work.

The crop of new facts is not very large, not so large as it would have been had I been working with a strip of the sun, say fifty miles or a hundred miles wide, instead of one considerably over a thousand—indeed, nearer two thousand in width; but in addition to the new facts obtained, I have

ry largely strengthened my former observations, so that the many hours have spent in watching phenomena, now perfectly familiar to me, have t been absolutely lost.

The negative results which Dr. Frankland and myself have obtained in r laboratory-work in the matter of the yellow bright line, near D, in the ectrum of the chromosphere being a hydrogen line, led me to make a ecial series of observations on that line, with a view of differentiating it, possible, from the line C.

It had been remarked, some time ago, by Professor Zollner, that the llow line was often less high in a prominence than the C line; this, hower, is no evidence (bearing in mind our results with regard to magneum). The proofs I have now to lay before the Royal Society are of a fferent order, and are, I take it, conclusive :—

1. With a tangential slit I have seen the yellow line bright below the romosphere, while the C line has been dark ; the two lines being in e same field of view.

2. In the case of a bright prominence over a spot *on the disk*, the C d F lines have been seen bright, while the yellow line has been invisible.

3. In a high-pressure injection of hydrogen, the motion indicated by ange of wave-length has been *less* in the case of the yellow line than in e case of C and F.

4. In a similar quiescent injection the pressure indicated has been less.

5. In one case the C line was seen long and unbroken, while the yellow e was equally long, but *broken*.

The circumstance that this line is so rarely seen dark upon the sun ikes me suspect a connexion between it and the line at 5015 Ångström, iich is also a bright line, and often is seen bright in the chromosphere, d then higher than the sodium and magnesium lines, when they are ible at the same time ; and the question arises, must we not attribute ese lines to a substance which exists at a higher temperature than those xed with it, and to one of very great levity? for its absorption line nains invisible, as a rule, in spot spectra.

I have been able to make a series of observations on the fine spot which s visible when I commenced them on the 10th instant, not far from the ntre of its path over the disk. At this time, the spot, as I judged by e almost entire absence of indications of general absorption in the penibral regions, was shallow, and this has happened to many of the spots en lately. A few hours' observation showed that it was getting deeper parently, and that the umbræ were enlarging and increasing in number, if a general downsinking were taking place ; but clouds came over, and e observations were interrupted.

By the next day (April 11) the spot had certainly developed, and now ere was a magnificently bright prominence, completely over the darkest ss of umbra, the prominence being fed from the penumbra or very close it, a fact indicated by greater brilliancy than in the bright C and F lines.

April 12. The prominence was persistent.

April 15. Spot nearing the limb, prominence still persistent over spot. At eleven I saw no prominence of importance on the limb, but about an hour afterwards I was absolutely startled by a prominence not, I think, depending upon the spot I have referred to, but certainly near it, more than 2′ high, showing a tremendous motion towards the eye. There were light clouds, which reflected to me the solar spectrum, and I therefore saw the black C line at the same time. The prominence C line (on which changes of wave-length are not so well visible as in the F line) was only coincident with the absorption-line for a few seconds of arc!

Ten minutes afterwards the thickness of the line towards the right was all the indication of motion I got. In another ten minutes the bright and dark lines were coincident.

And shortly afterwards what motion there was was towards the red!

I pointed out to the Royal Society, now more than a year ago *, that the largest prominences, *as seen at any one time*, are not necessarily those in which either the intensest action or the most rapid change is going on. From the observations made on this and the following day, I think that we may divide prominences into two classes :—

1. Those in which great action is going on, lower vapours being injected; in the majority of cases these are not high, they last only a short time—are throbs, and are oft renewed, and are not seen so frequently near the sun's poles as near the equator. They often accompany spots, but are not limited to them. These are the intensely bright prominences of the American photographs.

2. Those which are perfectly tranquil, so far as wave-length evidence goes. They are often high, are persistent, and not very bright. These do not, as a rule, accompany spots. These are the "radiance" and dull prominences shown in the American photographs.

I now return to my observations of the spot. On the 16th the last of the many umbræ was close to the limb, and the most violent action was indicated occasionally. I was working with the C line, and certainly never saw such rapid changes of wave-length before. The motion was chiefly horizontal, or nearly so, and this was probably the reason why, in spite of the great action, the prominences, three or four of which were shut out, never rose very high.

I append some drawings made, at my request, by an artist, Mr. Holiday, who happened to be with me, and who had never seen my instrument or the solar spectrum widely dispersed before. I attach great importance to them, as they are the untrained observations of a keen judge of form.

The appearances were at times extraordinary and new to me. The hydrogen shot out rapidly, scintillating as it went, and suddenly here and there the bright line, broad and badly defined, would be pierced, as it were, by a line of intensely brilliant light parallel to the length of the

* Proc. Roy. Soc. 1869, p. 354, Mar. 17.

spectrum, and at times the whole prominence spectrum was built up of bright lines so arranged, *indicating that the prominence itself was built up of single discharges*, shot out from the region near the limb with a velocity sometimes amounting to 100 miles a second. After this had gone on for a time, the prominence mounted, and the cyclonic motion became evident; for away from the sun, as shown in my sketch, the separate masses were travelling away from the eye; then gradually a background of less luminous hydrogen was formed, moving with various velocities, and on this background the separate "bombs" appeared (I was working with a vertical spectrum) like exquisitely jewelled ear-rings.

It soon became evident that the region of the chromosphere just behind that in which the prominence arose, was being *driven back* with a velocity something like 20 miles a second, the back-rush being so local that with the small image I am unfortunately compelled to use, both the moving and rigid portions were included in the thickness of the slit. I saw the two absorption-lines overlap.

These observations were of great importance to me; for the rapid action enabled me to put together several phenomena I was perfectly familiar with separately, and see their connected meaning.

They may be summarized as follows, and it will be seen that they teach us much concerning the nature of prominences. When the air is perfectly tranquil in the neighbourhood of a large spot, or, indeed, generally in any part of the disk, we see absorption-lines running along the whole length of the spectrum, crossing the Fraunhofer lines, and they vary in depth of shade and breadth according as we have pore, corrugation, or spot under the corresponding part of the slit,—a pore, in fact, is a spot. Here and there, where the spectrum is brightest (where a bright point of facula is under the slit), we suddenly see an interesting bright lozenge of light. This I take to be due to bright hydrogen at a greater pressure than ordinary, and this then is the reason of the intensely bright points seen in ranges of faculæ observed near the limb.

The appearance of this lozenge in the spectroscope, which indicates a diminution of pressure round its central portion, is the signal for some, and often all of the following phenomena:—

1. A thinning and strange variations in the visibility and thickness of the hydrogen absorption-line under observation.

2. The appearance of other lozenges in the same locality.

3. The more or less decided formation of a bright prominence on the disk.

4. If near the limb, this prominence may extend beyond it, and its motion-form will then become more easy of observation. In such cases the motion is cyclonic in the majority of cases, and generally very rapid, and—another feature of a solar storm—the photospheric vapours are torn up with the intensely bright hydrogen, *the number of bright lines visible*

determining the depth from which the vapours are torn, and varying almost directly with the amount of motion indicated.

Here, then, we have, I think, the chain that connects the prominences with the brighter points of the faculæ.

These lozenge-shaped appearances, which were observed close to the spot on the 16th, were accompanied by the "throbs" of the eruption, to which I have before referred; while Mr. Holiday was with me—a space of two hours—there were two outbursts, separated by a state of almost rest, and each outburst consisting of a series of discharges, as I have shown. I subsequently witnessed a third outburst. The phenomena observed on all three were the same in kind.

On this day I was so anxious to watch the various motion-forms of the hydrogen-lines, that I did not use the tangential slit. This I did the next day (the 17th of April) in the same region, when similar eruptions were visible, though the spot was no longer visible.

Judge of my surprise and delight, when upon sweeping along the spectrum, I found HUNDREDS of the Fraunhofer lines beautifully bright at the base of the prominence!!!

The complication of the chromosphere spectrum was greatest in the regions more refrangible than C, from E to long past *b* and near F, and high-pressure iron vapour was one of the chief causes of the phenomenon.

I have before stated to the Royal Society that I have seen the chromosphere full of lines; but the fullness then was as emptiness compared with the observation to which I now refer.

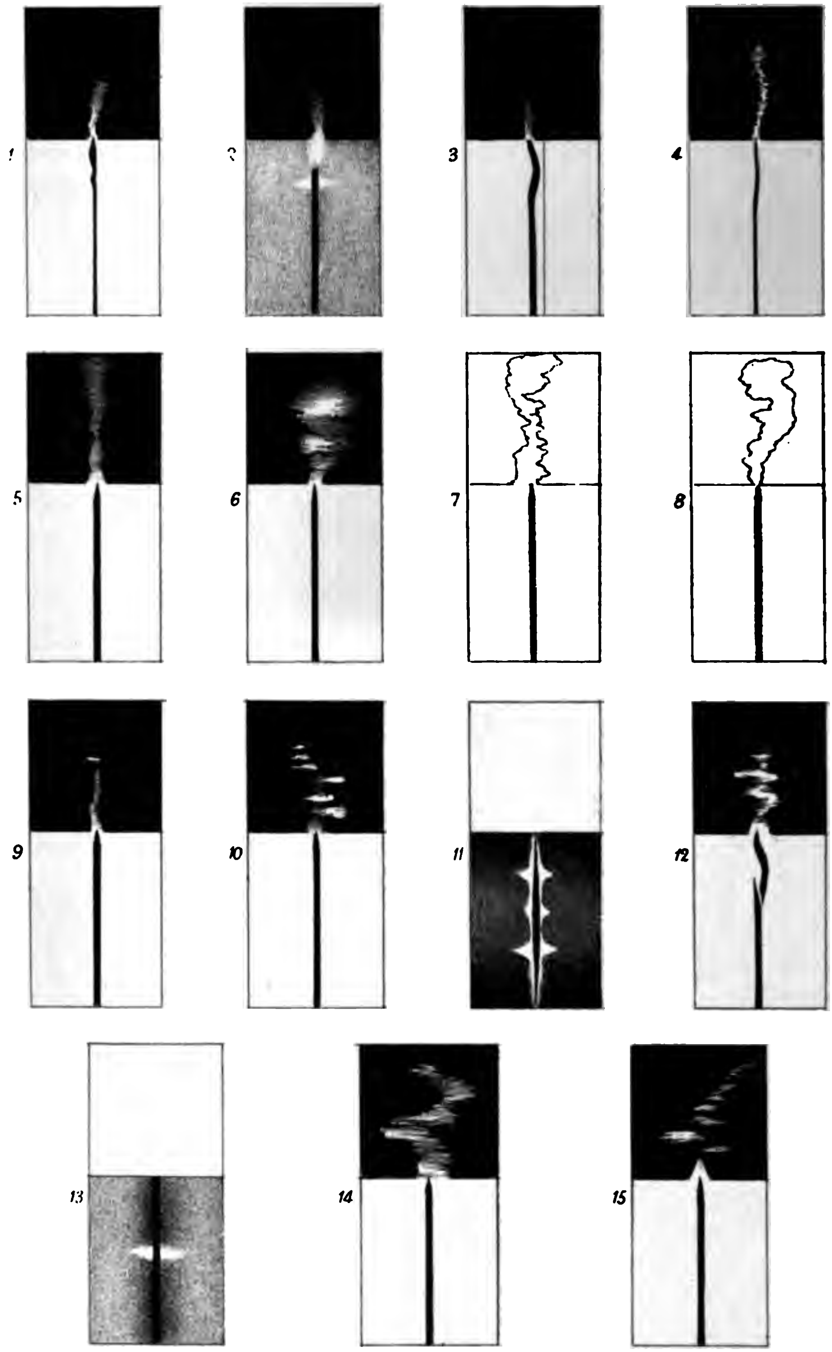
A more convincing proof of the theory of the solar constitution, put forward by Dr. Frankland and myself, could scarcely have been furnished. This observation not only endorses all my former work in this direction, but it tends to show the shallowness of the region on which many of the more important solar phenomena take place, as well as its exact locality.

The appearance of the F line, with a tangential slit at the base of the prominence, included two of the lozenge-shaped brilliant spots to which I have before referred; they were more elongated than usual—an effect of pressure, I hold—greater pressure and therefore greater complication of the chromosphere spectrum. This complication is almost impossible of observation on the disk.

It is noteworthy that in another prominence, on the same side of the sun, although the action was great, the erupted materials were simple, *i.e.* only sodium and magnesium, and that a moderate alteration of wave-length in these vapours was obvious.

Besides these observations on the 17th, I also availed myself of the pureness of the air to telescopically examine the two spots on the disk, which the spectroscope reported tranquil as to up and down rushes. I saw every cloud-dome in their neighbourhood perfectly, and I saw the domes drawn out, by horizontal currents doubtless, in the penumbræ

[redacted]


1
2
3
4
5
6
7
8
9
10
11
12
13
14
15

while on the floors of the spots, here and there, were similar single cloud-masses, the distribution of which varied from time to time, the spectrum of these masses resembling that of their fellows on the general surface of the sun.

I have before stated that the region of a spot comprised by the penumbra appears to be shallower in the spots I have observed lately (we are now near the maximum period of sun spots); I have further to remark that I have evidence that the chromosphere is also shallower than it was in 1868.

I am now making special observations on these two points, as I consider that many important conclusions may be drawn from them.

DESCRIPTION OF PLATE III.

Mr. Holiday's drawings and remarks.

1. Prominence much bent.
2. Prominence encroaching over limb—bright line crossing black line.
3. Black line (F) curved downwards, sometimes nearly touching iron line below.
4.
5. Prominence nearly divided.
6. Intensely brilliant flashes above and below centres (of F lines); the interruptions very complete.

7 & 8. Curves in prominence very marked.

9, 10, 12, 14, 15. My own drawings, made during first and second outbursts.

11. A lozenge on the limb as seen with a tangential slit.

13. A lozenge as seen on the sun itself.

VII. "On some Elementary Principles in Animal Mechanics.—No. IV. On the difference between a Hand and a Foot, as shown by their Flexor tendons." By the Rev. Samuel Haughton, F.R.S., M.D. Dubl., D.C.L. Oxon., Fellow of Trinity College, Dublin. Received April 23, 1870.

The fore feet of vertebrate animals are often used merely as organs of locomotion, like the hind feet; and in the higher mammals they are more or less "cephalized," or appropriated as hands to the use of the brain.

The proper use of a hand when thus specialized in its action, is to grasp objects; while the proper use of a foot is to propel the animal forward by the intervention of the ground.

In the case of the hand, the flexor muscles of the fore arm act upon the finger tendons, in a direction from the muscles towards the tendons, which latter undergo friction at the wrist and other joints of the hand, the force being applied by the muscles to the tendon above the wrist, and the resistance being applied at the extremities of the tendons below the wrist by the object grasped by the hand.

From the principle of "Least Action in Nature" we are entitled to assume the strength of each portion of a tendon to be proportional to the force it is required to transmit; and since, in a proper hand, these forces are continually diminished by friction, as we proceed from the muscle to

the fingers, we should expect the strength of the tendon above the wrist to be greater than the united strengths of all the finger-tendons.

Conversely, in a proper foot, the force is applied by the ground to the extremities of the tendons of the toes, and transmitted to the flexor muscles of the leg, by means of the tendons of the inner ankle, which undergo friction in passing round that and the other joints of the foot. In this case, therefore, we should expect the united strengths of the flexor tendons of the toes to exceed the strength of the flexor tendons above the heel.

In the case of the hand, friction acts against the muscles; in the case of the foot, friction aids the muscles.

I have measured the relative strengths of the deep flexor tendons of the hand above and below the wrist in several animals, and also the relative strengths of the long flexor tendons of the foot above and below the ankle in the following manner:—

I weighed certain lengths of the tendons above the wrist and ankle, and compared these weights with the weights of equal lengths of the flexor tendons of the fingers or toes, assuming that the weights of equal lengths are proportional to their cross sections, and these again proportional to the strengths of the tendons at the place of section. The difference between the weights above and below the joint represents the sum of all the frictions experienced by the tendons between the two points of section.

The following Tables contain the results of my measurements:—

Table I. *Friction of Long Flexor Tendons of Toes.* (Cross section of toe tendons greater than cross section of muscle tendons.)

		Amount of friction. per cent.			Amount of friction. per cent.
1.	Pyrenean Mastiff	65·4	17.	Australian Dinjo	33·8
2.	African Lion	59·0	18.	Japanese Bear	31·7
3.	Common Fox	57·6	19.	Virginian Bear	25·9
4.	African Jabiru	56·8	20.	Common Llama	25·9
5.	American Rhêa	52·4	21.	Hedgehog	25·0
6.	Indian Jackall	49·2	22.	African Ostrich	24·6
7.	American Jaguar	49·2	23.	Common Otter	19·8
8.	New-Zealand Weka Rail	47·5	24.	Man (mean of 5)	16·2
9.	Silver Pheasant	47·4	25.	Spider-Monkey	12·3
10.	Bengal Tiger	46·0	26.	Goat	9·5
11.	Indian Leopard	45·5	27.	One-horned Rhinoceros	9·0
12.	Six-banded Armadillo	44·4	28.	Negro-Monkey	8·0
13.	Three-toed Sloth	42·5	29.	Brahmin Cow	6·8
14.	Black Swan	36·0	30.	Nemestrine Macaque	2·0
15.	Common Hare	36·0	31.	Boomer Kangaroo	0·0
16.	European Wolf	34·0			

The foregoing animals all realize the typical idea of a true foot, with a variable amount of friction at the ankle-joint; this friction disappearing altogether in the Boomer Kangaroo, whose method of progression realizes absolute mechanical perfection, as no force whatever is consumed by the friction of the flexor tendons at the heel.

The only animals whose feet deviated from the typical foot were three, viz. Alligator, Common Porcupine, and Phalanger. In these animals the foot has the mechanical action of a hand, or grasping organ; and the flexor tendons above the ankle exceeded those below the ankle by the following amounts:—

	per cent.
1. Alligator	11·5
2. Common Porcupine	20·0
3. Phalanger	29·2

In the case of the flexor tendons of the hand, I obtained the following results:—

Table II. *Friction of Deep Flexor Tendons of Hand.* (Cross section of muscle tendons greater than cross section of finger tendons.)

	Amount of friction. per cent.		Amount of friction. per cent.
1. Common Porcupine	71·0	8. Negro-Monkey	27·4
2. Sooty Mangaby	49·2	9. Spider-Monkey	26·5
3. Nemestrine Macaque	40·7	10. Bengal Tiger	22·7
4. Capuchin Monkey	35·3	11. Common Fox	20·7
5. Virginian Bear	35·0	12. Pyrenean Mastiff	7·0
6. European Wolf	31·4	13. Goat	0·0
7. Japanese Bear	30·6		

It will be observed that the fore foot of the Goat, regarded simply as an orrgan of locomotion, attains a perfection comparable with that of the hind foot of the Kangaroo, no force being lost by friction at the wrist-joint.

The only animal in which I found a departure from the typical hand was the Llama, in which the flexor tendons of the fingers exceed the flexor tendon above the wrist by 14·4 per cent.

The bearing of the foregoing results on the habits of locomotion of the several animals will suggest themselves at once to naturalists who have carefully studied those habits. I shall merely add that the subject admits of being carried into the details of the separate or combined actions of the several fingers and toes, and that the habits of various kinds of monkeys in the use of certain combinations of fingers or toes may be explained satisfactorily by the minute study of the arrangement and several strengths of the various flexor tendons distributed to the fingers or toes.

VIII. "Experiments on the effect of Alcohol (Ethyl Alcohol) on the Human Body." By E. A. PARKES, M.D., F.R.S., Professor of Hygiene in the Army Medical School, and Count CYPRIAN WOLLOWICZ, M.D., Assistant Surgeon, Army Medical Staff. Received April 4, 1870.

As a knowledge of the physiological effects of alcohol on the human body is a matter of great importance, and as previous observations leave some points in doubt, we took the opportunity which the willingness and zeal of a very intelligent healthy soldier afforded us of investigating this subject.

In order not to lengthen the paper, we have given only our own observations, without referring to those of others.

The plan of observation was as follows:—For twenty-six days the man remained on a diet precisely similar as to food and times of meals in every respect, except that for the first eight days he took only water (in the shape of coffee, tea, and simple water); for the next six days he added to this diet rectified spirit, in such proportion that he took, in divided quantities, on the first day one fluid ounce (=28·4 cub. centims.) of absolute alcohol; on the second day two fluid ounces; on the third day four ounces, and on the fifth and sixth days eight ounces on each day. He then returned to water for six days, and then for three days took on each day half a bottle (=12 ounces, or 341 c. c.) of fine brandy, containing 48 per cent. of alcohol. Then for three days more he returned to water.

There were thus five periods, viz. of water-drinking, alcohol, water, brandy, water.

Before commencing the experiments, the man, who had been accustomed to take one or two pints of beer daily, abstained altogether from any alcoholic liquid for ten days.

This man, F. B., is twenty-eight years of age, 5 feet 6 inches in height, and his usual weight is 134 or 136 lbs. He is finely formed, with little fat, and with largely developed powerful muscles; he has a clean smooth skin, a clear bright eye, good teeth, and is in all respects in perfect health. He is very intelligent, and assisted us so much that we are quite certain that there has not been a mistake even for a minute in the time of taking the temperatures and passing the urine. As he had always been accustomed to smoke, we thought it proper to allow him half an ounce of tobacco daily, for fear the deprivation of it might disturb his health.

In addition to the experiments recorded in this paper, we tested the accuracy of his vision, and the muscular power before and during the use of alcohol; but as we could not detect any difference, we do not give the experiments.

Our object being to test the dietetic effects of alcohol, we gave it in small and large quantities, but avoided producing any extreme symptoms of narcotism.

FOOD.

Amount of solid food taken daily through the whole period:—

	Ounces. Avoirdupois.	Amount of nitrogen. Grains.
Bread	16	60·99
Beefsteak............	12	173*
Fat for frying ditto	2	?
Butter	1	?
Sugar	3	
Milk................	6	16·5
Potatoes	16	16
Salt	$\frac{1}{16}$	
		266·49
		or 17·27 grammes.

The meat was fried in the fat. The meals were taken always at the same time, viz. at 8 A.M., 1.30 P.M., and 5 P.M.; at 10 P.M. he took four ounces of water.

The amount of water taken was:—

	In fluid ounces.	In c. c.
First period before alcohol.	48	1363
Alcoholic period.		
First day	47	1334
Second day	46	1306
Third day................	44·5	1263·8
Fourth day	42·7	1214
Fifth day	41	1164
Sixth day................	41	1164
After alcohol	48	1363
Brandy period	42	1164
After brandy	48	1363

It was not intended that the quantity of water should be altered; but through a misconception, the man thought the spirit and brandy were to take the place of the water, and took therefore less water in proportion. In one respect the mistake was fortunate. The total amount of water taken in the so-called solid food, and as drink, was about 72½ fluid ounces, or 2059 c. c. daily during the water days, and a little less during the days on which he took alcohol and brandy.

* The nitrogen in the beefsteak was determined once; the result was almost identical with the results given in experiments in exercise recorded in No. 94 (1867) of the Proceedings of the Royal Society. As the bread was analyzed on a former occasion, it was not so now; its composition is very constant, the same amount of flour, water, and yeast being always used in the hospital bakery at Netley.

Weight of Body without clothes.

(Accuracy of Machine = turns with one ounce avoirdupois.)

Taken at 8 A.M., after the bladder was emptied, before breakfast and at the end of the twenty-four hours constituting the day.

Days.	Water alone or alcohol and water, taken as drink.	Weight in lbs.	Weight in kilogrammes.
1	Water	133·5	60·68
2	Water	133·75	60·795
3	Water	133·75	60·795
4	Water	134·5	61·1
5	Water	135·5	61·59
6	Water	135·8	61·72
7	Water	135·9	61·77
8	Water	136	61·81
9	One fluid ounce of absolute alcohol	136	61·81
10	Two fluid ounces	136	61·81
11	Four fluid ounces	135·75	61·7
12	Six fluid ounees	136	61·81
13	Eight fluid onnces	136	61·81
14	Eight fluid ounces	136	61·81
15	Water	136	61·81
16	Water	136	61·81
17	Water	135·5	61·59
18	Water	135·25	61·477
19	Water	135·5	61·59
20	Water	135·5	61·59
21	Brandy twelve fluid ounces (containing six fluid ounces of alcohol)	135·5	61·59
22	" "	135·5	61·59
23	" "	136	61·81
24	Water	136	61·81
25	Water	136	61·81
26	Water	136	61·81

During the first few days there was a gradual increase in weight, owing probably to the food being rather greater and the exereise less than before; equilibrium was reached on the eighth day, and the weight remained almost unchanged during the alcoholic period. There was slight decrease after alcohol; and on the last brandy day a slight increase, which was maintained in the after period. The general result appears to be that (other conditions remaining constant) the effect of alcohol in modifying weight is quite unimportant.

The Temperature of the Axilla and Rectum.

The temperature of the axilla was taken (in Fahr. degrees) every two hours, from 8 A.M. to 10 P.M., the man being in bed and covered with the clothes. The temperature of the rectum was taken at 10 A.M., 2 P.M., and 10 P.M. The thermometer was in each case kept in for twenty minutes. We did not take the night temperatures for fear of injuring the health by destroying rest.

Axilla Temperatures.

The temperatures of the first day are omitted.

First Period, before Alcohol.

Hours.	Days.						
	Second, water.	Third, water.	Fourth, water.	Fifth, water.	Sixth, water.	Seventh, water.	Eighth, water.
8 a.m.	97·1	98	97·2	98·6	97	98·5	98·4
10 „	97·7	97·2	98·1	98·7	98	98·5	99
12 noon	97·8	97·9	97·9	98·2	98·1	99·1	98
2 p.m.	98·3	97·9	98·1	98·0	98	98·1	98
4 „	98·3	97·9	98·0	99·0	97·7	98·9	98·4
6 „	97·7	97·4	98·2	99·0	97·4	99	99·4
8 „	98·3	97·4	98·0	98·2	97·8	99	100·4
10 „	97·9	97·8	97·9	98·0	97·7	98	100·4
Means.........	97·9	97·7	97·9	98·46	97·7	98·69	99·1

Second Period, with Alcohol.

Hours.	Days.					
	Ninth, 1 fl. oz. alcohol.	Tenth, 2 fl. oz. alcohol.	Eleventh, 4 fl. oz. alcohol.	Twelfth, 6 fl. oz. alcohol.	Thirteenth, 8 fl. oz. alcohol.	Fourteenth, 8 fl. oz. alcohol.
8 a.m.......	97·8	98·2	98·4	97·7	98·6	98·4
10 „	98	98	98·4	98·5	100·3	98·2
12 noon ...	97·6	98·6	98·4	99·4	100·4	98·4
2 p.m......	98·4	97·8	100·1	98	99	97·8
4 „	97·6	99·5	98·5	98·4	98·9	97·6
6 „	98·2	98·2	99	100	98·6	98·4
8 „	98·4	99·6	98·6	99·2	99·2	98·4
10 „	98	97·8	98	98·8	97·6	97·8
Means	98	98·46	98·7	98·6	99·08	98·1

Third Period, after Alcohol.

Hours.	Days.					
	Fifteenth, water.	Sixteenth, water.	Seventeenth water.	Eighteenth, water.	Nineteenth, water.	Twentieth, water.
8 a.m.......	98·2	98·1	98·2	98·2	98·2	98
10 „	99	98·8	97·6	98	97·8	98·4
12 noon ...	98·2	98·8	98·4	97·4	98·5	98
2 p.m.......	97·8	98·2	98·4	98·4	98·6	98
4 „	97·6	98·2	98·0	98·6	98	98
6 „	98·4	99	98·4	97	98·4	98·6
8 „	98·4	100·7	98·0	99·4	97·8	98·2
10 „	97·8	97·6	98·6	98	98	98
Means	98·17	98·8	98·2	98·12	98·16	98·15

Fourth and Fifth Period. Brandy and after Brandy.

Hours.	Days.					
	21st, 12 fl. oz. brandy.	22nd, 12 fl. oz. brandy.	23rd, 12 fl. oz. Brandy.	24th, water.	25th, water.	26th, water.
8 a.m.	98·2	98·6	97·8	98·2	98	98·2
10 „	98·4	98·8	98·4	98·5	98·4	98·4
12 noon ...	98·4	99·4	98·2	98	98·2	98·2
2 p.m.......	98·9	97·4	98·0	98·4	99	99
4 „	99	98·8	98·0	98·4	97·8	98·7
6 „	99·6	99	98·8	98·8	98·2	98
8 „	99·4	98·4	98·8	98·2	98	97·8
10 „	99·2	97·8	98·2	98·	97·8	98·7
Means	98·8	98·5	98·25	98·3	98·17	98·35

If the means of the days of the 5 periods be put together, and the means for each period be taken, the results are—

	Mean temperature.
Before alcohol	98·207
During alcohol	98·49
After alcohol	98·266
During brandy	98·51
After brandy	98·27

These experiments show that alcohol and brandy produce little change in the temperature of the axilla in healthy men; but what effect there is appears to be rather in the direction of increase than of diminution. But that the effect of 8 ounces (=227 c. c.) of absolute alcohol, taken in 24 hours, is really trifling is seen by the Table; on the 13th day, when this large quantity was taken, the temperature rose higher than on any other day; the thermometer was over 100° at 10 and 12 o'clock, and the mean of the 8 observations was 99°; it might have been thought that alcohol really increased the temperature, but on the next day, with the same amount of alcohol, the temperature was lower throughout, and the mean of the day was only 98°·1. On the 12th and 13th days in fact the man had a slight febrile catarrh, as will be noticed further on, and the temperature rose during this attack.

We draw the conclusion that the changes in temperature in the axilla were insignificant.

Temperature of the Rectum.

Days.	Fluid taken.	Hours.				
		8 a.m.	2 p.m.	4 p.m.	6 p.m.	10 p.m.
1	Water	......	......	......	......	......
2	Water	......	98·9	......	......	97·9
3	Water	98·2	99	......	......	98·1
4	Water	98·1	99·2	......	......	98·9
5	Water	98·6	99·1	......	......	98·1
6	Water	98·1	99	......	......	99·1
7	Water	99·2	98·9	......	......	99
8	Water	99	100·4	......	......	101
9	Alcohol, 1 fluid ounce	99·4	101	......	99·4	98·2
10	Alcohol, 2 fluid ounces	98·4	99·6	......	100	99·6
11	Alcohol, 4 fluid ounces	98·6	99·5	......	99·6	99·6
12	Alcohol, 6 fluid ounces	97·6	99·7	99·9	99·7	100·2
13	Alcohol, 8 fluid ounces	100·2	100·4	100·5	99·2	98·2
14	Alcohol, 8 fluid ounces	99·6	99·6	......	......	98·4
15	Water	99	98·8	......	......	98·8
16	Water	98·8	99·4	......	......	98·2
17	Water	98·6	99·4	......	......	98
18	Water	98·4	99·5	......	......	98·4
19	Water	99	98·4	......	......	98·6
20	Water	99	99·6	......	......	99·5
21	Brandy, 12 fluid ounces	99·6	99	......	......	99·8
22	Brandy, 12 fluid ounces	100	99·4	......	......	99·1
23	Brandy, 12 fluid ounces	98·6	99·6	......	......	99
24	Water	99	99·8	......	......	98·8
25	Water	98·8	99·6	......	......	98·6
26	Water	99·2	99·6	......	......	99·5

The mean results are as follows:—

Hours.	Rectum mean temperature.				
	First period. Water.	Second period. Alcohol.	Third period. Water.	Fourth period. Brandy.	Fifth period. Water.
8 a.m.	98·5	98·96	98·8	99·4	99
2 p.m.	99·21	99·96	99·18	99·3	99·66
10 p.m.	98·87	99·03	98·6	99·3	98·96
Mean of the three observations	98·86	99·31	98·86	99·33	99·21

The rectal observations show that alcohol and brandy in the above quantities cause no lessening of temperature in the rectum; on the contrary, there is slight increase in both the second and fourth periods as compared with the first and third (which were precisely the same), though, as in the case of the axilla, the difference is not great, being in each case very nearly half a degree Fahr.

In this man the rectum temperature is slightly greater than the axillary. As no great number of observations have been made on this point,

the following notes of a single day (the eighteenth, when the man was taking water) may be interesting:—

Hour.	Axilla temperature.	Rectum temperature.
8 a.m.	98°·2	98°·4
10 „	98	
11 „	98	98·6
12 noon	97·4	98·2
1 „	97·6	98·4
2 „	98·4	99·5
3 „	98·2	99·4
4 „	98·6	99·2
5 „	97·4	98·6
6 „	97	98
7 „		97·6
8 „	99·4	98·2
9 „	97·6	98·2
10 „	98	98·4
Mean	97·98	98·51

The mean difference on this day in favour of the rectum is 0°·53; but, as appears from the former Tables, the rectum sometimes has a temperature of 1°, or even 2°, more than the axilla: but such difference as the last number seldom occurred.

The general result from all these observations surprised us, considering the numerous experiments on men and animals in which the temperature has been found to be lowered by alcohol. An explanation may, however, be possible. Our experiments being to ascertain the dietetic properties of alcohol, we never aimed at producing very decided narcotism or marked symptoms of poisoning; and as we had to deal with a perfectly healthy resisting organism, which received always the same quantity of food, the effect of alcohol in lowering temperature might not be so well marked as in an ill-fed or unhealthy body. We do not dispute the accuracy of the observations which show that large and narcotic doses of alcohol lower the temperature of the body in men and animals; but our experiments prove that alcohol, in the limits we have stated and with an equal supply of food, did not have this effect in a perfectly healthy man.

The rising of mean temperature which seemed to occur was not considerable enough to make it probable that it was due to heat derived from combustion of alcohol; it was more probably owing to quickened circulation, and in addition the slight febrile attack which occurred on the twelfth and thirteenth days, augmented the mean temperature of the alcoholic period; but this would not account for the similar slight increase in the brandy period.

The Effect on the Circulation.

The pulse (taken usually every two hours) was decidedly more frequent when alcohol and brandy were used. The mean of all the observations in the recumbent position was 73·57 beats per minute in the first period when water was taken; during the alcoholic days the mean number of beats was 88·5; after alcohol 78·6; during the brandy days 91·4, and after brandy 81·1.

If particular hours are taken the same results come out, as shown in the following Table:—

	Mean pulse at 10 a.m.	Mean pulse at 2 p.m.	Mean pulse at 10 p.m.
Before alcohol	75·5	80·8	73
During "	99	94	80·8
After "	89·66	87·5	71·6
During brandy	96·6	93	92
After "	88·6	84	73

There is therefore no doubt that the frequency of the pulse was increased, and the effect was also persistent; for, though it fell after the alcohol was left off, it had not reached in six days the point which was proper to it before the alcohol.

The pulse was not only increased in rapidity, but it was fuller; it appeared to have more volume.

The highest mean pulse on any day before alcohol was 77·5 beats; the mean pulse of the first alcoholic day (one fluid ounce of absolute alcohol) was 80; with two ounces of alcohol 78·3; with four ounces 86; with six ounces 98·3 (but there was exceptional fever); with eight ounces 93·6; and on the last day, with eight ounces, 94·7. On the first day after alcohol it sank to 80.

The effect on the circulation in the small vessels of the skin was very marked. The face, ears, and neck were flushed, and on the days of the large doses the face was slightly swollen. The skin of the trunk, as well as of the face, appeared hot to the man himself, and this was no doubt dependent on the same cause. It was some time before the turgescence of the small cutaneous vessels lessened. Accompanying it was a sense of fulness and heaviness in the head, as if the intracranial vessels were also enlarged, and there was a feeling of warmth at the epigastrium.

Sphygmographic observations were made on the right radial artery. They were always taken with the same instrument, with an equal pressure, and when the man was in a recumbent position. Altogether more than 150 tracings were taken, but some were spoilt in photographing*. All the remainder are subjoined.

One fluid ounce of absolute alcohol in twenty-four hours altered the

* They were taken and photographed with great care by Mr. James Sylvester, Apothecary to the Forces, who also gave us much assistance in various ways.

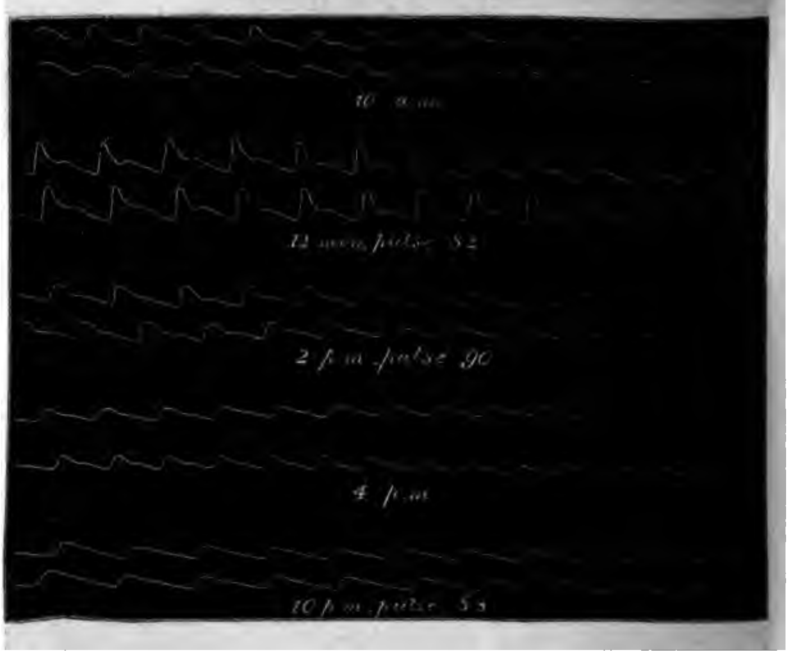
[redacted]

Tenth Day.

One ounce of alcohol at 8 a.m.
,, ,, ,, 1.30 p.m.

Eleventh Day.

Two ounces of alcohol at		8 a.m.
One ounce	..	1.30 p.m.
One ,,	,,	5 p.m.

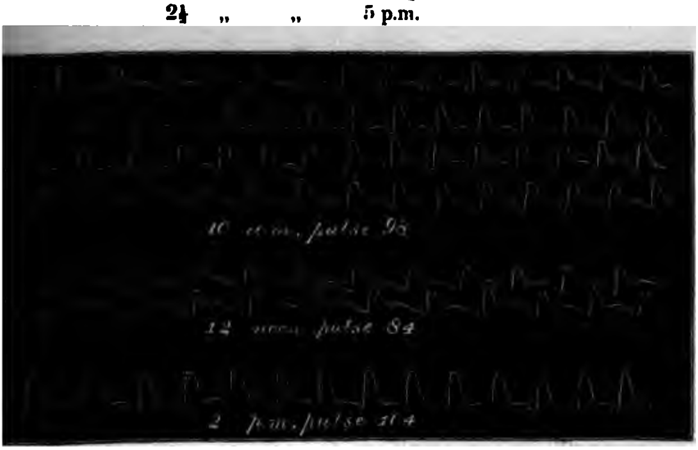


Twelfth Day.

3 ounces of alcohol at 8 a.m.
1¼ ounce „ 1.30 p.m.
1¼ „ „ 5 p.m.

Thirteenth Day.

3 ounces of alcohol at 8 a.m.
2¼ „ „ 1.30 p.m.
2¼ „ „ 5 p.m.

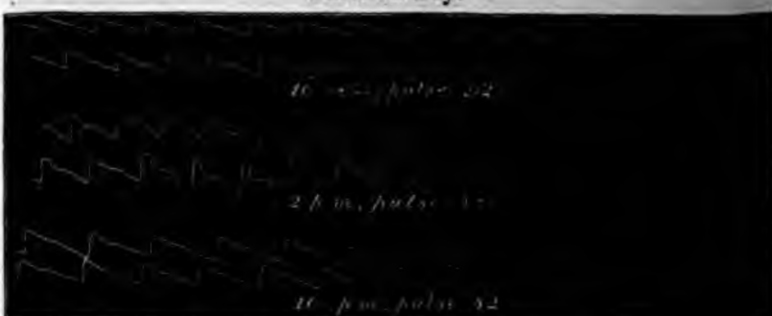


Fourteenth Day.

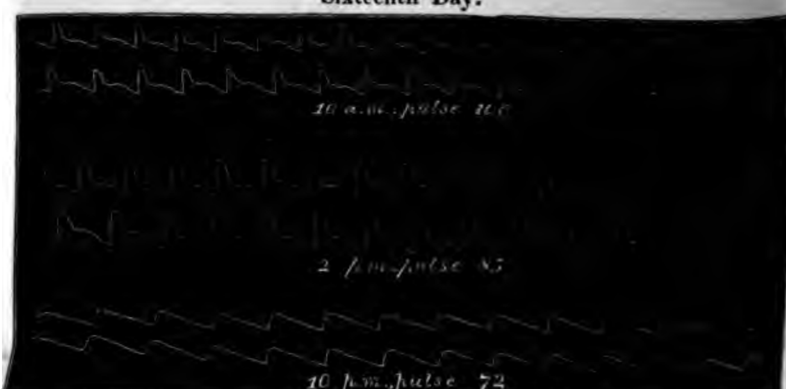
3 ozs. of alcohol at 8 a.m.—2½ ozs. of alcohol at 1.30 p.m.—2½ ozs. of alcohol at 5 p.m.

Third Period.—6 days Water-drinking.

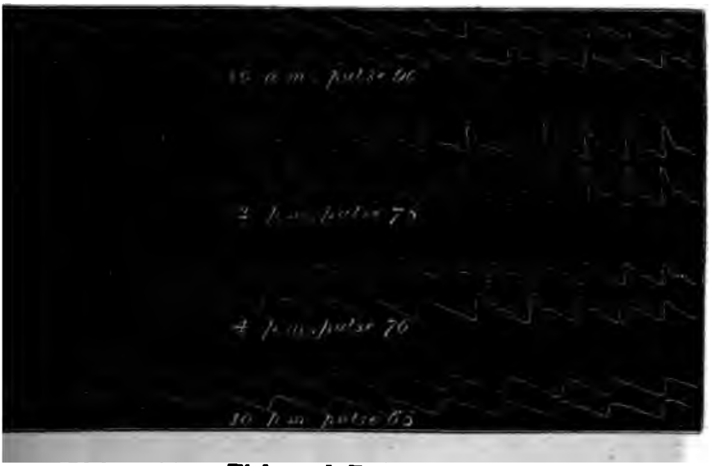
Fifteenth Day.



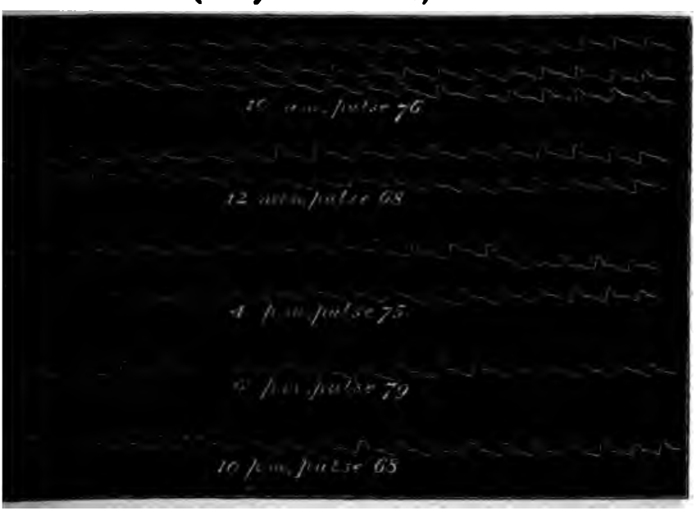
Sixteenth Day.



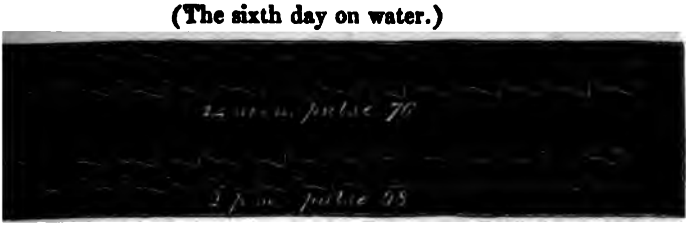
Seventeenth Day.



Eighteenth Day.
(A day of rest in bed.)



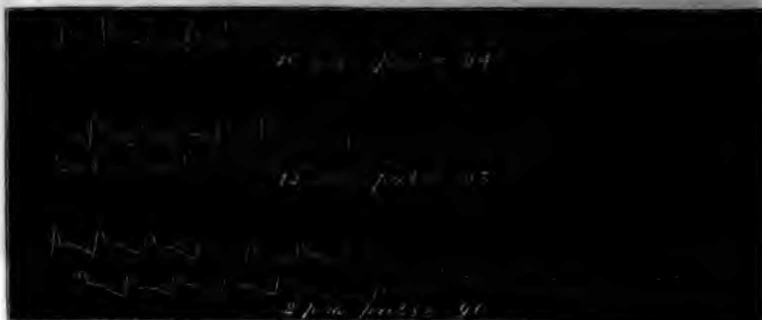
Twentieth Day.
(The sixth day on water.)



FOURTH PERIOD.—3 DAYS BRANDY.

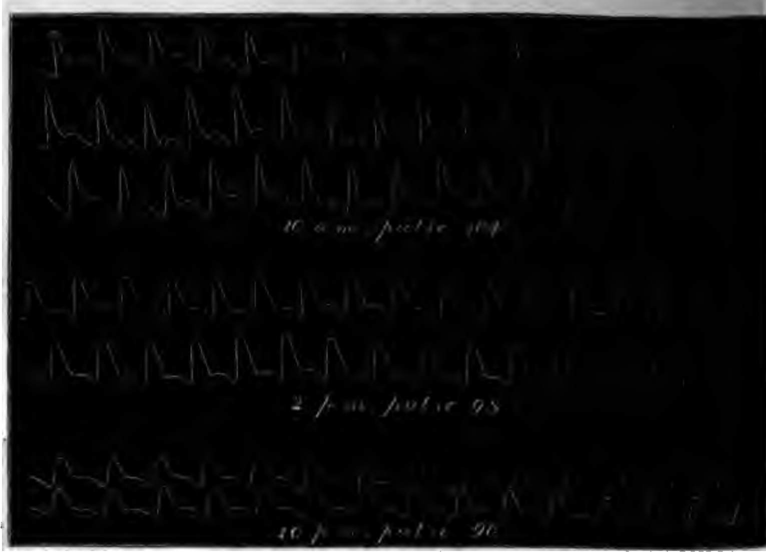
Twenty-first Day.

Four ounces at 8 a.m.
" " 1.30 p.m.
" " 5 p.m.



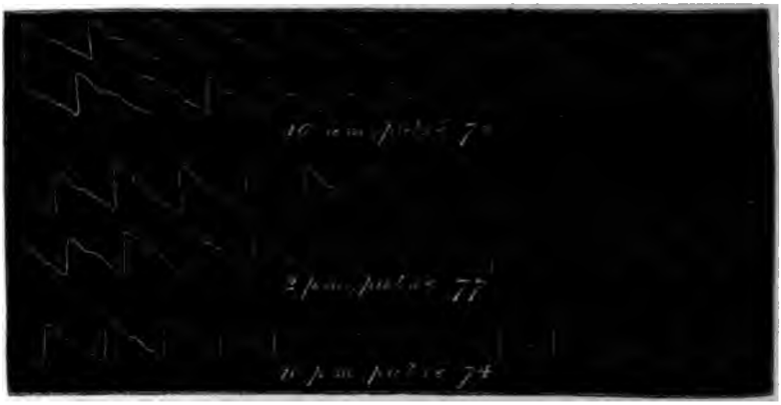
Twenty-second Day.

Four ounces of brandy at 8 a.m.
" " 1.30 p.m.
" " 5 p.m.



Fifth Period.—Water.

Twenty-fourth Day.



Twenty-fifth Day.

Twenty-seventh Day.

Seven days after.

(15 minutes after taking a glass of beer.)

After the alcohol was left off the tracings show indications of its influence, even to the sixth day. The tracing on the eighteenth day (the fourth after the cessation of alcohol) shows a weak and quickly acting heart; but allowance must be made for the fact that that was a day of complete rest in bed. On the sixth day after alcohol the mean pulse was 76·2 per minute, and the tracing shows still rapidity and feebleness of the heart's action. This seems to confirm the usual doctrine that increased rapidity of contraction from the action of alcohol is followed by exhaustion; but it also shows that this effect does not ensue so immediately as is supposed, but that the effect of the alcohol is more persistent.

When brandy was then given, the effect on the exhausted heart was very obvious; the ventricle commenced to contract again more rapidly, and, in fact, the effect of the brandy is more marked than that of alcohol.

It is difficult perhaps to explain all the indications of the brandy tracings, but there seems no doubt that the ventricular contraction was very sudden; the aortic valves opened with violence; a rapid wave traversed the blood, sending the lever up very high; the summit of the curve is sharp, and the equilibrium of tension between ventricle and artery must have been soon reached; the arteries emptied themselves very rapidly.

After the brandy was left off the tracings are seen gradually returning to the curve of health, though they had not reached it on the morning of the twenty-seventh day (the fourth after brandy), when the experiments were obliged to be discontinued.

Seven days later the pulse was nearly healthy again.

It is noticeable that twelve ounces of brandy (containing 48 per cent. of alcohol) had more effect than eight ounces of absolute alcohol, but it must be remembered that when the brandy was given the heart had not recovered from the influence of the alcohol; in other words, it was not perfectly healthy *.

* Dr. Burdon-Sanderson was kind enough to look at three tracings, No. 1 of the water period, No. 2 of the alcoholic period, and No. 3 of the brandy period. He writes as follows:—

"I think (1) that No. 1 is a normal pulse.

"(2) That the changes exhibited in Nos. 2 and 3 are of the same nature, but different in degree; *i. e.* that the degree of modification is greater in 3 than in 2. Hence the explanation of both must be the same.

"(3) The alteration of form is partly due to the mere increase of frequency: but in

Putting together the evidence derived from the pulse as felt by the finger, from the state of the cutaneous vessels, and from the sphygmographic tracings, it seems fair to conclude that the chief effects of alcohol on the circulation in health are on the ventricles (the rapidity with which contractions are accomplished being greatly increased), and on the capillaries (which are dilated and allow blood to pass more freely through them). The valuable observations of Dr. Anstie have shown that in many febrile cases, when alcohol is acting usefully, the arterial tension is increased; while in other cases, when there is narcotism, the tension is lowered. In this healthy man the effect of either small or large doses on the arterial tension is not perhaps well marked.

Action on the Urine.

Elimination of water by the kidneys.

Days.	Fluid taken in twenty-four hours in food and drink.	Quantity of urine in c. c.
1	72½ fluid ounces of water, or 2059 c. c.	1726
2	72½ fluid ounces of water, or 2059 c. c.	1197
3	72½ fluid ounces of water, or 2059 c. c.	1290
4	72½ fluid ounces of water, or 2059 c. c.	1220
5	72½ fluid ounces of water, or 2059 c. c.	950
6	72½ fluid ounces of water, or 2059 c. c.	1167
7	72½ fluid ounces of water, or 2059 c. c.	1205
8	72½ fluid ounces of water, or 2059 c. c.	1000
9	71·5 fluid ounces, or 2030 c. c., and 1 fluid ounce of alcohol	1300
10	70·5 fluid ounces, or 2002 c. c., and 2 fluid ounces of alcohol	1550
11	69 fluid ounces, or 1959 c. c., and 4 fluid ounces of alcohol	1440
12	67 fluid ounces, or 1902 c. c., and 6 fluid ounces of alcohol	1060
13	65.5 fluid ounces. or 1860 c. c., and 8 fluid ounces of alcohol	1800
14	65·5 fluid ounces, or 1860 c. c., and 8 fluid ounces of alcohol	1020
15	72½ fluid ounces of water, or 2059 c. c.	980
16	72½ fluid ounces of water, or 2059 c. c.	1600
17	72½ fluid ounces of water, or 2059 c. c.	1400
18	72½ fluid ounces of water, or 2059 c. c.	1660
19	72½ fluid ounces of water, or 2059 c. c.	1180
20	72½ fluid ounces of water, or 2059 c. c.	1110
21	66·5 fluid ounces of water, or 1880 c. c., and 6 ounces of alcoholic brandy	1610
22	66·5 fluid ounces of water, or 1880 c. c., and 6 ounces of alcoholic brandy	1270
23	66·5 fluid ounces of water, or 1880 c. c., and 6 ounces of alcoholic brandy	1260
24	72½ fluid ounces, or 2059 c. c.	1100
25	72½ fluid ounces, or 2059 c. c.	1330
26	72½ fluid ounces, or 2059 c. c.	1580

addition to this the tracing shows the special characters of the pulsus celer, the description of which in my book, page 14, seems still correct (Handbook of the Sphygmograph. 1867).

"(4) The celerity or shortness of the expansile movement I understand to signify that the left ventricle performs its contraction *within a shorter period*, and therefore uses more force within a given time than in its natural state.

"(5) I do not see any reason for supposing that the arterial pressure is increased."

The mean amounts are as follows:—

Period.	Mean amount of water taken in food and drink. cub. centims.	Mean amount of urine passed. cub. centims.
First period (without alcohol)....	2059	1219
Second period (with alcohol)	1935	1361
Third period (with water)	2059	1321
Fourth period (with brandy)	1889	1380
Fifth period (with water)	2059	1337

As the amount of urine increased in the alcoholic period 142 cub. centims., while the water taken was less by 124 cubic centims., and the same result in a less degree occurred in the brandy period, there is no doubt that the alcohol increased the urinary water. Whether this was the consequence, as seems possible, of the greater frequency of the heart's action, or whether it arose from any purely diuretic influence of the alcohol, is uncertain. Was the body left poorer in water, or was the exit through the skin or lungs hindered?

As 4·3 ounces less of water passed in, and 5·3 ounces more passed out, in the alcoholic period, and as the mean amount of alcohol passing in was under 5 fluid ounces, the body ought to have lost weight, and perhaps would have done so but for one circumstance.

The possible amount of change of weight in this way would be of course slight, viz. about 4 ounces, and it happened that there was a less excretion of alvine matter (viz. 1 ounce less daily than during the first period), which would tend to cover the possible loss of water by the increased flow of urine. Also the error of the machine may be one ounce. We draw the conclusion that there was no decided evidence of lessening of elimination of water by other channels sufficient to account for the increased urinary flow.

The Nitrogen of the Urine.

The urea of 24 hours was determined by Liebig's mercuric nitrate solution, the chlorine being got rid of; and, in addition, the total nitrogen was determined by burning with soda-lime after the method of Voit, and leading the ammonia into a standard solution of sulphuric acid. In this way any error in the determination by either process was sure to be detected.

Days.	Fluid taken.	Urea, in grammes.	Nitrogen in urea, in grammes.	Nitrogen by soda-lime.
1	Water	37·000	17·266	17·151
2	„	33·960	15·848	16·142
3	„	33·080	15·437	16·298
4	„	38·040	17·752	17·752
5	„	33·540	15·652	16·525
6	„	35·100	16·380	16·070
7	„	30·080	14·457	13·770
8	„	32·990	15·396	14·555
9	Alcohol	35·938	16·771	16·614
10	„	36·758	17·150	17·387
11	„	32·126	14·992	15·029
12	„	38·658	18·052	20·300
13	„	34·047	15·890	15·592
14	„	34·129	15·930	15·715
15	Water	35·457	16·436	16·700
16	„	40·352	18·831	18·170
17	„	37·073	17·301	17·890
18	„	35·000	16·330	17·090
19	„	37·770	17·640	17·090
20	„	31·224	14·571	14·185
21	Brandy	34·357	16·030	16·003
22	„	35·712	16·666	17·140
23	„	34·344	16·027	16·109
24	Water	34·677	16·182	16·167
25	„	32·250	15·000	15·108
26	„	36·780	17·165	17·050

The mean daily amounts are :—

	Urea.	Nitrogen in urea.	Nitrogen by soda-lime.
	grammes.	grammes.	grammes.
First period (water)	34·336	16·023	16·033
Second period (alcohol).........	35·276	16·464	16·773
Third period (water)............	36·146	16·851	16·954
Fourth period (brandy).........	34·804	16·241	16·417
Fifth period (water)	34·569	16·115	16·108

As 17·27 grammes of nitrogen (or probably a little more) entered with the food, and as, in the two stools which were examined, 1·6 and 2 grammes of nitrogen passed off respectively, it is certain that in this, as in other cases recorded, the whole of the nitrogen passed off by the kidneys and bowels, and none emerged by the skin or lungs. Of the 17¼ or 17½ grammes which entered as food, 16 or 16½ passed off with the urine and 1¼ or 1½, or from $\frac{1}{11}$ to $\frac{1}{13}$, by the bowels.

The effect of alcohol and brandy on the elimination of nitrogen was not great. In the alcoholic period there was a slight increase over the previous period, but this was dependent (partly, at any rate) on an accidental circumstance. On the twelfth day (during alcohol) the weather was very cold,

and the man had a chill; there was slight shivering, pain in the hips, and frequent sneezing. The temperature of the axilla reached 100° at 6 P.M., and 99°·2 at 8 P.M.; the temperature of the rectum at 10 P.M. was 100·2. The urine decreased greatly in amount (from 1440 cub. centims. to 1060 cub. centims.), and became very turbid from lithates. The urea increased to 38·65 grammes, giving 18·05 grammes of nitrogen, and the nitrogen by soda-lime was no less than 20·32 grammes. As this large excess surprised us, both processes were repeated three times with the same results; and it is therefore to be concluded that, in consequence of this ephemeral fever, there was a larger amount of urea (*i. e.* of substances precipitated by mercuric nitrate), and also a great excess of nitrogenous substances not precipitated by mercuric nitrate.

On the following day the ephemeral fever was better, though the temperature was high in the early part of the day: the amount of urine then became excessive (1800 cub. centims.), but the urea and the nitrogen determined by soda-lime both fell to the average. If this fever-day be deducted, the average of the five remaining alcoholic-days gives 16·067 grammes of nitrogen, or practically the same as in the water-period.

We draw the conclusion that some, probably all, the excess of nitrogenous elimination during the alcoholic period was due to this transient fever, which, it may be noted, was neither hindered in coming on nor apparently prevented in passing off, by the 6 and 8 ounces of absolute alcohol which were taken on those days.

In the period after the alcohol the amount, both of ureal and total nitrogen, increased. The excess was chiefly due to a great elimination on the sixteenth day. On this day again a slight febrile attack recurred, and the temperature ran high. At 8 P.M. it reached 100°·7, and then fell rapidly, so that at 10 P.M. it was normal in both axilla and rectum. The mean temperature of the day was 98°·8, which was considerably higher than on any other day in this period.

On the following three days the nitrogen continued high, and fell on the next day far below the average. In the brandy period it continued to fall, and in the last period (three days of water-drinking) was almost precisely the same as in the first.

The disturbing influences from these febrile attacks being allowed for, and the small amount of the changes in the quantity of nitrogen, even if these attacks are included, being taken into account, it may be concluded that alcohol in the above quantities produces no effect of importance in altering the elimination of nitrogen in the healthy body when the ingress of nitrogen is constant. If any change does occur, which is not certain, it is on the side of increase; and this might possibly be accounted for by the increased rapidity of the heart's action, and the augmented flow of urine, which would carry a little more urea with it.

Our conclusion is quite contrary to the observations formerly made on this subject, which indicated that nitrogen is largely retained in the body when alcohol is used, and that in this way alcohol both increases assimilation or, when food is deficient, saves the tissues from destruction and husbands strength. Whatever may be the case in febrile diseases (and on this point the evidence is defective), we are quite certain that this is not true for health, and that as long as the ingress of nitrogen is the same, 8 ounces of absolute alcohol and 12 ounces of brandy, containing nearly 6 ounces of alcohol, have no effect, or a trifling effect, on the processes which end in the elimination of nitrogen by the urine, and most decidedly do not lessen the elimination *.

The Phosphoric Acid, Chlorine, and Free Acidity of the Urine.

The phosphoric acid was determined by nitrate of uranium, the chlorine by nitrate of silver, the acidity by the graduated alkaline solution:—

* It may be noted with regard to the two processes for determining nitrogen, viz. precipitation by Liebig's mercuric nitrate and burning by soda-lime, that the mercuric nitrate throws down other nitrogenous matters besides the urea. Indeed, Voit considers (Zeitschr. für Biologie, Band ii. p. 470) that the total nitrogen in the urine of men may be safely concluded from this test. But this appears not to be so in all men. In the man now experimented upon, the nitrogen by soda-lime is actually very nearly the same as that calculated from the mercuric-nitrate precipitate. But in other men, and even in this man now and then, the former process gave a much larger result than the latter.

It will be observed that occasionally the process by soda-lime gives a smaller result than that by mercuric nitrate. The same fact is observable in the table given by Voit in the paper above referred to (p. 469). The explanation is probably this:—Possibly some of the non-ureal substances thrown down by mercuric nitrate may contain less nitrogen than urea, and the calculation is therefore incorrect; but the chief cause appears to be the following:—Both processes are liable to error. The mercuric nitrate being a colour test, is often difficult to estimate exactly; its failure is on the side of excess, and the amount of failure may be 2 or perhaps 3 per cent. On the other hand, the process by soda-lime has an error in the other direction: there is sometimes a difficulty in getting off the last traces of ammonia, and there may be therefore a slight error on the side of defect. If in any urine in which the amount of nitrogen by soda-lime ought really to coincide with that by mercuric nitrate, but in which each error of manipulation reaches its maximum limit (viz. that the mercuric-nitrate solution shows more nitrogen than exists, and the soda-lime process less), the amount of nitrogen by the latter plan may appear considerably less than by the former.

Days.	Period.	Phosphoric acid.	Chlorine.	Free acidity =crystallized oxalic acid.
		grammes.	grammes.	grammes.
1	Water	2·554	10·507	2·119
2	„	2·239	5·524	1·313
3	„	2·161	7·342	...
4	„	1·891	7·648	1·977
5	„	1·876	4·584	2·483
6	„	2·020	6·152	...
7	„	1 711	7·265	2·173
8	„	2·000	6·603	1·778
Mean.		2·056	6·915	1·974
9	Alcohol	2·184	7·776	2·174
10	„	2·821	7·126	2·592
11	„	2·117	7·082	2·485
12	„	2·400	7·826	2·345
13	„	1·870	7·508	2·116
14	„	1·990	8·780	2·292
Mean.		2·228	7·586	2·342
15	Water	2·107	6·608	2·930
16	„	2·560	9·656	1·633
17	„	2·716	10·437	1·902
18	„	2·407	9·267	2·035
19	„	2 690	8·796	2·840
20	„	1·953	6 698	1·909
Mean.		2·405	8·577	2·208
21	Brandy	2·592	8·773	2·525
22	„	2·413	10·363	2·656
23	„	1·890	10·735	2·171
Mean.		2·298	9·943	2·451
24	Water	2·233	7·712	2·307
25	„	2·367	9·206	1·391
26	„	2·607	11·218	2·520
Mean.		2·405	9 378	2·073

The changes in the phosphoric acid are so slight, that it is certain the alcohol exerted little effect. Thus, the mean of the first period being 2·056 grammes, on the two last days of the alcohol period, when 8 ounces of absolute alcohol were taken each day, the amount of phosphoric acid was 1·87 and 1·99 grammes respectively, which is the same as the mean of the first period. Now, if alcohol exerted any effect, we should expect these two days to show it. The mean of the next, or water period, when the body was in reality still impregnated with alcohol, was a little more (2·405 grammes). On the third day of brandy, when a bottle and a half had been taken in three days, the excretion was 1·89 gramme, or practically the same as in the first period.

Looking to the amounts of phosphoric acid excreted on the two last alcoholic days and the last brandy day, when the effect of the spirit, if any, would be most marked, it seems clear, if the phosphoric acid in the urine be in any way a measure of the metamorphosis of the nervous tissue (which

we do not affirm), that these experiments do not warrant any assertion that the alcohol interferes with such metamorphosis. The phosphoric acid was in fact unaffected even by such large quantities as 454 cub. centims., or not much less than ½ litre of absolute alcohol in 48 hours.

The chlorine was in larger quantities in the latter period of the experiments; but whether this was owing to the alcohol is doubtful. As the chlorine also passes off by the skin and bowels, variations in the amount eliminated by these channels affect the urine. On the 10th of February cold weather set in, and continued until the 18th; and it seems probable that some lessened action of the skin caused more chloride of sodium to pass in the urine.

The free acidity appeared to be increased in the alcoholic, and still more in the brandy period; but whether the increase is large enough to take it out of the limits of usual variation is not certain. It seems singular, if alcohol increases the free acidity, that on the two days when 8 fluid ounces were taken each day, the acidity was less than two days in the first period, and less than on the second alcoholic days, when only 2 ounces of alcohol were taken.

The acidity during the three brandy days was, however, high throughout, and it fell afterwards considerably, so that probably brandy does somewhat increase the acidity.

It is noticeable that the febrile attack on the twelfth day, which so influenced the nitrogen, and caused a large deposit of urates, was without influence on the free acidity.

On the whole, it may be concluded that the influence of alcohol on these three urinary constituents is inconsiderable.

The Alvine Discharges.

The discharges from the bowels were weighed every day; they were always natural except on the two first days, when there was some looseness. On those days the stools were rather liquid, and weighed 13¾ and 11½ ounces. Excluding these discharges, the mean numbers are as follows:—

	Weight in ounces avoirdupois.	Weight in grammes.
First period (water, last 6 days)	4·81	136·6
Second period (alcohol)	3·8	107·9
Third period (water)	3·04	86·34
Fourth period (brandy)	5·35	166
Fifth period (water)	3·41	96·8

The nitrogen was determined twice, viz. on the fifth day (water), and on the 12th day (6 ounces of alcohol); it amounted to 1·639 and 2·087 grammes respectively.

The alcohol, therefore, did not lessen the elimination of nitrogen by the bowels; and, considering the usual great variations in the weights of the stools from day to day, it is probable that it did not lessen their amount.

The Pulmonary Excretion.

On this point we made no experiments. The method of Professor von Pettenkofer has accustomed physiologists to such accuracy in the determination of the elimination of carbon, and there is so general a feeling that this method, as dealing with long periods, is the best that can be employed, that, as we had not Pettenkofer's appliances, we preferred doing nothing to falling short of a perfectly satisfactory and unquestionable result.

The Elimination of Alcohol.

The question as to the destruction or otherwise of alcohol in the body is very difficult to answer, owing to the impossibility of collecting all the excreta. The experiments of Schulinus, and especially of Anstie and Dupré, seem to show clearly that only a small part can be recovered from the body of animals or from the excreta. The latter authors, by using the bichromate of potassium and sulphuric-acid solution as a colour-test, and also by converting the alcohol into acetic acid and estimating it by an alkaline solution, could only prove the elimination of very small quantities by the urine; and the elimination was soon accomplished.

Owing to the number of experiments we had to make, we found we could not attempt to solve this very difficult question of elimination; and we will here merely briefly give the qualitative observations which alone we were able to make, and which, as far as they go, confirm the results arrived at by Perrin and Lallemand, Edward Smith, and others.

We used for this purpose the chromate test proposed by Masing, and used by most observers since.

Elimination by the Lungs.

During the first or water period, the man breathed several times daily, for 15 minutes at a time, through the solution of bichromate of potassium in sulphuric acid, without any change of colour being produced. On the fifth day (water) he breathed through a glass tube surrounded by a freezing mixture. About 1·7 cub. centim. of fluid were obtained, which gave no green reaction with the test. On the first day of alcohol (1 fluid ounce) no alcohol was indicated in the breath by the test; on the second day (2 fluid ounces) the test was slightly affected; on the four following days (4, 6, 8, and 8 ounces of alcohol) markedly so, but with variable intensity at different times of the day.

On the last day of alcohol the water in the breath was condensed during 15 minutes, in a glass tube surrounded by ice; ·7 cub. centim. of fluid were obtained, which gave a strong green reaction with the bichromate test.

On the following day breathing had no effect on the fluid.

During the brandy days the breath always produced a green tint, and usually it was very marked.

We did not attempt any determination of quantity by this colour test; and Anstie has pointed out that the bichromate test is so delicate that the

quantity passing off may easily be overrated; but it can hardly be doubted that in twenty-four hours there must be a good deal of elimination by this channel.

Elimination by the Skin.

On the seventh day, when only water was taken, the whole arm was placed in a glass jar, which was closed by india-rubber. A little fluid was collected, which gave no evidence of alcohol with the bichromate test.

In the afternoon of the eleventh day (the third of alcohol), when he had taken seven fluid ounces in three days, the arm was enclosed for six hours in the glass jar. About 12 c. c. of an acid fluid were collected; a small quantity of which gave an immediate and strong green reaction with the bichromate test.

On the fourteenth day (the sixth of alcohol), the arm was again enclosed in the jar, and 8 c. c. of an opalescent fluid collected, which gave a very decided reaction with the bichromate.

On the twenty-third day (the third of brandy) the arm was again placed in the jar for six hours; 10 c. c. of an acid fluid collected, which gave a strong green reaction with the bichromate test.

The general result of these experiments indicated that the skin is a considerable emunctory of alcohol, perhaps more so than the lungs, if the bichromate test is a safe one, which we are inclined to doubt.

Elimination by the Kidneys.

The examination was conducted as follows:—250 c. c. of the urine without any addition were placed in a large retort and distilled at a low heat, till about 150 c. c. had passed over. It was tested with bichromate; then 50 c. c. were redistilled, and about 15 c. c. were allowed to pass over. The following table gives the results:—

Day.	Fluid taken.	Reaction of first distillate with bichromate test.	Reaction of second distillate with bichromate test.
3.	Water......................	None.	
9.	Alcohol, 1 fluid ounce ...	None.	None.
10.	Alcohol, 2 fluid ounces...	None.	Distinct.
11.	Alcohol, 4 fluid ounces...	Slight.	Distinct.
12.	Alcohol, 6 fluid ounces...	Distinct.	Very strong.
13.	Alcohol, 8 fluid ounces...	Very strong.	Very strong.
14.	Alcohol, 8 fluid ounces...	Very strong.	Very great.
20.	Water, and the same for 5 days before	Very slight, just possible to be affirmed.	
21.	Brandy, 12 fluid ounces.	Very strong.	
22.	Brandy, 12 fluid ounces.	Very strong.	
23.	Brandy, 12 fluid ounces.	Very strong.	

This table shows distinctly that with one ounce of alcohol in twenty-four hours, none was detected in the urine of that day; it was detected when two fluid ounces were taken; and then, as the amount of alcohol was increased, more and more passed into the urine, until at last the reaction

became very strong. As to the exact amount of alcohol passing off, we can say nothing; but, looking to the delicacy of the test, it was probably not great.

In the case of the brandy, we attempted on the first day to determine the quantity by the method of Dupré, viz. converting the alcohol into acetic acid by heating with chrome-alum.

The results indicated rather a larger quantity than he found; but still the amount was small. In the whole day's urine only ·1763 gramme, or 2·7 grains of alcohol were discoverable by this method.

Elimination by the Bowels.

The stools were mixed with distilled water; and after standing for seven or eight days in covered vessels, the water was poured off, and 30 c. c. were distilled from 250 c. c.

Day.	Fluid taken.	Reaction of distillate with the bichromate test.
11.	Alcohol.	Decided, but not great.
12.	"	"
13.	"	"
14.	"	"

We think it can scarcely be doubted that the elimination of alcohol does not take place so rapidly as is supposed. Looking to the evidence of the pulse, of the sphygmographic tracings, and of the urine on the twentieth day, we must conclude that, twenty-nine fluid ounces of absolute alcohol having been taken in six days, the body had still traces of it on the sixth day after the alcohol was left off.

The evidence of Anstie and Dupré is certainly strong against the urine being a great channel of elimination; but possibly, though not excessive at any one time, the exit is longer continued than they supposed; and when the constant passage from the skin and from the lungs and bowels is remembered, we can easily suppose that the totality of elimination may be really considerable.

But whether all the alcohol thus passes off, or whether some is destroyed, our experiments do not enable us to state.

General Conclusions.

1. One and two fluid ounces (28·4 c. c. and 56·8 c. c.) of absolute alcohol given in divided quantities in 24 hours to a perfectly healthy man seemed to increase the appetite. Four fluid ounces lessened it considerably; and larger quantities almost entirely destroyed it. On the last day of alcohol the man was three quarters of an hour eating 8 ounces of bread, and could hardly do so. Had he been left to his own wishes the amount of food taken would have been much diminished.

It appears, therefore, that in this individual some point near 2 fluid ounces of absolute alcohol is the limit of the useful action on appetite; but

If instead of the mean of the 8 days or 73·57
this one day, viz. 77 beats per minute, with the
sure not to overestimate the action of the alcoh
On the 9th day, with 1 fluid ounce of alcohol,
more.

On the 10th day, with 2 fluid ounces, 1872 tim
On the 11th day, with 4 fluid ounces, 12,960 ti
On the 12th day, with 6 fluid ounces, 30,672 t
On the 13th day, with 8 fluid ounces, 23,904 t
On the 14th day, with 8 fluid ounces, 25,488 ti

But as there was ephemeral fever on the 12th
deduction, and to estimate the number of beats
tween the 11th and 13th days, or 18,432. Add
excess of beats during the alcoholic days was 14,4
more than 13 per cent.

The first day of alcohol gave an excess of 4 pe
per cent.; and the mean of these two gives almo
excess as the mean of the 6 days.

Admitting that each beat of the heart was as s
period as in the water period (and it was really m
on the last two days of alcohol was doing one-fift

Adopting the lowest estimate which has bee
done by the heart, viz. as equal to 122 tons
during the alcoholic period did daily work in e
tons one foot, and in the last two days did extr
24 tons lifted as far.

The period of rest for the heart was shortene

tracings show a more rapid contraction of the ventricles, but less power than in the alcoholic period. The brandy acted, in fact, on a heart whose nutrition had not been perfectly restored.

The peripheral circulation was accelerated and the vessels were enlarged; and the effect was so marked as to show that this is an important influence for good or for evil when alcohol is used.

Referring only to this healthy man, it is clear that the amount of alcohol the heart will bear without losing its healthy sphygmographic tracing is small, and it must be supposed that some disease of heart or vessels would eventually follow the overaction produced by large doses of alcohol.

3. Although large doses of alcohol lessened appetite, they did not appear to impede primary digestion, as far as this could be judged of by the sensations of the man; nor did they seem to check the normal chemical changes in the body which end in the elimination of nitrogenous excreta, of phosphoric acid, and of free acidity. In other words, we were unable to trace either the good or the evil ascribed to alcohol in this direction: it neither depressed these chemical changes nor obviously increased them; it neither saved the tissues nor exhausted them; and even in the period of ephemeral fever its effects were negative.

But, of course, in these experiments we were not dealing with diseased tissues, nor with structures altered in composition by long-continued excess of alcohol. The results in such cases might be different; and it may be desirable to repeat that though appetite was lessened, the amount of food taken was the same each day.

4. Neither pure alcohol nor brandy, in the quantities given, lessened the temperature; in other words, they did not arrest the chemical changes which produce animal heat, or lessen the processes which regulate its amount, any more than they influenced nitrogenous tissue-change. Alcohol in no way influenced the rise of temperature during the attack of ephemeral fever; it neither lowered nor increased it. This appears to us conclusive against the proposal to use alcohol as a reducer of febrile heat.

On the other hand it is not clear that alcohol increased the temperature: it produced subjective feelings of warmth in the stomach, in the face, round the loins, and over the shoulders; but at the time when these were felt (for about one hour after tolerably large doses) the thermometer in the axilla and rectum showed no rise. This is best seen by comparing the two o'clock observations, which were taken about half an hour after dinner. The feelings result from the enlargement of the vessels and the greater flow of blood through them; so, also, the ephemeral fever was decidedly not made worse by it.

5. An effect on the nervous system was not proved by any evidence of increase or decline in the amount of phosphoric acid; but there were marked subjective feelings; and possibly also the increased action of the heart was a nervous condition, as the short contractions of the ventricle were like those ascribed to alterations in the nervous currents. The feelings which

were produced by four fluid ounces daily, and in a still higher degree by the larger quantities of alcohol, proved that narcotism was produced. There was no exhilaration, but a degree of heaviness, indisposition to exertion, and loss of cheerfulness and alacrity; there was slight headache, and even some torpor and sleepiness. All these effects were more marked with brandy. The commencement of narcotism was therefore produced in this man by some quantity much less than 4 fluid ounces, and probably nearer 2. It was nearly this amount which also commenced to destroy the appetite; and it may also be observed that a considerable rise in the frequency of the pulse occurred on the third day of alcohol, when 4 ounces were taken, whereas on the days with one or two ounces the pulse, though quickened, was so in a much less degree.

Putting therefore these points together, viz. that the obvious effect on the nervous system (*i. e.* narcotism), the loss of appetite, and a great rise in the quickness and frequency of the heart's beats occurred at the same time, it seems fair to conclude that there must be a relation between the phenomena, or, in other words, that all were owing to nervous implication.

It appears, then, clear that any quantity over two ounces of absolute alcohol daily would certainly do harm to this man; but whether this, or even a smaller quantity, might not be hurtful if it were continued day after day, the experiments do not show. It is quite obvious that alcohol is not necessary for him; that is, that every function was perfectly performed without alcohol, and that even one ounce in twenty-four hours produced a decided effect on his heart, which was not necessary for his health, and perhaps, if the effect continued, would eventually lead to alterations in circulation, and to degeneration of tissues. It is not difficult to say what would be excess for him; but it is not easy to decide what would be moderation; it is only certain that it would be something under two fluid ounces of absolute alcohol in twenty-four hours.

It will be seen that the general result of our experiments is to confirm the opinions held by physicians as to what must be the indications of alcohol both in health and disease. The effects on appetite and on circulation are the practical points to seize; and if we are correct in our inferences, the commencement of narcotism marks the point when both appetite and circulation will begin to be damaged. As to the metamorphosis of nitrogenous tissues or to animal heat, it seems improbable that alcohol in quantities that can be properly used in diet has any effect; it appears to us unlikely (in the face of the chemical results) that it can enable the body to perform more work on less food, though by quickening a failing heart it may enable work to be done which otherwise could not be so. It may then act like the spur in the side of a horse, eliciting force, though not supplying it.

The employment of alcohol in health and disease is so great a subject that we should have felt tempted to extend these remarks to some points of medical practice, had it been desirable to do so in this place. We will only say that while we recognize in these experiments the great practical

use of alcohol in rousing a failing appetite, exciting a feeble heart, and accelerating a languid capillary circulation, we have been strongly impressed with the necessity for great moderation and caution. In spite of our previous experience in the use of alcohol and brandy, we were hardly prepared for the ease with which appetite may be destroyed, the heart unduly excited, and the capillary circulation improperly increased. Considering its daily and almost universal use, there is no agent which seems to us to require more caution and more skill to obtain the good and to avoid the evil which its use entails.

We wish to guard ourselves against the supposition that in speaking of alcohol and brandy we refer at all to wine and beer, which contain substances, in addition to alcohol, which may make their action in nutrition somewhat different.

Presents received May 12, 1870.

Transactions.

Birmingham:—Institution of Mechanical Engineers. Proceedings. Nov. 2, 1859, Jan. 25, 1860, July 28, 29, 1868. Part 1, Aug. 3, 4, 1869; Part 2, Nov. 4, 1869. 8vo. *Birmingham* 1859–69.
The Institution.

Bremen:—Naturwissenschaftlicher Verein. Abhandlungen, Bd. II. Heft 2. 8vo. *Bremen* 1870. The Union.

Calcutta:—Asiatic Society of Bengal. Journal. 1869, Part 1, No. 4. 8vo. *Calcutta* 1870. Proceedings. 1869, No. 11; 1870, No. 1. 8vo. *Calcutta* 1869–70. The Society.

Harlem:—Musée Teyler. Archives. Vol. II. fasc. 4. roy. 8vo. *Harlem* 1869. The Museum.

Montreal:—M'Gill University. Calendar and Examination Papers, 1868–69. 8vo. *Montreal* 1869. The University.

Neuchâtel:—Société des Sciences Naturelles. Bulletin. Tome VIII. cahier 2. 8vo. *Neuchâtel* 1869. The Society.

Paris:—École des Mines. Annales des Mines. 6e série. Tome XVI. 5e et 6e livraisons de 1869. 8vo. *Paris* 1869. The Institution.

Association pour l'encouragement des Études Grecques en France. Annuaire. 4e année, 1870. 8vo. *Paris* 1870. The Association.

Philadelphia:—Franklin Institute. Journal. Nos. 527–30. 8vo. *Philadelphia* 1869–70. The Institute.

Würzburg:—Physikal.-Medicin. Gesellschaft. Verhandlungen. Neue Folge. Band I. Heft 4. 8vo. *Würzburg* 1869. The Society.

Analytical Indexes to Volume ... known as the Remembrancia, preserved at ... City of London. 8vo. *London* 1870.

The Library Committee of t

May 19, 1870.

Transactions.

Birmingham :—Institution of Mechanical Eng August, 1869. Newcastle Meeting. I *Newcastle* 1869–70.

Calcutta :—Asiatic Society of Bengal. Journ Part 2, No. 3. 8vo. *Calcutta* 1869. Proce 8vo. *Calcutta*.

Christiania :—Universitet. De Vi Logicæ Ra sophiæ historia, a M. J. Monrad. 8vo. chiels Syner og Chaldæernes Astrolab, *Christiania* 1866. Ungedruckte, unbeac Quellen zur Geschichte des Taufsymbo von C. P. Caspari, 1, 2. 8vo. *Christiania*

Devonshire Association for the Advancement Art. Report and Transactions. Vol. III.

Dublin :—Royal Irish Academy. Transactio Parts 9–15; Antiquities, Part 8; Polit *Dublin* 1867–70.

Falmouth :—Royal Cornwall Polytechnic Socie Report. 8vo. *Falmouth* 1869.

Transactions (*continued*).

Lausanne:—Société Vaudoise des Sciences Naturelles. Bulletin. Vol. X. No. 62. 8vo. *Lausanne* 1869. The Society.

Leeds:—Philosophical and Literary Society. Catalogue of the Library. 8vo. *Leeds* 1866. The Society.

London:—Anthropological Society. Anthropological Review. No. 29. 8vo. *London* 1870. The Society.

Geological Society. Quarterly Journal. Vol. XXVI. Part 2. (No. 102.) 8vo. *London* 1870. The Society.

Montreal:—Natural-History Society. The Canadian Naturalist and Geologist (and Quarterly Journal of Science). New series. Vol. III. Nos. 5, 6; Vol. IV. No. 2. 8vo. *Montreal* 1868–69. The Society.

Paris:—Société Entomologique de France. Annales. 4e série. Tome IX. 8vo. *Paris* 1869. The Society.

Philadelphia:—American Philosophical Society. Proceedings. Vol. XI. No. 81. 8vo. *Philadelphia* 1869. The Society.

Utrecht:—Physiologisch Laboratorium der Utrechtsche Hoogeschool. Onderzoekingen. Tweede Reeks, II. 8vo. *Utrecht* 1869. Dr. F. C. Donders, For. Mem. R. S.

Alderson (Sir James, F.R.S.) Address of the President of the Royal College of Physicians to the Fellows on the 11th of April, 1870. 8vo. *London.* The Author.

Donders (F. C., For. Mem. R. S.) Het Tienjarig Bestaan van het Nederlandsch Gasthuis voor Ooglijders; Verslag. 8vo. *Utrecht* 1869. The Author.

Farr (Dr. W., F.R.S.) Report to the International Statistical Congress held at the Hague in 1869. 8vo. *London* 1870. The Author.

Fayrer (J.) H.R.H. the Duke of Edinburgh, K.G., in India. 4to. *Calcutta* 1870. The Author.

Linder (M.) Du rôle de l'Attraction Universelle et de la résistance de l'éther dans les variations de forme des comètes, à propos de la théorie cométaire de M. Tyndall. 4to. *Paris* 1869. The Author.

Mensbrugghe (G. van der) Sur la Viscosité superficielle des Lames de solution de Saponine. 8vo. *Bruxelles* 1870. The Author.

Mohn (H.) Température de la Mer entre l'Islande, l'Écosse et la Norvége. 8vo. *Christiania* 1870. The Author.

Pander (C. H.) Ueber die Ctenodipterinen des devonischen Systems. 4to. *St. Petersburg* 1858. Ueber die Saurodipterinen, Dendrodonten, Glyptolepiden und Cheirolepiden des devonischen Systems. 4to. *St. Petersburg* 1860. The Etat-Major of Mining Engineers of Russia.

Pourtales (L. F. de) Contributions to the Fauna of the Gulf-stream at great Depths. 8vo. *Cambridge, Mass.* 1867. The Author.

Reslhuber (A.) Resultate aus den im Jahre 1868 auf der Sternwarte zu Kremsmünster angestellten meteorologischen Beobachtungen. 12mo. *Linz* 1870. The Author.

Stevenson (D.) Altered Relations of British and Foreign Industries and Manufactures: the Cause and the Cure. 8vo. *Edinburgh* 1869. The Author.

Zantedeschi (F.) La Meteorografia del Globo studiata a diverse altitudini da Terra. 8vo. *Brescia* 1869. The Author.

Nederlandsch Archief voor Genees- en Natuurkunde, door F. C. Donders en W. Koster. Deel IV. Afl. 5; Deel V. Afl. 1. 8vo. *Utrecht* 1869. Dr. Donders, For. Mem. R. S.

Report of the Meteorological Reporter to the Government of Bengal for the year 1868–69. fol. *Calcutta* 1869. H. T. Blanford, Esq.

The Society adjourned over Ascension Day and the Whitsuntide Recess to Thursday, June 16.

June 2, 1870.

The Annual Meeting for the election of Fellows was held this day.

General Sir EDWARD SABINE, K.C.B., President, in the Chair.

The Statutes relating to the election of Fellows having been read, Dr. Duncan and Capt. Evans were, with the consent of the Society, nominated Scrutators to assist the Secretaries in examining the Lists.

The votes of the Fellows present having been collected, the following Candidates were declared to be duly elected into the Society:—

William Froude, C.E.
Edward Headlam Greenhow, M.D.
James Jago, M.D.
Nevil Story Maskelyne, M.A.
Maxwell Tylden-Masters, M.D.
Alfred Newton, M.A.
Andrew Noble, Esq.
Capt. Sherard Osborn, R.N.
Rev. Stephen Parkinson, B.D.
Capt. Robert Mann Parsons, R.E.
William Henry Ransom, M.D.
Robert H. Scott, Esq.
George Frederic Verdon, C.B.
Augustus Voelcker, Ph.D.
Samuel Wilks, M.D.

Thanks were voted to the Scrutators.

"Preliminary Report of the Scientific Exploration of the Deep Sea in H.M. Surveying-vessel 'Porcupine,' during the Summer of 1869," conducted by Dr. CARPENTER, V.P.R.S., Mr. J. GWYN JEFFREYS, F.R.S., and Prof. WYVILLE THOMSON, LL.D., F.R.S. Received November 18, 1869.

Received November 18, 1869.

PART I.

INTRODUCTION.

PRELIMINARY PROCEEDINGS.

The following Extracts from the Minutes of the Council of the Royal Society set forth the origin of the 'Porcupine' Expedition, and the objects which it was designed to carry out.

January 21, 1869.

The Preliminary Report of the Dredging Operations conducted by Drs. Carpenter and Wyville Thomson (in the 'Lightning') having been considered, it was

Resolved,—That, looking to the valuable results obtained from these Marine Researches, restricted in scope as they have been in a first trial, the President and Council consider it most desirable, with a view to the advancement of Zoology and other branches of science, that the exploration should be renewed in the course of the ensuing summer, and carried over a wider area; and that the aid of Her Majesty's Government, so liberally afforded last year, be again requested in furtherance of the undertaking.

Resolved,—That a Committee be appointed to report to the Council on the measures it will be advisable to take in order to carry the foregoing resolution most advantageously into effect. The Committee to consist of the President and Officers, with Dr. Carpenter, Mr. Gwyn Jeffreys, and Captain Richards.

February 18, 1869.

Read the following Report of the Committee on Marine Researches:—

"The Committee appointed by the Council on the 21st of January, to consider the measures advisable for the further prosecution of Researches into the Physical and Biological Conditions of the Deep Sea in the neighbourhood of the British Coast, beg leave to Report as follows:—

"The results obtained by the Dredgings and Temperature-Soundings carried on during the brief Cruise of H.M.S. 'Lightning' in August and September 1868, taken in connexion with those of the Dredgings recently prosecuted under the direction of the Governments of Sweden and of the United States, and with the remarkable Temperature-Soundings of Captain Shortland in the Arabian Gulf, have conclusively shown—

"1. That the Ocean-bottom, at depths of 500 fathoms or more, presents a vast field for research, of which the systematic exploration can scarcely fail to yield results of the highest interest and importance, in regard alike to Physical, Biological, and Geological Science.

"2. That the prosecution of such a systematic exploration is altogether beyond the reach of private enterprise, requiring means and appliances which can only be furnished by Government.

"It may be hoped that Her Majesty's Government may be induced at some future time to consider this work as one of the special duties of the British Navy; which possesses, in the world-wide distribution of its Ships, far greater opportunities for such researches than the Navy of any other country.

"At present, however, the Committee consider it desirable that the Royal Society should represent to Her Majesty's Government the importance of at once following up the suggestions appended to Dr. Carpenter's 'Preliminary Report' of the Cruise of the 'Lightning,' by instituting, during the coming season, a detailed survey of the deeper part of the Ocean-bottom between the North of Scotland and the Faroe Islands, and by extending that survey in both a N.E. and a S.W. direction, so as thoroughly to investigate the Physical and the Biological conditions of the two Submarine Provinces included in that area, which are characterized by a strongly marked contrast in Climate, with a corresponding dissimilarity in Animal Life, and to trace this climatic dissimilarity to its source; as well as to carry down the like survey to depths much greater than have been yet explored by the Dredge.

"This, it is believed, can be accomplished without difficulty (unless the weather should prove extraordinarily unpropitious) by the employment

of a suitable vessel, provided with the requisite appliances, between the middle of May and the middle of September. The Ship should be of sufficient size to furnish a Crew of which each 'watch' could carry on the work continuously without undue fatigue, so as to take the fullest advantage of calm weather and long summer days; and should also provide adequate accommodation for the study of the specimens when freshly obtained, which should be one of the primary objects of the Expedition. As there would be no occasion to extend the Survey to a greater distance than (at the most) 400 miles from land, no difficulty would be experienced in obtaining the supplies necessary for such a four months' cruise, by running from time to time to the port that might be nearest. Thus, supposing that the Ship took its departure from Cork or Galway, and proceeded first to the channel between the British Isles and Rockall Bank, where depths of from 1000 to 1300 fathoms are known to exist, the Dredgings and Temperature-Soundings could be proceeded with in a northerly direction, until it would be convenient to make Stornoway. Taking a fresh departure from that port, the exploration might then be carried on over the area to the N.W. of the Hebrides, in which the more moderate depths (from 500 to 600 fathoms) would afford greater facility for the detailed survey of that part of the Ocean-bottom on which a Cretaceous deposit is in progress—the Fauna of this area having been shown by the 'Lightning' researches to present features of most especial interest, while the careful study of the deposit may be expected to elucidate many phenomena as yet unexplained which are presented by the ancient Chalk Formation. A month or six weeks would probably be required for this part of the Survey, at the end of which time the vessel might again run to Stornoway for supplies. The area to the North and N.E. of Lewis should then be worked in the like careful manner; and as the 'cold area' would here be encountered, special attention should be given to the determination of its boundaries, and of the sources of its climatic peculiarity. These would probably require the extension of the survey for some distance in a N.E. direction, which would carry the vessel into the neighbourhood of the Shetland Isles; and Lerwick would then be a suitable port for supplies. Whatever time might then remain would be advantageously employed in dredging at such a distance round the Shetlands as would give depths of from 250 to 400 fathoms, Mr. Gwyn Jeffreys's dredgings in that locality having been limited to 200 fathoms.

"The Natural-History work of such an Expedition should be prosecuted under the direction of a Chief (who need not, however, be the same throughout), aided by two competent Assistants (to be provided by the Royal Society), who should be engaged for the whole Cruise. Mr. Gywn Jeffreys is ready to take charge of it during the first five or six weeks, say, to the end of June, when Professor Wyville Thomson would be prepared to take his place; and Dr. Carpenter would be able to join the Expedition early in August, remaining with it to the end. It would be a great ad-

vantage if the Surgeon appointed to the Ship should have sufficient knowledge of Natural History, and sufficient interest in the inquiry, to participate in the work.

"The experience of the previous Expedition will furnish adequate guidance as to the appliances which it would be necessary to ask the Government to provide, in case they accede to the present application.

"With reference to the Scientific instruments and apparatus to be provided by the Royal Society, the Committee recommend that the detailed consideration of them be referred to a Special Committee, consisting of Gentlemen practically conversant with the construction and working of such instruments."

Resolved,—That the Report now read be received and adopted, and that application be made to Her Majesty's Government accordingly.

The following Draft of a Letter to be transmitted by the Secretary to the Secretary of the Admiralty was approved:—

"The Royal Society,
Burlington House,
February 18, 1869.

"SIR,—Referring to the 'Preliminary Report' by Dr. Carpenter of the Results of the Deep-Sea Exploration carried on during the brief cruise of Her Majesty's Steam-vessel 'Lightning' in August and September last, which has already been transmitted for the consideration of the Lords Commissioners of the Admiralty—I am directed by the President and Council of the Royal Society to state that, looking to the valuable information obtained from these Marine Researches, although comparatively restricted in duration and extent, they deem it most desirable, in the interests of Biological and Physical Science, and in no small degree also for the advancement of Hydrographical knowledge, that a fresh exploration should be entered upon in the ensuing summer, and extended over a wider area; and they now desire earnestly to recommend the matter to the favourable consideration of My Lords, in the hope that the aid of Her Majesty's Government, which was so readily and liberally bestowed last year, may be afforded to the undertaking now contemplated, for which such support would be indispensable.

In favour of the practicability and probable success of the proposed fresh exploration, I am directed to explain that the objects to be aimed at, as well as the course to be followed and the measures to be employed for their attainment, have mainly been suggested by the observations made and the experience gained in the last Expedition.

"Further information as to the proposed exploration will be found in the Report, herewith transmitted, of a Committee to whose consideration the subject was referred by the Council.

"It is understood that the requisite Scientific Apparatus and the remuneration of the Assistants to be employed would be provided by the

Royal Society. With regard to the appliances which Her Majesty's Government may be asked to provide, the experience of the previous expedition will furnish adequate guidance, whenever the general scheme may be approved. It has appeared to the President and Council, that if the ship required for the proposed service could be provided by the temporary employment of one of Her Majesty's Surveying Vessels now in commission, anything beyond a trifling outlay on the part of the Government would be rendered unnecessary. "I remain,

"Your obedient Servant,

"W. Sharpey, M.D.,

"*The Secretary to the Admiralty.*" *Sec. R.S.*"

Resolved,—That a Committee be appointed to consider the Scientific Apparatus it will be desirable to provide for the proposed Expedition. The Committee to consist of the President and Officers, with Dr. Carpenter, Captain Richards, Mr. Siemens, Dr. Tyndall, and Sir Charles Wheatstone, with power to add to their number.

That a sum of £200 from the Government Grant be assigned to Dr. Carpenter for the further prosecution of Researches into the Temperature and Zoology of the Deep Sea.

March 18, 1869.

An oral communication was made by the Hydrographer to the effect that the Lords Commissioners of the Admiralty had acceded to the request conveyed in Dr. Sharpey's letter of Feb. 18; that H.M. Surveying-vessel 'Porcupine' had been assigned for the service; and that the special equipment needed for its efficient performance was proceeding under the direction of her Commander, Capt. Calver.

April 15, 1869.

Read the following Letter from the Admiralty:—

"Admiralty, 19 March, 1869.

"Sir,—With reference to previous correspondence, I am commanded by My Lords Commissioners of the Admiralty to acquaint you that Dr. Carpenter and his Assistants, who have been deputed by the Royal Society to accompany the Expedition about to be dispatched to the neighbourhood of the Faroe Isles for the purpose of investigating the bottom of the Ocean by means of deep-sea soundings, will be entertained whilst embarked on board the 'Porcupine' t the Government expense.

"I am, Sir,

"Your obedient Servant,

"W. G. Romaine."

"*The President of the Royal Society.*"

June 17, 1869.

Read the following Report:—

"The Committee appointed Feb. 18, 1869, to consider the Scientific

Apparatus it will be desirable to provide for the proposed Expedition for Marine Researches, beg leave to lay before the Council the following Report:—

"The chief subjects of Physical Enquiry which presented themselves as interesting on their own account, or in relation to the existence of Life at great depths, were as follows:—

"(1) The Temperature both at the bottom and at various depths between that and the surface.

"(2) The nature and amount of the dissolved Gases.

"(3) The amount of Organic matter contained in the water, and the nature and amount of the Inorganic salts.

"(4) The amount of Light to be found at great depths.

"Among these subjects the Committee thought it desirable to confine themselves in the first instance to such as had previously to some extent been taken in hand, or could pretty certainly be carried out.

"The determination of Temperatures has hitherto rested chiefly upon the registration of *minimum* Thermometers. It is obvious that the temperature registered by minimum thermometers sunk to the bottom of the sea, even if their registration were unaffected by the pressure, would only give the lowest temperature reached *somewhere* between top and bottom, not *necessarily* at the bottom itself. The temperatures at various depths might indeed, provided they nowhere increased on going deeper, be determined by a series of minimum thermometers placed at different distances along the line, though this would involve considerable difficulties. Still, the liability of the index to slip, and the probability that the indication of the thermometers would be affected by the great pressure to which they were exposed, rendered it very desirable to control their indications by an independent method.

"Two plans were proposed for this purpose, one by Sir Charles Wheatstone, and one by Mr. Siemens. Both plans involved the employment of a voltaic current, excited by a battery on deck; and required a cable for the conveyance of insulated wires. The former plan depended upon the action of an immersed Breguet's thermometer, which by an electro-mechanical arrangement was read by an indicating instrument placed on deck. The latter plan made the indication of temperature depend on the existence of a thermal variation in the electric resistance of a conducting wire. It rested on the equalization of the derived currents in two perfectly similar partial circuits, containing each a copper wire running the whole length of the cable, the sea, and a resistance-coil of fine platinum wire; the coil in the one circuit being immersed in the sea at the end of the cable, and that in the other being immersed in a vessel on deck, containing water the temperature of which could be regulated by the addition of hot or cold water, and determined by an ordinary thermometer.

"The instruments required in Sir Charles Wheatstone's plan were more expensive, and would take longer to construct; and besides, the Com-

mittee were unwilling to risk the loss of a somewhat costly instrument in case the cable were to break. On these accounts they thought it best to adopt the simpler plan proposed by Mr. Siemens; and the apparatus required for carrying the plan into execution is now completed, and in use in the expedition.

"Meanwhile a plan had been devised by Dr. Miller for obviating the effect of pressure on a minimum thermometer, without preventing access to the stem for the purpose of setting the index. It consists in enclosing the bulb in an outer bulb rivetted-on a little way up the stem, the interval between the bulbs being partly filled with liquid, for the sake of quicker conduction. The Committee have had a few minimum thermometers constructed on this principle, which have been found to answer perfectly. The method is described in a short paper which will be read to the Society to-morrow.

"For obtaining specimens of water from any depth to which the dredging extends, the Committee have procured an instrument constructed as to its leading features on the plan of that described by Dr. Marcet in the Philosophical Transactions for 1819, and used successfully in the earlier northern expeditions.

"Mr. Gwyn Jeffreys is now out on the first cruise of the 'Porcupine,' the vessel which the Admiralty have sent out for the purpose, and is accompanied by Mr. W. L. Carpenter, B.Sc. (son of Dr. Carpenter), who undertakes the general execution of the physical and chemical part of the inquiry. A letter has been received by the President from Mr. Jeffreys, who speaks highly of the zeal and efficiency of Mr. Carpenter. The thermometers protected according to Dr. Miller's plan, and the instrument for obtaining specimens of water from great depths, have been found to work satisfactorily in actual practice. Mr. Siemens's instrument was not quite ready when the vessel started on her first cruise, and was not on board when the above letter was written. The gas-analyses have been successfully carried on, notwithstanding the motion of the vessel. From a letter subsequently received from Mr. Carpenter, it appears that Mr. Siemens's apparatus, so far as it has as yet been tried, works in perfect harmony with the thermometers protected according to Dr. Miller's plan."

"June 16, 1869."

Resolved,—That the Report now read be received and entered on the Minutes.

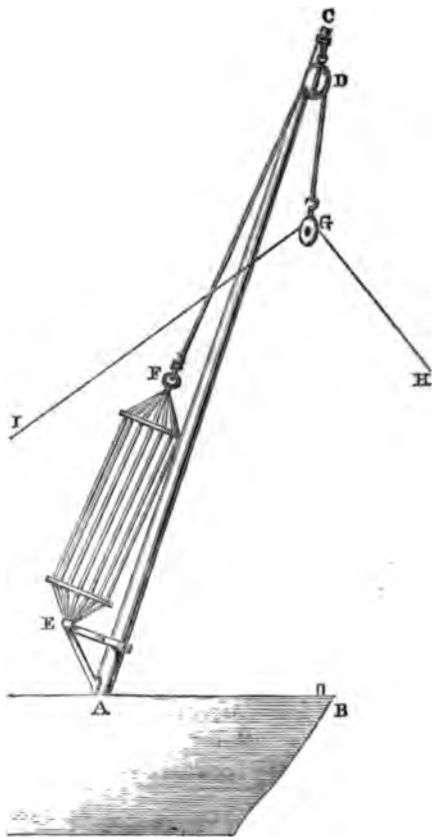
EQUIPMENT.

1. The equipment of the 'Porcupine' for the purposes of Deep-sea Sounding and Dredging was devised on the basis of the experience gained in previous Deep-sea Sounding Voyages (especially in that of the 'Hydra'*),

* Sounding Voyage of H.M.S. 'Hydra,' Captain P. R. Shortland, 1868; also Notes on Deep-Sea Soundings, by Staff-Commander Davis, 1867.

and in the 'Lightning' Expedition.—As it was considered advisable by Capt. Calver that provision should be made for carrying on Sounding and Dredging at either end of the ship, a "derrick" (A C) with an "accumulator" * (E F) was rigged out both at the bow and the stern, on the plan shown in the accompanying figure.

Fig. 1.



* The Accumulator is composed of a number of strong vulcanized India-rubber springs combined at their extremities E F; and its use is twofold,—*first*, to indicate by its elongation any excessive strain upon the sounding or dredging line I H, which passes through the block G; and *second* to ease off the suddenness of such strain, and give time for the action by which it may be relieved. This is especially valuable in Deep-sea Sounding and Dredging when the vessel is pitching; for the friction of two or three miles of immersed line is so great as to prevent its yielding to any sudden jerk, such as that given to its attached extremity by a vertical motion of a few feet when the vessel rises to a sea. And it is absolutely needful when Dredging is carried on from a vessel as large as the 'Porcupine'; since, whenever the dredge 'fouls,' the momentum of such

2. An ample supply of Sounding-line was provided, specially manufactured for the purpose; this line, made of the best Italian hemp, although no more than 0·8 inch in circumference, bears a strain of 12 cwt. For Soundings within 1000 fathoms' depth, it was found most convenient to employ an ordinary cylindrical Deep-sea Lead weighing 1 cwt., having at its base a conical cup for bringing up mud or sand from the bottom, which is furnished with a circular lid that falls down and closes it in when the lead strikes.—Above the Lead a Water-bottle (§ 19) was attached to the line, by which a sample of sea-water could be brought up from the bottom or from any intermediate depth. And above this again there were attached two or more Thermometers, enclosed in cylindrical copper cases having holes at the top and bottom through which the sea-water streams upwards as the lead descends.

3. The Sounding-lead with its appurtenances is allowed to descend as rapidly as it can carry out the line; but instead of descending at a constantly accelerating rate, it requires more time for every additional 100 fathoms; this retardation being due, not, as is popularly supposed, to an increase in the density of the water*, but to the friction of the sounding-line in its descent, which of course increases with every additional fathom that runs out. It is this friction that produces the chief strain upon the line when the Lead is being drawn up, and renders great caution requisite in regulating the rate of the reeling-in which is effected by the donkey-engine.

4. For the deeper Soundings, the 'Hydra' Apparatus was employed. The essential principle of this is the same with that of all the other forms of Deep-sea Sounding apparatus now in use; the weights or sinkers being so attached as to be let go by a mechanical contrivance when it touches the bottom, so that the line is relieved from the duty of raising them to the surface,—the rod or tube alone, with the water-bottle and thermometers, being brought up by it. For Soundings at depths of from 1000 to 1500 fathoms, *two* sinkers, each of 112 lbs., were employed; and for yet deeper soundings *three* were used. The peculiarity of the 'Hydra' apparatus consists partly in the mechanical contrivance for the detachment of the sinkers; and partly in the construction of the rod which carries them, this being a strong tube furnished with valves that open upwards, so as to allow the water to stream through it freely in its descent, whilst they enclose the mud or sand into which the tube is forced on striking the bottom before the sinker is detached†.

---

a vessel, however slowly it might be moving through the water, would cause the dredge-line to part, if the strain were sudden instead of gradual.

* This is so trifling, even at 2¼ *miles depth*, as not to equal the difference in density between fresh and salt water; being estimated by Dr. Miller at certainly not more than 1-47th of its volume, whilst sea-water of sp. gr. 1·027 is 1-37th heavier than fresh water.

† A detailed account of this Apparatus will be found in the "Sounding Voyage of H.M.S. 'Hydra,'" already referred to.

5. Every one of the numerous deep Soundings obtained in this Expedition was taken, not only under the superintendence, but actually by the hands, of Capt. Calver himself; of whose skill in the conduct of this operation (which often requires great nicety in the management of the vessel, so as to secure a good up-and-down direction of the line) it is enough to say that it is worthy of the distinguished Service to which he belongs, and to his high position in it. Not a single fathom of line has been lost, and not a single instrument has suffered damage, throughout the whole Expedition.

6. The Dredges supplied to the 'Porcupine' by the Admiralty were constructed upon the model of those which were found to work best in the 'Lightning' Expedition. The experience of the First Cruise, however, in which the dredging was carried down to more than twice the depth attained last year, led Capt. Calver to have a still heavier dredge constructed at Belfast, upon a somewhat different pattern; and it was with this that the very deep Dredgings of the Second cruise were executed, by which the condition of the sea-bottom was successfully investigated at a depth of 2435 fathoms (§§ 45–50)*. An ample supply of strong Dredge-rope was provided; and a very simple and convenient arrangement was devised by Capt. Calver for hanging this in coils upon pins attached to the inner side of the quarter-deck bulwarks (§ 46), so that the *three nautical* miles of line required for the deepest Dredging could be thus disposed without at all encumbering the deck, and in a manner which enabled it to be most conveniently handled both in paying-out and reeling-in, with the additional advantage of keeping it remarkably free from "kinks."

7. An important addition to the Dredging-apparatus, which was devised by Capt. Calver before the commencement of the Third cruise, will be described in its proper place (§ 63). The result of its employment was so extraordinary, that no deep dredging can hereafter be accounted of any value in which it has not been used; and it is only now to be regretted that the idea had not presented itself earlier, so as to have been carried out in the First and Second Cruises.

8. The whole of this equipment would have been ineffective if a suitable Donkey-engine had not been supplied for working it. The experience of the 'Lightning' had shown that a single-cylinder engine is not adapted for this purpose, being liable to stop at either end of its stroke when a heavy strain is put on the drum, and then moving onwards with a jerk, so as to throw on the line a tension which may very probably cause it to part. It was therefore urged upon the Authorities at Woolwich that a double-cylinder engine should be supplied to the 'Porcupine'; and a 'donkey' on this plan was accordingly fixed, which proved most efficient. Nothing could exceed the steadiness of its working, or the facility with which its speed and power could be regulated in accordance with the purposes to

* This is nearly equal to the height of Mont Blanc; and exceeds by more than 500 fathoms the depth from which the Atlantic Cable was brought to the surface.

which it was applied. With the drum ordinarily used it brought up on one occasion, from a depth of 767 fathoms, just *half a ton* of Atlantic mud, in a Dredge which, with its appurtenances, weighed 8 cwt.,—making 18 cwt. in the whole. This was not the limit of its capability; for by the substitution of a smaller drum a still greater power could be obtained,—of course at the sacrifice of speed; and it was by this means that the heaviest Dredge, containing $1\frac{1}{2}$ cwt. of Atlantic mud, was drawn up, by a rope of more than three nautical miles in length, from a depth of 2435 fathoms (§§ 45–50).

9. The working of the Dredge was superintended throughout by Capt. Calver, whose trained ability very early gave him so complete a mastery over the operation, that he found no difficulty in carrying it down to depths at which this kind of exploration would have been previously deemed out of the question. It is impossible for us to speak too highly of the skill he displayed, or too warmly of the sympathy he showed in our work. The placing the Dredge on a bottom nearly three miles from the surface, the working it while there, and the subsequent hauling of it in, with its precious sample of the Life of the Ocean-bed at that vast depth (all executed without the smallest failure, or even such a "hitch" as might have caused the loss of an entire day's work), is an achievement of which our Commander might well be proud, if pride were in his nature. That only one Dredge was lost during the whole Expedition affords ample proof alike of the excellence of his arrangements and of the unwearying assiduity with which they were carried into effective operation. We would here add that the other Officers of the 'Porcupine,' viz. Staff-Commander Inskipp, Mr. Davidson, and Lieut. Browning, most heartily and zealously seconded their Commander, in promoting alike the scientific objects of the Expedition and the welfare and comfort of all who were engaged in carrying them out.

10. With regard to the equipment of the Ship, it only remains to be added that the Chart Room was assigned for the Scientific work of the Expedition; and that the accommodation it afforded (though not all that could be desired) enabled Chemical Analyses and Microscopic observations to be carried on at the same time.

11. The provision of the Apparatus needed for the Physical and Chemical enquiries, which formed a special object of this Expedition, having been placed by the Council of the Royal Society (Minutes for Feb. 18) under the direction of a Committee "consisting of gentlemen practically conversant with the construction and working of such instruments," every arrangement was made which was considered expedient by the very eminent Authorities of which that Committee was composed. The general conclusions at which they arrived are embodied in the Report (p. 401) which they presented to the Council (Minutes of June 17th); but it seems desirable here to record in somewhat greater detail the nature

of the preliminary enquiries made, and the arrangements actually adopted.

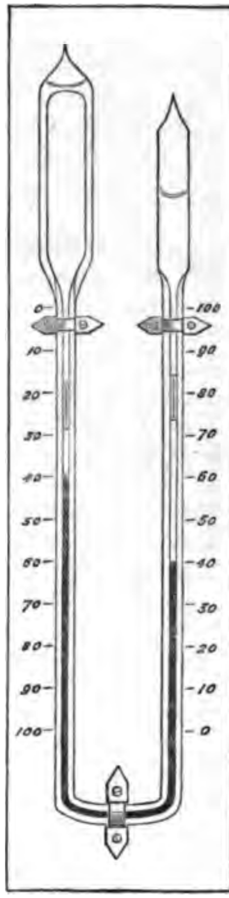
12. It had been remarked in the Report of the "Lightning Expedition" (Proceedings of the Royal Society, Dec. 17, 1868, p. 185) that while the existence of a *minimum* Temperature (probably that of the *bottom*) at least as low as 32° (0° Cent.), over a considerable area of which the depth was between 500 and 600 fathoms, had been conclusively established, the actual *minimum* might probably have been from 2° to 4° *below* that recorded by the Thermometers employed, the pressure of 100 atmospheres, to which their bulbs were subjected at a depth of about 535 fathoms*, being very likely to alter the capacity of the bulbs to that extent. "In any renewal of the enquiry," it was added, "it will be of course desirable that the Thermometric apparatus used should be specially protected from this source of error."

13. So soon, therefore, as there was reason to believe that the application of the Council of the Royal Society for such renewal would be acceded to by H.M. Government, steps were taken to determine the precise amount of this error, and to devise the best means of preventing it. After consultation between the Hydrographer, Dr. Carpenter, and Mr. Casella (the maker of Meteorological Instruments to the Admiralty), it was determined that an apparatus should be constructed on the principle of the Bramah Press; in which Thermometers immersed in water should be submitted to hydraulic pressure, which could be gradually raised till it reached *three tons on the square inch*, its amount being indicated by a pressure-gauge as the experiment proceeded. Mr. Casella further undertook to construct Thermometers with bulbs of extra thickness, in order that it might be ascertained whether the error arising from external pressure (if such should be proved to exist) could be kept in check by this simple expedient. The question was at the same time made the subject of consideration by a Committee appointed by the Council of the Royal Society, as set forth in the Minutes already cited (p. 402, 403); and it was determined that trial should be given to a plan proposed by Dr. W. A. Miller, which consists in the enclosure of the bulb of the Six's Thermometer (the form of Self-registering Thermometer that had been found by experience best adapted to Deep-sea Soundings) in a second or outer bulb, sealed around the neck of the stem,—the space between the inner and outer bulbs being nearly filled with alcohol, and the greater part of the air being displaced from the small unfilled space, by boiling the spirit before the outer bulb is sealed. In this manner the inner bulb is protected from the influence of variations in external pressure upon the outer, the only effect of which is to alter the capacity of the unfilled space; whilst changes of temperature in the medium surrounding the outer bulb are speedily transmitted to the fluid contained within the inner, by convection through the thin stratum

* The pressure of a column of Sea-water of 100 fathoms depth is 280 lbs. upon the square inch, or *one ton* for every 800 fathoms.

of alcohol interposed between the two*. Several Thermometers were constructed upon this plan by Mr. Casella; and these were tested in the pressure-apparatus, together with various instruments of the ordinary construction, as well as with instruments constructed by Mr. Casella with bulbs of extra thickness. A preliminary trial having indicated (1) that the effect of hydraulic pressure upon *ordinary* Thermometers (as shown by the rise of the maximum index) is always very considerable, though varying in amount according to the construction of the instrument, (2) that this effect cannot be prevented by an *increase in the thickness* of the bulb, and (3) that the rise of the *maximum* index in thermometers protected according to Dr. Miller's plan was comparatively trifling,—a series of comparisons between the "protected" and the "unprotected" instruments was very carefully conducted under the direction of Staff-Commander Davis of the Hydrographic Office; who, having had experience in Thermometric Soundings in Sir James C. Ross's Antarctic Expedition, felt specially interested in the determination of this question. In these experiments the difference between the ordinary *unprotected* Thermometers constructed by Mr. Casella for the Admiralty (by which the Temperature-Soundings had been taken in the 'Lightning' Expedition), and *protected* Thermometers constructed on the same pattern in every other respect, was carefully noted at gradually increasing pressures, so as to determine the amount of such difference at depths respectively corresponding to these pressures. The question whether the small elevation of the *maximum* index observed in the *protected* Thermometers is fairly attributable to an actual increment in the temperature of the water in which they are immersed, consequent upon the compression to which it is subjected during the experiment, was carefully considered by Dr. Miller (Proceedings, *loc. cit.*), who satisfied himself,

Fig. 2.



* See Dr. Miller's "Note upon a Self-registering Thermometer adapted to Deep-sea Soundings," in Proceedings of Royal Society, June 17, 1869, p. 482. The same principle had been previously applied in Thermometers constructed under the direction of Admiral Fitzroy, the space between the two bulbs, however, being occupied by mercury instead of spirit. But owing to some imperfection in the construction of these instruments, their performance was not satisfactory, and they were found very liable to fracture.

by experiments devised for the purpose, that this is the true account of it, and that the inner bulb of these Thermometers is not altered in capacity in any appreciable degree by a pressure reaching to *three tons* on the square inch *. This pressure was found to send up the maximum index of the best *unprotected* Thermometers made upon the Admiralty pattern as much as 10°; whilst a pressure of 2½ tons on the square inch sent up the index of an ordinary Phillips's maximum mercurial thermometer no less than 117°·5.

14. A considerable number of Thermometers by different makers, including six protected according to Dr. Miller's plan (all of them previously tested in the Pressure-apparatus), were supplied to the 'Porcupine' by the Meteorological Department; and during its earlier Cruises numerous comparative observations were made at different depths, with the view of determining the differences between the *protected* and various forms of *unprotected* Thermometers at gradationally increasing depths,—such differences being here of course due to *pressure only*. The records of these observations, having been transmitted to the Admiralty, were carefully reduced to curves by Capt. Davis, and compared with their differences at corresponding pressures in the Pressure-apparatus; with the result of showing (when due allowance was made for the small increment of Temperature in the experiments) such a close conformity, that it became obvious that the protected Miller-Casella thermometers might be thoroughly relied on for indicating the true temperature within 1° Fahr. under any pressure not exceeding that to which they had been tested,—this being equivalent to that of a column of Sea-water 2400 fathoms (4389 mètres) deep. This happens to be almost exactly that of the deepest Sounding taken in the 'Porcupine' Expedition, which was 2435 fathoms (4453 mètres).

15. The thorough reliableness of this instrument having been thus demonstrated, it was considered unnecessary to carry the *comparative* observations further; and in the last Cruise two *protected* Thermometers were alone employed. The excellence of these instruments may be inferred from the fact that they never differed more than a few tenths of a degree (Fahr.)†, and that after having travelled vertically downwards and upwards with the Sounding-apparatus to a total amount of nearly a hundred miles (one of the Soundings having been taken at a depth of nearly three miles, and

* [The results of a more elaborate series of investigations subsequently carried on by Staff-Commander Davis, which were communicated to the Royal Society, May 19, 1870, lead him to believe that the small elevation above alluded to is *not entirely* accounted for by increment of Temperature, and that it consequently indicates that *some* influence is still exerted on the inner bulb by Pressure on the outer. The elevation thus produced, however, does not in any case amount to 1° (Fahr.); and the error can be reduced to a scale, by the application of which it can be easily corrected.]

† These small differences are probably due to slight differences in the rate at which the instruments took the temperature of the water around; in which case the *lower* reading would be the most correct.—Before leaving Belfast on the Third Cruise, Prof. Wyville Thomson tested the condition of these Thermometers by immersing them in ice; and they both recorded exactly 32°.

several others at a depth of above two miles), they have been found to be in as good order as when they were first sent out by Mr. Casella. While this most satisfactory result is partly due to the careful handling of the apparatus by Capt. Calver, it is mainly attributable on the one hand to the excellence of the principle on which the Thermometers are constructed, and on the other to the admirable workmanship of Mr. Casella; for the records of previous Temperature-Soundings show that the fracture of the bulbs of unprotected Thermometers at great depths was a very common occurrence, whilst the record of observations made in the 'Lightning' Expedition* shows that the indications of Thermometers of less perfect construction often show a considerable discrepancy.

16. In concluding this account of the behaviour of the protected Miller-Casella Thermometers under the most trying conditions, it may be added that wherever the localities of the Temperature-Soundings taken with these instruments during the Third cruise of the 'Porcupine' were the same (or nearly so) with those of the Temperature-Soundings taken in the 'Lightning' Expedition, *their correspondence proved to be very close,* when the proper correction for the depths at which they were taken was applied to the latter (§ 95). Thus the *differences* of temperature between the Warm and the Cold Areas indicated by those observations† remained the same, although the Temperatures *recorded* by the "unprotected" Thermometers required to be reduced by from 2° to 3° to show the *actual* temperatures,—a recorded temperature of 46° at 650 fathoms in the Warm Area indicating a real temperature of 43°, while a recorded temperature of 32° at 550 fathoms in the Cold Area indicated a real temperature of about 29·8°.

17. As it was considered expedient by the Committee (p. 402) that a trial should be given to Mr. Siemens's apparatus for the determination of deep-sea temperatures, this apparatus (which he terms a Differential Thermometer) was fitted on board the 'Porcupine,' and provided with 1000 fathoms of a small cable about the size of the ordinary Sounding-line, which contained the two insulated wires necessary for the establishment of the two circuits to be brought into comparison. The indications of this instrument depend upon the equalization of two currents transmitted through resistance-coils of fine platinum wire; one of these coils being sent down at the end of the sounding-cable, whilst the other is immersed in a vessel on deck, the water in which can be gradually lowered in temperature by the addition of ice or the use of a freezing-mixture. When the equalization of the currents is shown by the galvanometer, the temperature of the water in the vessel on deck (indicated by an ordinary thermometer) will represent that of the stratum of the sea beneath, in which the resistance-coil is immersed at the time.—Nothing can be more perfect than the working of this apparatus when the Galvanometer rests on a fixed plane surface; and its accuracy and delicacy were satisfactorily proved by experiments carried on not

* Proceedings of the Royal Society, Dec. 17, 1868, p. 172, *notes.*

† Ibid. p. 188.

merely on shore, but also on board the 'Porcupine' when lying in dock or harbour. But it could not be worked with the Galvanometer supplied when there was the least roll of the vessel; for it was then found impossible to make the *zero* observations requisite to indicate equilibrium, though Mr. W. L. Carpenter (who had charge of the apparatus, p. 403) tried every expedient that circumstances admitted. It is obvious, therefore, that this instrument can only be made use of on board ship when the Galvanometer is so suspended as not to participate in the rolling or pitching of the vessel; and it is to be hoped that Mr. Siemens, with his well-known ingenuity, may be able to devise the means of accomplishing this.

18. It may be well here to mention that as it was found impracticable to employ Mr. Siemens's Differential Thermometer for the determination of the question whether the *minimum* temperature registered by the Thermometers is the actual *bottom* temperature, or is the temperature of some intermediate stratum, this was effected by taking *series* of Temperature Soundings with Thermometers sent down to successively increasing depths in the same locality. Such series were obtained in each of the Cruises; with the result, as will be shown hereafter (§94 *et seq.*), of not merely confirming the conclusion advanced in the 'Lightning' Report (p. 189) that the minimum temperature *is* that of the bottom, but of affording a set of most important data for a general doctrine of the interchange between Equatorial and Polar waters in the great Oceanic basins.

19. The next subject considered by the Scientific Committee was the feasibility of constructing a vessel which should fill itself with Water, either at the bottom or at any intermediate depth, as might be required; and which should bring such water to the surface without the loss of any of the Gases dissolved in it. This might be easily accomplished, were it not for (1) the expansion which water taken under great pressure undergoes when that pressure is removed, the force of which would be sufficient to burst the strongest vessel that could be made; and (2) the expansile force of the gases dissolved in it under great pressure, which would exert itself in the same direction. Various plans were suggested for meeting this difficulty; but it was considered that as time would not permit of the preparation of any but very easily constructed apparatus, it would be better on the present occasion to adopt a form of Water-Bottle suggested by the Hydrographer on the basis of the cylindrical copper cases used for the protection of deep sea thermometers, these having been found to bring up specimens of water whose turbid condition left no doubt that it had come from the stratum immediately covering the soft ocean-bottom. The Water-Bottle constructed on the Hydrographer's plan is a simple strong cylinder of brass, 26 inches long, and 2·3 inch in interior diameter, holding about 60 oz. of water. In the disk which closes it in at each end there is a circular aperture, into which a conical valve is accurately fitted. While this bottle is descending through the water with the Sounding-Apparatus, the valves readily yield to the upward pressure, and

a continuous current streams through it; but so soon as the descent is checked, either by the arrival of the apparatus at the bottom, or by a stop put on the line from above, the valves fall into their places, and thus enclose the water that may fill the bottle at the moment. The ex-

Fig. 3.

A. B.

Water-Bottle as seen at A externally, and at B in section; drawn to a scale of *one-eighth* the actual size.

pansion of this water and of its dissolved gases, as the bottle is brought to the surface, causes a pressure from within, which lifts the upper valve so as to permit the escape of whatever part of the contents of the bottle may be in excess of its capacity. The interior of the bottle was coated with varnish, to prevent the chemical action of the sea-water upon it.—The working of this very simple apparatus was found to be entirely satisfactory. Abundant evidence was obtained that, when it descended to the bottom, it brought up bottom-water: thus, in the area of the "Globigerina-mud," the water was slightly turbid, and deposited after a time a fine sediment (which might be removed by filtration), that proved to consist almost entirely of extremely minute *Globigerinæ*. And hence it may be fairly inferred that when its descent was checked at any intermediate point, the

water brought up in it would be derived from that stratum. Although it can scarcely be supposed that the whole amount of the gases dissolved in the very deep water is retained when the superincumbent pressure is removed, yet it may be inferred, from the slight excess which still usually presented itself, that the very deep water must include a greater proportion of gases than that taken at or near the surface.

20. A number of large glass bottles were provided, for bringing home samples of Sea-water taken in various localities and at different depths; and of these Dr. Frankland kindly undertook to make careful analyses, which should show not merely the proportions of its Saline constituents, but—what has recently come to be a point of most unexpected interest (§ 23)—the amount of Organic matter it may contain. The results of these analyses are stated in Appendix II.

21. But the determination of the nature and proportions of the dissolved Gases could only be effected by immediate analysis; and it was considered by the Committee that it would be expedient to attempt this, notwithstanding the difficulties which might be expected to arise from the motion of the vessel. A method devised by Dr. Miller, as most suitable to the circumstances, was carried into practical operation by Mr. W. L. Carpenter, who succeeded in working this apparatus so efficiently during the First Cruise, and obtained by means of it results of such singular interest, that it was considered desirable that the same system should be followed throughout the Expedition. This work was therefore committed in the Second Cruise to the charge of Mr. Hunter, Assistant to Prof. Andrews of Queen's College, Belfast; and it was carried on during the Third Cruise by Mr. P. Herbert Carpenter, according to the instructions he had received from Mr. Hunter, whom he had accompanied in the Second Cruise.—A general statement of the results obtained, which on the whole accorded well with each other, is included in the present Report (Appendix I.); particulars of the method employed, with details of the analytical results, and a fuller discussion of their *rationale*, will be furnished hereafter by Mr. W. L. Carpenter.

22. The accurate working of a Balance on board a ship at sea being obviously impracticable, the Specific Gravity of every specimen of Deep-sea-water brought up by the bottle was taken by Hydrometers specially constructed to indicate it within the required range to four places of decimals; and this was compared with the Specific Gravity of Surface-water. The determinations obtained by this method, however, of which the results are stated in Appendix I., cannot be regarded as equal in accuracy to those obtained by the Balance; and greater reliance, therefore, is to be placed on the Specific Gravities of the samples analyzed by Dr. Frankland (Appendix II.).

23. Further, tests devised by Dr. Angus Smith to determine the amount of Organic matter (1) in a non-decomposing, and probably therefore an assimilable state, and (2) in a state of decomposition, were frequently applied; with the remarkable result (Appendix I.), which has been since fully con-

firmed by the elaborate analyses of Dr. Frankland (Appendix II.), of indicating the universal presence of a highly Nitrogenous substance, such as may well be supposed to afford a direct supply of nutritive material to the Rhizopodic Fauna (*Sponges* and *Foraminifera*, with *Bathybius*?) of the Ocean-bottom, as was first suggested by Prof. Wyville Thomson in his Memoir on *Holtenia**.

24. For the management of the Dredging-operations two Assistants were appointed on the recommendation of Mr. Gwyn Jeffreys, under whom both of them had previously worked: Mr. Laughrin of Polperro, an old Coastguard-man, and an Associate of the Linnean Society, for dredging and sifting; and Mr. B. S. Dodd for picking out, cleaning, and storing the specimens collected. Both did their respective shares of the work carefully and zealously.

25. The Sieves were constructed under the direction of Mr. Jeffreys. These were five in number, and were "nested" or fitted one within another, with a strong handle of galvanized iron affixed to the bottom sieve on each side; so that the dredged material might pass through all the sieves at the same time, as they were worked in a large tub of sea-water on the deck. Their frames were of oak; and their lining was of copper wove-wire, the mesh of the top sieve being 2 holes to an inch, that of the next 4 holes, and of the succeeding sieves 8, 16, and 32. Each sieve was furnished with a beading round the inside rim, to prevent specimens remaining under the edges when the sieves were washed after each dredging; the risk of intermixture of specimens obtained from different dredgings was thus avoided.

26. Two other kinds of Sieve were also found useful.—One was spherical, with a lid fastened inside by bolts; its frame consisted of a strong network of copper ribs, which was lined with very fine wire-gauze of the same metal, and it had a ring through which a line would pass. Its use was to sift and wash away in the sea the impalpable mud got in large quantities at great depths; so as to leave only for examination all organisms exceeding in size 1-36th of an inch, this being the diameter of the mesh in the wire-lining. Some of the residuum or strained mud was likewise preserved, after sifting the material in the usual way. This contrivance, which we called the "globe-sieve," saved a great deal of the time and useless labour expended in washing dredged material of that viscid kind through the ordinary sieves in a tub of sea-water, which soon becomes so turbid, that unless the tub is continually emptied and refilled it is extremely difficult (if possible) to detect any specimens.—Another kind of sieve had a similar framework; but the body was semiglobose, with an open funnel-shaped neck. It was fastened to a long pole, and served for catching Pteropods, *Salpæ*, and other animals on the surface of the sea. This went by the name of the "scoop-sieve."

27. An ample supply of spirit, jars, and bottles was provided; and the

* Philosophical Transactions, 1869, p. 801.

most convenient storage-room was assigned for them that the small size of the vessel permitted.

28. The unexpected amount of the Collections made during each Cruise, and especially during the Third, put all these resources to a severe test; and it is satisfactory to be able to state that nothing was found wanting which could not be supplied at the ports at which the 'Porcupine' put in.

29. The work of the Expedition was distributed, according to the plan originally marked out, into Three Cruises: the *first* of which was under the Scientific charge of Mr. Jeffreys, who was accompanied by Mr. W. L. Carpenter; the *second* under the Scientific charge of Prof. Wyville Thomson, who was accompanied by Mr. Hunter; and the *third* under the Scientific charge of Dr. Carpenter, who had the advantage of the companionship of Prof. Wyville Thomson, as well as of his son Mr. P. Herbert Carpenter.—The ground assigned to the First and Second Cruises, however, was somewhat different from that originally proposed (p. 399). For as it was considered that the exploration of the "Porcupine Bank," which lies about 150 miles to the west of Galway, and beyond which the water rapidly deepens to 1500 fathoms, would be likely to afford results of great value, and would present a very suitable locality for ascertaining to what depths Dredging could be successfully carried down, it was arranged that this exploration, with that of the deep channel intervening between the British plateau and "Rockall Bank" should be the work of the First Cruise; and that in the Second Cruise this exploration should be carried on in a northerly and north-westerly direction, so as to be connected with the work which had been assigned to the Third Cruise, viz. the more thorough and extended exploration of the region traversed in the 'Lightning' Expedition.—It will be seen hereafter (§ 40) that it was by a change subsequently made in the direction of the Second Cruise that the most remarkable achievement in the whole Expedition was rendered possible.

NARRATIVE.

First Cruise. (Chart, Plate 4.)

30. The First Cruise of H.M.S. 'Porcupine' commenced on the 18th of May, and ended on the 13th of July. It comprised the Atlantic coasts of Ireland, from the Skelligs to Rockall (a distance of about $6\frac{1}{2}°$ or 450 miles), Loughs Swilly and Foyle on the north coast, and the North Channel on the way to Belfast. The first dredging was made on our way round from Woolwich to Galway, on the 24th of May, about forty miles off Valentia, in 110 fathoms; bottom sandy, with a little mud. The Fauna was mostly Northern; and the following are the more remarkable species then procured:—Mollusca: *Ostrea cochlear, Neæra rostrata, Verticordia abyssicola, Dentalium abyssorum, Aporrhaïs Serre-*

sianus, Buccinum Humphreysianum, Murex imbricatus, Pleurotoma carinata, and *Cavolina trispinosa.*—ECHINODERMATA: *Echinus elegans, Cidaris papillata,* and *Spatangus Raschi.*—ACTINOZOA: *Caryophyllia Smithii,* var. *borealis.* Of these, *Ostrea cochlear, Aporrhaïs Serresianus,* and *Murex imbricatus* are Mediterranean species; and *Trochus granulatus* also imparted somewhat of a Southern character, although that species was afterwards found living in the Shetland district. *Ostrea cochlear* is a small deep-water species of Oyster, and is one of the shells which M. Alphonse Milne-Edwards noticed adhering to the Telegraph-Cable between Sardinia and Algiers, at a depth of about 1100 fathoms (see 'Lightning Report,' p. 182); but it has been found (by Mr. Gwyn Jeffreys) attached to the columns of the Temple of Jupiter Serapis at Pozzuoli near Naples, which are reputed not to have been submerged to any considerable depth. The above results of this dredging will give a fair idea of the Fauna inhabiting the 100-fathom line on the West coast of Ireland.

31. After coaling at Galway we steamed southward, and (the weather being very coarse and unpromising) we dredged in Dingle Bay at a depth of from 30 to 40 fathoms; bottom rocky and muddy. As before, in comparatively shallow water, we had two dredges out, one at the bow and the other at the stern; as had been previously the practice of Mr. Jeffreys in his own yacht, when dredging at from 20 to 200 fathoms' depth. In Dingle Bay the dredges several times caught in rocks or large stones, but were saved by the usual yarn-stops, and by the extraordinary strength of the 2-inch Chatham rope which was used. On one occasion, when the dredge was fast, the vessel, which is nearly 400 tons' burden, was pulled round and swung by the rope, as firmly as if she were at anchor and moored by a chain-cable. Here, again, the Mollusca were mostly Northern:—*Siphonodentalium Lofotense, Chiton Hanleyi, Tectura fulva, Odostomia clavula, Trophon truncatus,* and *Cylichna nitidula* fall within this category; while *Eulima subulata, Trophon muricatus, Pleurotoma attenuata,* and *Philine catena* may be reckoned Southern species. But the most remarkable shell obtained in this dredging was *Montacuta Dawsoni,* a species which had been described and figured by Mr. Jeffreys, from specimens found by Mr. Robert Dawson in the Moray Firth. Of this species specimens were subsequently detected by Mr. Jeffreys in the Royal Museum at Copenhagen, in the collection of Greenland shells made by the late Dr. H. P. C. Möller, as well as in Professor Torell's collection of Spitzbergen shells at Lund. The species had been briefly described and noticed by Dr. Möller in the addenda to his 'Index Molluscorum Grœnlandiæ,' as a "Testa bivalvis;" but he did not give it any other name. The size of the Greenland and Spitzbergen specimens is considerably greater than that of British specimens; thus adding another to the numerous cases of a similar kind which have from time to time been adduced by Mr. Jeffreys as justifying his statement that of those species of Mollusca

which are common to Northern and Southern latitudes, and which inhabit the same bathymetrical zone, the Northern are usually larger than the Southern specimens. It may perhaps be a not unfair inference that the origin of such species is Northern, and that they dwindle and become depauperated in proportion to the distance to which they have migrated or been transported from their ancestral homes.

32. The next week was occupied in sounding and dredging off Valentia and on the way to Galway, at depths varying from 85 to 808 fathoms (Stations 2 to 7). The Fauna throughout was Northern; and several interesting acquisitions were made in all departments of the Invertebrata. Among these may be mentioned:—MOLLUSCA: *Nucula pumila* (Norway), *Leda frigida* (Spitzbergen and Finmark), *Verticordia abyssicola* (Finmark), *Siphonodentalium quinquangulare* (Norway and Mediterranean), and an undescribed species of *Fusus*, allied to *F. Sabini*.—ECHINODERMATA: the remarkable *Brisinga endecacnemos*, hitherto only known as a Northern form.—ACTINOZOA: *Flabellum laciniatum*, Edw. and J. Haime = *Ulocyathus arcticus*, Sars (Norway and Shetland, as well as a Sicilian fossil), of which rare and delicate coral unusually perfect specimens were obtained. That fine Shetland Sponge *Phakellia ventilabrum* was also met with thus far south, in 90 fathoms. Many of the most marked types of the deep-water CRUSTACEA of the Shetland sea were here dredged; while in company with these were *Gonoplax rhomboïdes*, Fab., a well-known Mediterranean species, an undescribed and very fine *Ebalia*, a new species of the Mediterranean genus *Ethusa*, together with numerous *Mysidea*, *Cumacea*, and *Amphipoda* new to our Fauna. *Cyprinididæ* also were abundant on this ground. The 808 fathoms' dredging was then a novelty, being (as we believed) the greatest depth ever explored in that way. The length of rope paid out was 1110 fathoms, and the time occupied in hauling in was fifty-five minutes. The same proportionate time was observed in other dredgings during this cruise, viz. five minutes for every 100 fathoms of rope. The dredge contained about two hundredweight of soft and sticky mud, in appearance resembling "China clay." The animals brought up on this occasion were quite lively. More than one specimen was examined of a small Gastropod (described and figured by Mr. Jeffreys as *Lacuna tenella*), which had very conspicuous eyes. There was also a young and active specimen of the large Norwegian Crab, *Geryon tridens*, Kröyer, which is very rare in the Scandinavian seas, and was the only North European Brachyuran which had not as yet been found in British waters. —We had here, for the first time, an opportunity of comparing the temperatures indicated by Dr. Miller's "protected" Thermometers, and those of the ordinary construction, at a considerable depth. The *minimum* recorded by one of the former was 41°·4, whilst that recorded by one of the best ordinary thermometers was 45°·2. As this difference of 3°·8 was almost exactly what the results of the experiments previously made had indicated as the effect of a pressure amounting to *one ton* on the square inch

(the pressure of a column of sea-water at 800 fathoms' depth), this close coincidence gave us a feeling of great confidence in the practical working of the "protected" instrument.

33. We next applied ourselves to the examination of the sea-bed between Galway and the Porcupine Bank, as well as beyond the Bank, at depths ranging from 85 to 1230 fathoms (Stations 10 to 17). All the Mollusca were Northern, except *Aporrhaïs Serresianus*; and even that we are now inclined to consider identical with *A. Macandreæ*, which inhabits the coasts of Norway and Shetland, the latter appearing to be a dwarf variety or form. The more remarkable species were, among MOLLUSCA, *Limopsis aurita* (a well-known tertiary fossil), *Arca glacialis*, *Verticordia abyssicola*, *Dentalium abyssorum*, *Trochus cinereus*, *Fusus despectus*, *F. Islandicus*, *F. fenestratus*, and *Columbella haliæeti* (a tertiary fossil); among ECHINODERMATA, *Cidaris papillata* and *Echinus Norvegicus*; and the fine branching Coral *Lophohelia prolifera*. In the deepest dredging made in this part of the cruise (Station 17, 1230 fathoms), in which the *minimum* temperature (shown by subsequent inquiry to be that of the bottom) was 37°·8, there occurred several new species and two new genera of the *Arca* family, *Trochus minutissimus* of Mighels (a North-American species) having two conspicuous eyes, a species of *Ampelisca* (Crustacean) with the usual number of four eyes, comparatively gigantic *Foraminifera*, and siliceous *Polycystina*. The FORAMINIFERA obtained in these and previous dredgings in deep water were of great interest. A large proportion of them belonged to the *Arenaceous* group, in which the calcareous shell is replaced by a "test" formed of agglutinated sand-grains; and of this group a large number of new types presented themselves, many of them very remarkable both for size and complexity of structure. The *Miliolines*, as in the 'Lightning' dredgings in the Warm area, were of exceedingly large size; and the *Cristellarians* were both large and varied in form, their axis of growth presenting every gradation from the rectilineal to the spiral. An enormous Fish (*Mola nasus*), which is not uncommon on the coasts of Upper Norway, was slowly swimming or floating on the surface of the sea; but we did not succeed in capturing it, for want of a harpoon.

34. We then put into Killibegs, Co. Donegal, and coaled there for our trip to Rockall, which is an isolated and conical rock, standing out of the Atlantic in Lat. 57° 35′, and Long. 13° 41′, at least 200 miles from the nearest land. In anticipation of this trip requiring a clear fortnight, coals were stacked on the deck, in addition to the usual stowage in the bunkers, so as to provide a sufficient supply. Some delay was caused by the non-arrival of a proper galvanometer to work Mr. Siemens's electro-thermometric apparatus, which we were anxious again to try.—We left Donegal Bay on the 27th of June, and returned to the mainland on the 9th of July, after dredging during seven days at depths exceeding 1200 fathoms, and on four other days at less depths. The greatest depth

reached was 1476 fathoms (Station 21). In this last-mentioned dredging we got several living *Mollusca* and other animals, a stalk-eyed *Crustacean* with two prominent and unusually large eyes, and a *Holothurian* of a lilac colour. The bottom at the greater depths consisted of a fine clayey mud, which varied in colour (in some cases being brownish, in others yellow, cream-colour, or drab, and occasionally greyish), and invariably having a greater or less admixture of pebbles, gravel, and sand. The upper layer formed a flocculent mass, which appeared to be animal matter in a state of partial decomposition. This was in all probability derived from the countless multitude of *Salpæ*, oceanic *Hydrozoa*, *Pteropods*, and other gelatinous animals, which literally covered the surface of the sea and filled our towing-net directly it was dipped overboard, and of which the remains must fall to the bottom after death. Such organisms doubtless afford a vast store of nutriment to the inhabitants of the deep.

35. Dredging in such deep water is not accomplished without difficulty. The dredge must be unusually heavy, to overcome the resistance to its sinking occasioned by the friction of the immense length of dredge-line paid out; and when it reaches the bottom, it sinks by its own weight into the mud, like an anchor. This would give only the same result as the cup-lead or any sounding-machine, but on a larger scale; and it would tell us very little about the Fauna. Further, if by the drift-way of the vessel, or by a few turns of the engine now and then, we are enabled to scrape the surface of the sea-bed, the dredge gets choked up with the flocculent mass above described. The fertile ingenuity of our experienced and excellent Commander devised a method which was a great improvement in deep-sea dredging, and which enabled us to obtain at least a sample of the substratum. This consisted in attaching to the rope two iron weights, each of 100 lbs., at a distance of 300 or 400 fathoms from the dredge (when the depth exceeded 1200 fathoms), so as to dredge from the weights instead of from the ship; the angle thus made caused the blade of the dredge to lie in its proper position. This method, in fact, reduced the working depth, by the distance of these weights from the vessel, to the easy and manageable limit of 300 or 400 fathoms. Another contrivance was to fasten the bag to the dredge in such a way that when it was hauled in, it could be unlaced, emptied, and afterwards washed quite clean. By this mode we were assured that the specimens really came from the place where each dredging was made. We tried on this and other occasions a contrivance devised by Mr. Easton, the eminent engineer, consisting of gutta-percha valves closing inwards in a wedge-like form, which were fitted to the mouth of the dredge. The object was to retain the contents of the dredge while it was being hauled in; as we had found by frequent and disappointing experience that a large portion of the contents generally escape through the mouth during this part of the dredging operation. This contrivance, though theoretically admirable, was found not to answer in practice, because the mouth of the dredge was so closed by the valves that

it had no contents to be retained. The principle, however, seems so good, that we should hope it may be more successfully applied.

36. The very deep dredgings in this trip yielded an abundance of novel and most interesting results in every division of Invertebrata. Among the MOLLUSCA were valves of an imperforate Brachiopod, with a septum in the lower valve, which we propose to name *Atretia gnomon*. Some shells were of a considerable size; and the fry of *Isocardia cor* (*Kelliella abyssicola*, Sars) were not uncommon. Among the CRUSTACEA there were new species of *Cumacea*; a beautiful *Amphipod* of a bright red colour, with feathery processes of a golden colour at the tail; with a considerable variety of *Isopoda*, *Phyllopoda*, and *Ostracoda*, among them several forms apparently new. There was also a magnificent *Annelid*, of a purplish hue, with purplish-brown spots on the line of segmentation. Two or three young specimens were here obtained, at a depth of 1215 fathoms (Station 28), of a most interesting *Clypeastroid*, of which a mature example was afterwards dredged in the Third Cruise (§ 77). These were at once recognized as belonging to an entirely new type; but since our return we find that a form, generically if not specifically the same, had been obtained by Count Pourtales during his last dredgings in the Gulf of Mexico, and had been described by Prof. Alex. Agassiz under the name *Pourtalesia miranda*. This type is of extraordinary interest from its being the living representative of a very singular little group of the *Ananchytidæ* (including the genus *Infulaster*, D'Orb., to which it seems most closely allied), which are specially characteristic of the newer Chalk. In the 1443 fathoms' dredging (Station 20) a *Holothurian* was obtained 5 inches long and 2¼ inches in circumference. Several very fine *Corals* were obtained during the Rockall trip; among them magnificent examples of *Lophohelia prolifera* and *Caryophyllia Smithii*. The *Foraminifera*, as before, were remarkable for their size, the same types being generally predominant. But specimens were here obtained for the first time of a peculiarly interesting *Orbitolite*, a type not hitherto discovered further north than the Mediterranean, and there attaining a comparatively small size. Perfect specimens of this *Orbitolite* must have a diameter of a sixpence; but owing to its extreme tenuity, and to the facility with which the rings separate from each other, no large specimens were obtained unbroken, though it was evident that their fracture had taken place in the process of collection. No greater proof can be adduced of the extreme *stillness* of the bottom at great depths, than is afforded by the extraordinary delicacy of these disks, which are so fragile as to be with difficulty mounted for observation. Their plan of growth corresponds with that of the "simple type" of this genus, all the "chamberlets" being on the same plane; but the form of the chamberlets corresponds with that of the chamberlets of the superficial layers of the "complex type" *. It is a fact of peculiar significance that instead

* See Dr. Carpenter's "Researches on the *Foraminifera*," Part I., in the Philosophical

of commencing with a "central" and "circumambient" chamber, like ordinary Orbitolites, this type commences with a *spire* of several turns, precisely like that of a young *Cornuspira*, thus showing the fundamental conformity of this *cyclical* type to the *spiral* plan of growth.—The animals, especially Mollusca, were by no means lively when brought on board and examined; perhaps this was owing to the great change of temperature (sometimes as much as 20°) between that of the sea-bed and that of the atmosphere.

37. A very elaborate Series of Temperature-soundings was made in the deepest parts of the sea traversed between the N.W. of Ireland and Rockall Bank, so as to enable us to determine the rate of diminution of temperature with increase of depth (see Table, p. 465). Thus at Station 19, at which the depth was 1360 fathoms, the temperatures were taken at 250, 500, 750, 1000, and 1360 fathoms, and showed a progressive though by no means uniform descent to the *minimum* recorded, which was $37^\circ{\cdot}4$; the most rapid change was between 500 and 750 fathoms. A similar Series, taken at Station 20, where the depth was 1443 fathoms and the bottom-temperature $37^\circ{\cdot}0$, and a third taken at Station 21, where the depth was 1476 fathoms, and the bottom-temperature $36^\circ{\cdot}9$, showed a very close accordance with each other and with the preceding. In another Series taken at Station 22 in 1263 fathoms, a careful comparison was made between the temperatures recorded by *two* "protected" thermometers and *six* ordinary thermometers; and the average error of these, which was very nearly 6° at the greatest depth, corresponded very closely with that indicated by the previous experiments at pressures answering to the several depths at which the observations were made.—The curious observation was made at Station 23, very near the Rockall Bank, that whilst the *minimum* indicated was $43^\circ{\cdot}4$ at a depth of 630 fathoms, the *maximum* index of both thermometers had risen to $74^\circ{\cdot}8$, or more than 17° above the surface-temperature. As in no other instance had any temperature been indicated higher than that of the surface, it seemed clear that a warm submarine spring must discharge itself in this locality. Circumstances prevented us, however, from ascertaining any further particulars in regard to it.

38. While we lay-to within a quarter of a mile from Rockall on the evening of Saturday the 3rd of July, fishing-parties were formed, and continued their sport until midnight. The rock was inhabited by a multitude of sea-fowl; and a large gannet perched on the highest pinnacle, looking like a sentinel or the president of the feathered republic.

39. At a distance of from 130 to 140 miles from the nearest part of the Irish coast we observed quantities of floating Seaweed (mostly *Fucus serratus*), and the feathers of sea-fowl covered with *Lepas fascicularis* and occasionally *L. sulcata*; and on the seaweed were also two kinds of sessile-

---

Transactions for 1855, p. 193 *et seq.*; and his "Introduction to the Study of the *Foraminifera*," p. 106 *et seq.*

eyed Crustaceans. The wind having been previously easterly, it is difficult to say what share the wind or tide had in the drift; but it could scarcely have been caused by any circulation from the equator. The Fauna nowhere showed the least trace of that wonderful and apparently restricted current known as the Gulf-stream. The beautiful Pteropod *Clio pyramidata* flitted about in considerable numbers; a delicate Cuttlefish (*Leachia ellipsoptera*), which is supposed to prey on *Salpæ*, was caught in the scoop-sieve, as well as several specimens of a small and very slender *Syngnathus* or pipefish. On our homeward passage we experienced severe weather, in which our vessel sustained some injury from the heavy cross seas which struck her. After putting into Killibegs we dredged in Lough Swilly, Lough Foyle, and the North Channel on the way to Belfast, where we arrived on the 13th of July.

Second Cruise. (Chart, Plate 5.)

40. As already stated, it was the original intention to devote the Second Cruise to the exploration of an area to the west of the outer Hebrides, between Rockall and the south-western limit of last year's work in the 'Lightning.' During the First Cruise, however, dredging had been carried down successfully to a depth of nearly 1500 fathoms; and the result so far realized our anticipations, and confirmed the experience of last year. The conditions (to that great depth at all events) were consistent with the life of all the types of Marine Invertebrata; though undoubtedly in very deep water the number of species procured of the higher groups was greatly reduced, and in many cases the individuals appeared to be dwarfed. From these observations (which thoroughly corroborated those of Dr. Wallich and others, about which there had been some difference of opinion on account of the imperfection of the appliances at the command of the observers), we concluded that probably in no part of the ocean were the conditions so altered by Depth as to preclude the existence of Animal Life,—that Life had no Bathymetrical limit. Still we could not consider the question thoroughly settled; and when, upon consultation with Captain Calver, we found him perfectly ready to attempt any depth, and from his previous experience sanguine of success, we determined to apply to the Hydrographer to sanction an attempt to dredge in the deepest soundings within our reach, viz. 2500 fathoms indicated on the chart 250 miles west of Ushant. The deepest reliable soundings do not go much beyond 3000 fathoms; and we felt that if we could establish the existence of Life, and if we could determine the conditions with accuracy down to 2500 fathoms, the general question would be virtually solved for all depths of the ocean, and any further investigation of its deeper abysses would be mere matter of curiosity and of detail. The Hydrographer cordially acquiesced in this change of plan; and on the 17th of July the 'Porcupine' left Belfast under the scientific direction of Professor Wyville Thomson; Mr. Hunter,

F.C.S., Chemical Assistant in Queens College, Belfast, taking charge of the examination and analysis of the sea-water.

41. The weather was very settled. On the Sunday, as we steamed down the Irish Channel, there was nearly a dead calm, a slight mist hanging over the water, and giving some very beautiful effects of coast scenery. On the evening of Sunday the 18th we anchored for the night off Ballycottin, a pretty little port about fifteen miles from Queenstown, and dropped round to Queenstown on Monday morning, where we anchored off Haulbowline Island at 7 A.M. At Queenstown Mr. P. Herbert Carpenter joined Mr. Hunter in the laboratory, to practise under his direction the gas-analysis, which it had been arranged that he should undertake during the Third Cruise. Monday the 18th was employed in coaling and procuring in Cork some things which were required for the chemical department; and at 7 P.M. we cast off from the wharf at Haulbowline and proceeded on our voyage.

42. During Monday night we steamed in a south-westerly direction across the mouth of the Channel. On Tuesday we dredged in 74 and 75 fathoms on the plateau which extends between Cape Clear and Ushant, on a bottom of mud and gravel with dead shells and a few living examples of the generally diffused species of moderate depths. The weather was remarkably fine, the barometer 30·25 in., and the temperature of the air 72°·5.

43. On Wednesday, July 21st, we continued our south-westerly course, the chart indicating during the earlier part of the day that we were still in the shallow water of the plateau of the Channel. At 4.30 A.M. we dredged gravel and dead shells in 95 fathoms, but towards mid-day the lead gave a much greater depth; and in the afternoon, rapidly passing over the edge of the plateau, we dredged in 725 fathoms with a bottom of muddy sand (Station 36). This is about the bathymetrical horizon at which we find the Vitreous Sponges in the northern area; and although the bottom is here very different, much more sandy with but a slight admixture of *Globigerina* ooze, we dredged a tolerably perfect, though dead, specimen of *Aphrocallistes Bocagei*, a vitreous sponge lately described by Dr. E. Perceval Wright from a specimen procured by Professor Barboza du Bocage from the Cape-Verde Islands, and one or two small specimens of *Holtenia Carpenteri*. The muddy sand contained a considerable proportion of gravel and dead shells.

44. On Thursday, July 22nd, the weather was still remarkably fine. The sea was moderate, with a slight swell from the north-west. We sounded in lat. 47° 38′ N., long. 12° 08′ W., in a depth of 2435 fathoms (Station 37). The Sounding-line used on this occasion was medium No. 2, of the best Italian hemp, the No. of threads 18, the weight per 100 fathoms 12 lbs. 8 oz., the circumference 0·8 inch, and the breaking-strain, dry, 1402 lbs., soaked a day, 1211 lbs.; and the 'Hydra' sounding-instrument was weighted with 336 lbs. The weight attached to the sounding-apparatus is of course allowed to descend quite freely without any check, but its velocity is

gradually and uniformly reduced during its descent by the increasing friction of the lengthening line. The uniformity of this retardation gives an infallible test of the success of the sounding, and a certain indication of the moment when the weight reaches the bottom. The latter was, however, valuable only for corroboration, as even at these enormous depths the shock of the arrest of the weight on the bottom, nearly three miles down, was distinctly perceptible to the skilled hand of our Commander. As the scientific value of our results depends upon the certainty of the determination of the depths, we subjoin a Table of the absolute rate of the descent of the weight in this sounding,—probably the deepest hitherto made which is thoroughly reliable, having been taken with the most perfect appliances, and with consummate skill.

Fathoms.	Time.	Interval.	Fathoms.	Time.	Interval.
	h m s	m s		h m s	m s
0	2 44 20		1300	58 5	1 23
100	45 5	45	1400	2 59 37	1 32
200	45 45	40	1500	3 1 9	1 32
300	46 30	45	1600	2 42	1 33
400	47 25	55	1700	4 19	1 37
500	48 15	50	1800	6 6	1 47
600	49 15	1 0	1900	7 53	1 47
700	50 24	1 9	2000	9 40	1 47
800	51 23	59	2100	11 29	1 49
900	52 45	1 22	2200	13 24	1 55
1000	54 0	1 15	2300	15 23	1 59
1100	55 21	1 21	2400	17 15	1 52
1200	56 42	1 21	2435	17 55	40

The whole time occupied in descent was 33 minutes 35 seconds; and in heaving up 2 hours 2 minutes. The cylinder of the sounding-apparatus came up filled with fine grey Atlantic ooze, containing a considerable proportion of fresh shells of *Globigerina*. The two Miller-Casella thermometers, Nos. 100 and 103, attached as usual to the line above the sounding-instrument, registered a minimum temperature of 36°·5 F. (2°·5 C.).

45. A Dredge was sent down at 5.45 P.M.; and as this was the deepest haul, the one which tested our resources most fully, and which seemed to us to prove that dredging could, with sufficient care and skill, be successfully carried out in any known depth in the ocean, we give here the details of the operation and the appliances used.—The dredge was of wrought iron, made on exactly the same plan as the "Naturalists'" dredge introduced by Ball and Forbes. The two scrapers were pitched at a very low angle. The arms were moveable, and about half of each arm next the eye to which the rope was attached was of strong chain: we are by no means sure, however, that this was an advantage. On one side the chain was attached to the arm of the dredge by a stop of five turns of spun yarn, so that in case of the dredge becoming entangled, or wedged by rocks or stones, a strain less than sufficient to break the dredge-rope would break the stop, alter the position of the dredge, and probably enable it

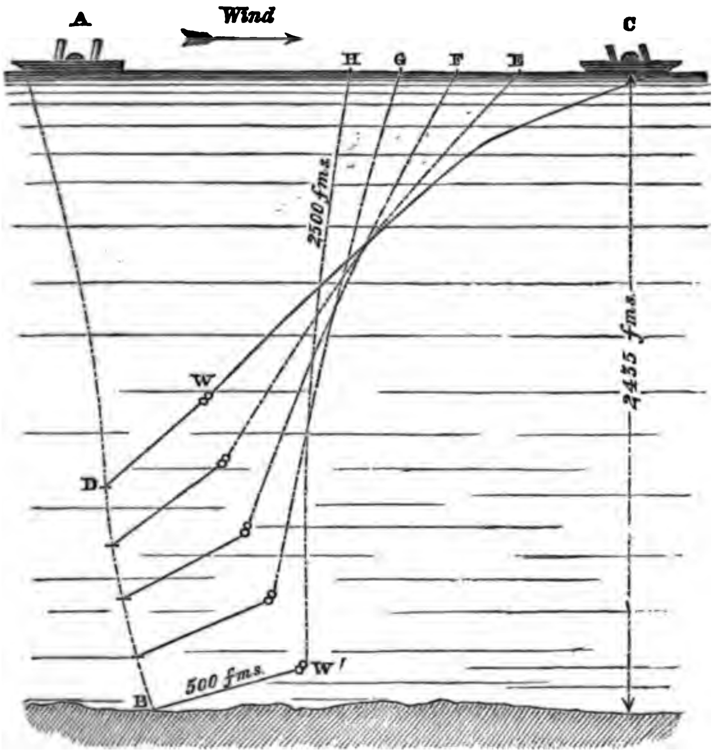
to free itself. The weight of the frame of the dredge was 225 lbs.; the mouth was about 4 feet 6 inches long by 6 inches wide at the throat or narrowest part, at the inner edge of the scrapers. The dredge-bag was double; the outer bag of strong twine netting, the meshes of the net ¾ inch in diameter; the inner of "bread-bag," a coarse open canvas. By an ingenious device of Captain Calver, the inner bag was divided into a set of compartments by pieces of plank fitted vertically into it from the mouth nearly to the bottom. This arrangement was intended to prevent the washing out of the contents of the dredge during its long upward journey.

46. The length of the dredge-rope was 3000 fathoms, nearly 3½ statute miles; of this 2000 fathoms were "hawser-laid" 2½ inches, with a breaking strain of 2¼ tons. The 1000 fathoms next the dredge were "hawser-laid" 2 inches. There was an admirable arrangement for stowing the rope—an arrangement which made its manipulation singularly easy, notwithstanding its great bulk and weight (about 5500 lbs.). A long row of large iron pins, about 2 feet in length, projected, rising obliquely from the top of the bulwark, along one side of the quarter-deck. Each of these held a coil of from 200 to 300 fathoms, and the rope was coiled continuously along the whole row. While the dredge was going down, the rope was rapidly taken by the men from these pins ("Aunt Sallies" we called them, from their each ending over the deck in a smooth white wooden ball) in succession, beginning with the one nearest the dredging-derrick; and in hauling up, a relay of men carried the rope along from the surging-drum of the donkey-engine, and hung it in coils on the pins in inverse order. A heavy spar formed a powerful derrick projecting over the port bow. A large block was suspended at the end of the derrick by a rope, which was not directly attached to the end of the derrick, but passed through an eye, and was fixed to a "bitt" on the deck. On a bight of this rope, between the "bitt" and the block, was lashed the "accumulator" described above (§ 1). The result of this arrangement was, that when any undue strain came upon the dredge-rope, the strain acted first upon the "accumulator;" and a graduated scale on the derrick, against which the "accumulator" played, gave, in cwts., an approximation, at all events, to the strain upon the rope. In letting go, the rope passed to the block of the derrick directly from the "Aunt Sallies;" in hauling up it passed from the block to the surging-drum of the admirable double-cylinder donkey-engine already mentioned, from which it was taken by the men and coiled on the "Aunt Sallies." Three sinkers were attached to the dredge-rope, one of 1 cwt., and the others of 56 lbs. each, at 500 fathoms from the dredge.

47. The 3000 fathoms of rope were out at 5.55 P.M., the vessel drifting slowly before a moderate breeze (force=4) from the N.W. The accompanying woodcut gives an idea of the various relative positions of the dredge and the vessel, according to the plan of dredging followed by Capt. Calver, which answered admirably.

A represents the position of the vessel when the dredge is let go, and the dotted line A B the line of descent of the dredge, rendered oblique by the tension of the rope. While the dredge is going down, the vessel drifts gradually to leeward; and when the whole (say) 3000 fathoms of rope are out, C, W, and D might represent the relative positions of the vessel, the weight attached 500 fathoms from the dredge, and the dredge itself.

Fig. 4.



The vessel now steams slowly to windward, occupying successively the positions E, F, G, and H. The weight, to which the water offers but little resistance, sinks from W to W', and the dredge and bag move slowly from D to B. The vessel is now allowed to drift back before the wind from H towards C. The tension of the motion of the vessel, instead of acting immediately upon the dredge, now drags forward the weight W', so that the dredging is carried on from the weight, and not directly from the vessel. The dredge is thus quietly pulled along, with its lip scraping the bottom in the attitude which it assumes from the position of the centre of weight of its iron frame and arms. If, on the other hand, the weights were hung close to the dredge, and the dredge were dragged directly from

the vessel, owing to the enormous weight and spring of the rope, the arms would be constantly lifted up, and the lip of the dredge prevented from scraping.—For very deep dredging, this operation of steaming up to windward till the dredge-rope was nearly perpendicular, after drifting for half an hour or so to leeward, was usually repeated three or four times.

48. At 8.50 P.M. we began to haul in, and the "Aunt Sallies" to fill again. The engine delivered the rope steadily at a uniform rate of rather more than a foot per second.—It is worthy of record that, except on one or two occasions, when an enormous load, at one time nearly a ton, came up in the dredge-bag, the donkey-engine maintained the same rate of heaving during the whole summer's work. A few minutes before 1 A.M. the weights appeared; and at one in the morning, 7¼ hours after it was cast over, the dredge was safely hauled on deck, having in the interval accomplished a journey of upwards of eight statute miles. The dredge-bag contained 1½ cwt. of the very characteristic pale grey Atlantic ooze. The total weight brought up by the engine was—

2000 fathoms 2½-inch rope	4000 lbs.
1000 fathoms 2-inch rope	1500 lbs.
	5500 lbs.
Weight of rope reduced to one quarter in the water=	1375 lbs.
Dredge and bag	275 lbs.
Ooze brought up	168 lbs.
Weight attached	224 lbs.
	2042

49. The Dredge, with its contents, was reverently laid aside under a tarpauling, and the watchers threw themselves down to rest till daylight. The contents of the dredge had the ordinary character of Atlantic chalk-mud. Since our return it has been analyzed by Mr. Hunter, who finds it to contain (besides an appreciable quantity of Organic matter, the exact proportion of which has not yet been accurately ascertained)—

Silica	23·34
Ferric oxide	5·91
Alumina	5·35
Carbonate of calcium	61·34
Carbonate of magnesium	4·00
Loss	0·06
	100·00

the alkalies having been removed by washing.—The ooze has not yet been subjected to careful Microscopic examination. The dredge appeared to have dipped pretty deep into the soft mud; its contents therefore contained but a small proportion of fresh shells of *Globigerina* and *Orbulina*.

There was an appreciable quantity of diffused amorphous organic matter, which we were inclined to regard as connected, whether as processes, or "mycelium," or germs, with the various shelled and shelless Protozoa.

50. On careful sifting, the ooze was found to contain fresh examples of each of the Invertebrate Subkingdoms. When examined at daylight on the morning of the 23rd, none of these were actually living, but their soft parts were perfectly fresh, and there was ample evidence of their having been living when they entered the dredge. The most remarkable species were:—

MOLLUSCA.—*Dentalium*, sp. n., of large size.
Pecten fenestratus, a Mediterranean species.
Dacrydium vitreum, Arctic, Norwegian, and Mediterranean.
Scrobicularia nitida, Norwegian, British, and Mediterranean.
Neæra obesa, Arctic and Norwegian.

CRUSTACEA.—*Anonyx Hölbollii*, Kröyer (=*A. denticulatus*, Bate), with the secondary appendage of the upper antennæ longer and more slender than in shallow-water specimens.
Ampelisca æquicornis, Bruzelius.
Munna, sp. n.

One or two ANNELIDES and GEPHYREA, which have not yet been determined.

ECHINODERMATA.—*Ophiocten Kröyeri*, Lütken; several well-grown specimens.
Echinocucumis typica, Sars. This seems to be a very widely distributed species; we got it in almost all our deep dredgings, both in the Warm and in the Cold areas.

A remarkable stalked Crinoid, allied to *Rhizocrinus*, but presenting some very marked differences.

POLYZOA.—*Salicornaria*, sp. n.

CŒLENTERATA.—Two fragments of a Hydroid Zoophyte.

PROTOZOA.—Numerous Foraminifera belonging to the groups already indicated (§ 33) as specially characteristic of these abyssal waters; together with *a branching flexible Rhizopod*, having a chitinous cortex studded with Globigerinæ, which encloses a sarcodic medulla of olive-green hue. This singular organism, of which fragments had been detected in other dredgings, here presented itself in great abundance.

One or two small SPONGES, which seem to be referable to a new group.

51. On Friday, July 23, we tried another haul at the same depth; but when the dredge came up at 1.30 P.M. it was found that the rope had fouled and lapped right round the dredge-bag, and that there was nothing

in the dredge. The dredge was sent down again at 3 P.M., and was brought up at 11 P.M., with upwards of 200 cwt. of ooze.—We got from this haul a new species of *Pleurotoma* and one of *Dentalium*, *Scrobicularia nitida*, *Dacrydium vitreum*, *Ophiocantha spinulosa*, and *Ophiocten Kröyeri*, with a few Crustaceans and many Foraminifera.

52. In both of these last deep dredgings the dredge brought up a large number of extremely beautiful *Polycystina*, and some forms apparently intermediate between *Polycystina* and Sponges, which will be described shortly. These organisms did not seem to be brought from the bottom, but appeared to be sifted into the dredge on its way up. They were as numerous adhering to the outside of the dredging-bag, as within it. During the soundings taken near this locality quite a shower of several beautiful species of the *Polycystina* and *Acanthometrina* fell upon the chart-room sky-light from the whole length of the sounding-line while it was being hauled in.

53. Dredging in such deep water was very trying. Each operation occupied seven or eight hours; and during the whole of that time it demanded and received the most anxious attention on the part of the Commander, who stood with his hand on the pulse of the Accumulator, ready at any moment, by a turn of the paddles, to ease any undue strain. The men, stimulated and encouraged by the cordial interest taken by their officers in our operations, worked willingly and well; but the labour of taking upwards of three miles of rope, coming up with a heavy strain from the surging-drum of the engine, and coiling it upon the "Aunt Sallies," was very severe. The rope itself looked frayed and strained, as if it could not be trusted to stand this extraordinary ordeal much longer. The question of the distribution of Life and the condition of the bottom had been solved; and the animals brought up, though of surprising interest, were few in number. On the morning of Saturday the 24th we therefore determined to cease dredging for the present, and to devote the day to an investigation which we regarded as at least equal in importance,—the determination of a series of Temperatures at intervals of 250 fathoms from the bottom to the surface. The following is a Table of the mean results of this series of observations (Station 38). The instruments used were the two Miller-Casella thermometers which were employed in all the temperature-soundings throughout the summer. The depth was 2090 fathoms.

Surface-temperature 64° F.=17°·08 C.

260 fathoms	...... 50·5	„ 10·28,	less than surface ..	13°·5 F.=	7°·5 C.
500 „	...... 47·8	„ 8·8,	„ 250 fath.	2·7 „	1·5
750 „	...... 41·3	„ 5·17,	„ 500 „	6·5 „	3·6
1000 „	...... 38·3	„ 3·5,	„ 750 „	3·0 „	1·7
1250 „	...... 37·7	„ 3·17,	„ 1000 „	0·6 „	0·3
1500 „	...... 37·2	„ 2·9,	„ 1250 „	0·5 „	0·3
1750 „	...... 36·7	„ 2·61,	„ 1500 „	0·5 „	0·3
2090 „	...... 36·3	„ 2·4,	„ 1750 „	0·4 „	0·2

54. The general result of this series of Soundings is of the highest interest; and although it may be premature to attempt an explanation of the details of the phenomena until all the temperature-observations which have been made in the North Atlantic have been reduced to the Miller-Casella standard and carefully correlated, still certain general conclusions seem self-evident.—The high surface-temperature, reduced by $13\frac{1}{2}$ degrees at 250 fathoms, is undoubtedly due to superheating by the direct heat of the sun. This is shown more clearly by the Table (§ 58), where nearly 7° are seen to be lost between the surface and 30 fathoms, and 4° more between 30 fathoms and 100 fathoms.—From 100 to 500 fathoms the temperature is still high and tolerably uniform, and it falls rapidly between 500 and 1000 fathoms; a reference to the second Table shows that the rapid fall is between 650 and 850 fathoms, during which interval there is a loss of nearly 6°. The second stage of elevated temperature, from 250 to 650 fathoms, seems to be caused by the north-easterly reflux of the great equatorial current. From 1000 fathoms the loss of heat goes on uniformly at the rate of 0°·5 for every 250 fathoms. The most singular feature in this decrease of temperature for the last mile and three quarters is its absolute uniformity, which appears to be inconsistent with the idea of a current, unless it were one of excessive slowness. It appears that the presence of this vast underlying body of comparatively cold water can only be accounted for on the supposition of a general interchange of warm and cold water, according to the doctrine laid down by Dr. Carpenter in the 'Lightning' Report, which will be more fully expounded hereafter (§ 115).

55. We were now steaming slowly back towards the coast of Ireland; and on Monday, July 26, we dredged in depths varying from 557 to 584 fathoms (Stations 39–41) in ooze, with a mixture of sand and dead shells. In these dredgings we got one or two very interesting Alcyonarian zoophytes, and several Ophiuridans, including *Ophiothrix fragilis*, *Amphiura Ballii*, and *Ophiacantha spinulosa*. Many of the animals were most brilliantly phosphorescent; and we were afterwards even more struck by this phenomenon in our Northern Cruise. In some places nearly everything brought up seemed to emit light, and the mud itself was perfectly full of luminous specks. The Alcyonarians, the Brittle-stars, and some Annelids were the most brilliant. The *Pennatulæ*, the *Virgulariæ*, and the *Gorgoniæ* shone with a lambent white light, so bright that it showed quite distinctly the hour on a watch. The light from *Pavonaria quadrangularis* was pale lilac, like the flame of cyanogen; while that from *Ophiacantha spinulosa* was of a brilliant green, corruscating from the centre of the disk, now along one arm, now along another, and sometimes vividly illuminating the whole outline of the starfish.

56. The question of the amount and the kind of Light in these abysses was constantly before us. That there *is* light, there can be no doubt. The eyes in many species of all classes were well developed; in some, very

remarkably so. A *Munida*, probably a variety of *Munida Banffi*, somewhat paler in colour than usual, and somewhat slighter in its proportions, which we met with abundantly in our northern dredgings, had remarkably large eyes, very brilliant, transparent, and bronzy, giving the impression of extreme sensitiveness. It is scarcely possible that any appreciable quantity of the Sun's light can penetrate beyond two hundred fathoms at most. The data with regard to the transmission of light through sea-water are very scanty; but the rapidity with which light diminishes during the first few fathoms seems to point to its speedy extinction. It seemed to us probable that the abyssal regions might depend for their light solely upon the Phosphorescence of their inhabitants. The only use which the lower animals make of light is to enable them to procure their food; and it is evident that in the night, or under any circumstances in which there is no source of general illumination, it would answer the same purpose of guiding them to their prey, if that prey itself were luminous. Among the Star-fishes the young specimens, 10 to 15 millims. from point to point of the rays, appeared to be much more luminous than mature examples of the same species. This is probably part of the great general plan which provides an enormous excess of the young of many species apparently as a supply of food; their wholesale destruction being necessary for the due restriction of the multiplication of the species, while the breeding individuals, on the other hand, are provided with special appliances for escape or defence. It is well known that fishes feed principally at night; and the path of a shoal of herrings may often be traced for miles by the broad band of phosphorescence caused by the glowing and scintillating of the myriads of phosphorescent animals, especially larvæ, with which the sea is crowded, and which supply their food. We can scarcely doubt that the phosphorescence of the inhabitants of the dark abysses of the sea performs, in regard to the great object of the supply of food, the functions performed in the upper world by the light of day.

57. On the 27th we dredged in 862 fathoms (Station 42), the weather being still very fine, and the sea quite smooth. The bottom was ooze with sand and dead shells. Among the Mollusca procured were a new species of *Pleuronectia*, *Leda abyssicola* (Arctic), *Leda Messinensis* (a Sicilian Tertiary fossil), *Dentalium gigas* (sp. n.), *Siphonodentalium* (sp. n.), *Cerithium metula*, *Amaura* (sp. n.), *Columbella Haliæeti*, *Cylichna pyramidata* (Norwegian and Mediterranean), and many dead shells of *Cavolina trispinosa*. These latter were very common in all the northern dredgings, though we never saw a living specimen on the surface.

58. During the afternoon we took a series of intermediate temperatures, at intervals of 50 fathoms, from the bottom at 862 fathoms to the surface. The following Table gives the general results of this series of observations:—

Surface (mean temp. of 100, Miller-Casella 103)	62°·8 F.	=17°·22 C.					
10 fathoms	62·1	,, 16·72,	less than surface		0°·7 F.	=	0°·5 C.
20 ,, ...	59·4	,, 15·22,	,,	10 fath.	2·7	,,	1·5
30 ,, ...	56·0	,, 13·33,	,,	20 ,,	3·4	,,	1·9
40 ,, ...	54·4	,, 12·44,	,,	30 ,,	1·6	,,	0·9
50 ,, ...	53·2	,, 11·8,	,,	40 ,,	1·2	,,	0·64
100 ,, ...	51·1	,, 10·6,	,,	50 ,,	2·1	,,	1·2
150 ,, ...	50·9	,, 10·5,	,,	100 ,,	0·2	,,	0·1
200 ,, ...	50·5	,, 10·3,	,,	150 ,,	0·4	,,	0·2
250 ,, ...	50·2	,, 10·11,	,,	200 ,,	0·3	,,	0·2
300 ,, ...	49·6	,, 9·8,	,,	250 ,,	0·6	,,	0·3
350 ,, ...	49·1	,, 9·5,	,,	300 ,,	0·5	,,	0·3
400 ,, ...	48·5	,, 9·17,	,,	350 ,,	0·6	,,	0·3
450 ,, ...	47·6	,, 8·7,	,,	400 ,,	0·9	,,	0·5
500 ,, ...	47·4	,, 8·55,	,,	450 ,,	0·2	,,	0·15
550 ,, ...	46·4	,, 8·0,	,,	500 ,,	1·0	,,	0·55
600 ,, ...	45·5	,, 7·4,	,,	550 ,,	0·9	,,	0·5
650 ,, ...	44·3	,, 6·83,	,,	600 ,,	1·2	,,	0·6
700 ,, ...	43·6	,, 6·44,	,,	650 ,,	0·7	,,	0·4
750 ,, ...	42·5	,, 5·83,	,,	700 ,,	1·1	,,	0·6
800 ,, ...	42·0	,, 5·55,	,,	750 ,,	0·5	,,	0·3
862 ,, ...	39·7	,, 4·3,	,,	800 ,,	2·3	,,	1·25

A Water-Bottle was sent down with the sounding-lead on each occasion; and the specific gravity of the water was carefully taken, the air contained in it analyzed, and the amount of organic matter estimated by Mr. Hunter. Mr. Hunter's results, which are given below, corroborate generally the observations made on the previous cruise by Mr. William L. Carpenter. The Specific Gravity of the water is somewhat higher at the surface than at a depth of 50 fathoms; this is probably due to evaporation, the observation having been made after a course of hot weather. From 50 fathoms downwards it increases slightly but steadily till within 50 fathoms of the bottom, when it again falls a little. The proportion of Carbonic acid in the contained gases increases slowly and steadily till within 50 fathoms of the bottom, when it increases suddenly to the extent of upwards of 15 per cent., mainly at the expense of the Nitrogen, which falls upwards of 14 per cent., the Oxygen remaining nearly stationary. The amount of Organic matter seems to be very uniform, varying, apparently irregularly, within narrow limits.

Depth in Fathoms.	Specific Gravity.	Percentage of Carbonic Acid.	Percentage of Oxygen.	Percentage of Nitrogen.	Total Percentage of Gases.	Oxygen required to neutralise Organic matter in 250 c. c. of water.
						gramme.
862	1027·5	48·23	17·22	34·50	3·5	·001
800	1027·7	33·75	17·79	48·46	2·8	·001
750	1027·5	31·92	18·76	49·32	2·8	·0012
700	1027·5	31·02	19·31	49·66	2·2	·0013
650	1027·5	30·00	19·80	50·20	2·4	
600	1027·5	28·34	20·14	51·52	2·4	·0005
550	1027·5	29·06	20·70	50·24	2·6	·0009
500	1027·5	27·26	....	....	2·2	·0014
450	1027·5	24·73	22·18	53·09	2·8	·0005
400	1027·5	29·73	22·71	47·51	2·5	·0014
350	1027·3	....	....	....	....	·0015
300	1027·3	....	....	....	....	·0018
250	1027·3	....	....	....	....	·0019
200	1027·3	....	....	....	....	·0017
50	1027·2	30·73	25·23	43·84	2·2	·0014
Surface	1027·5					

59. On the 28th we dredged in 1207 fathoms (Station 43), with a bottom of ooze. A large *Fusus* of a new species (*F. attenuatus*, Jeffreys) was brought up alive, with two or three *Gephyrea*, and an example each of *Ophiocten Kröyeri* and *Echinocucumis typica*. We again dredged on the 29th and 30th, gradually drawing in towards the coast of Ireland in 865, 458, 180, and 113 fathoms successively (Stations 44, 45). In 458 fathoms (Station 45) we procured a broken example of *Brisinga endecacnemos*, previously taken by Mr. Jeffreys off Valentia, and a number of interesting Mollusca; and in 458 and 180 fathoms (Stations 45 and 45*a*) an extraordinary abundance of animal life, including many very interesting forms—*Dentalium abyssorum*, *Aporrhaïs Serresianus*, *Solarium fallaciosum*, *Fusus fenestratus*, a beautiful Ophiurid, the type of a new genus allied to *Ophiura*, remarkably large specimens of the commoner forms, *Ophiothrix fragilis* (for example), nearly a foot and a half from tip to tip of the arms, and brilliantly coloured, abundance of *Caryophyllia Smithii*, and of all the ordinary deep-water forms of the region. About midday on Saturday, the 31st of July, we steamed into Queenstown. Having coaled at Haulbowline on Monday the 2nd of August, we were moored in the Abercorn basin, Belfast, after a pleasant return passage up the channel, on the evening of Wednesday the 4th.

Third Cruise. (Chart, Plate 6.)

60. In accordance with the original Programme (§ 29), the Third Cruise was devoted to the re-examination, on a more minute and extended scale, of

the region of the "Warm and Cold Areas" traversed last year in the 'Lightning,' with the special view of determining, if possible, (1) the Physical conditions on which depends the remarkable contrast then discovered between their *bottom*-temperatures, the Sea-bed being of nearly the same depth throughout, and their *surface*-temperatures being alike; and (2) the influence of this difference upon the distribution of Animal life, and on the nature of the Sea-bed itself.

61. As it was requisite that the boilers of the 'Porcupine' should be thoroughly cleansed after her return from the Second Cruise, we did not leave Belfast (to which port she had gone round from Cork) until Wednesday, August 11th; when we made direct for Stornoway as our final point of departure, arriving there on Friday the 13th. Having taken in as much coal as could be safely stowed on deck, as well as below, we left Stornoway on the afternoon of Sunday the 15th, and proceeded in the direction of the spot on which we had made our most successful dredging of last year ('Lightning' Report, § 16), and which we had come to call the *Holtenia*-ground*. Our dredging on this ground (Station 47) again proved remarkably successful, bringing up numerous specimens of *Holtenia*, of *Hyalonema* (one of them constituting a new species), of *Adrasta infundibulum* (a type allied to *Hyalonema*), and of *Tisiphonia* (a remarkable genus of Siliceous Sponges, obtained, like *Holtenia*, for the first time last year, of which a description, by Prof. Wyville Thomson, will soon be presented to the Royal Society), besides many other specimens of great interest, some of which appear to be new types. The experience of the 650 fathoms' dredge last year having led us to put aside and preserve the *siftings*, instead of attempting to pick them over at the time, we have since found them to yield an extraordinarily rich harvest of *Foraminifera*, including not merely the types mentioned in the 'Lightning' Report (§ 16), but a great number of others, especially of the *Arenaceous* Order, in which the shelly covering is replaced by a "test" composed of sand-grains more or less firmly cemented together. Although the *Holtenia*-ground lies within the Warm area, the Sea-bed of which is ordinarily covered by *Globigerina*-mud ('Lightning' Report, p. 190), yet this mud here contains a considerable admixture of sand, obviously derived from the Cold area with which it is here in immediate proximity. For this sand, when separated from the *Globigerina*-mud, corresponds precisely in its character with that of the Cold area, being especially distinguished by the admixture of particles of Augite and other minerals having an undoubtedly Volcanic source. This admixture is very perceptible to the experienced eye in the "tests" of *Astrorhiza* and other Arenaceous Foraminifera abundant

* The largest of the extraordinary Vitreous Sponges dredged in this locality last year has been described by Prof Wyville Thomson, in a Memoir presented to the Royal Society on June 17, and since published in the Philosophical Transactions for 1869, under the generic name *Holtenia*, in compliment to our excellent friend Amptman Holten, the Governor of the Faroe Islands.

in this locality, as well as in those of *Lituola* and other Arenaceous types inhabiting the Cold area, where the bottom is formed by sand and small stones alone.

62. It is not a little curious that one of the new types* discovered last year in the 650 fathoms' dredging ('Lightning' Report, § 19), which was made in a part of the Warm area far removed from the borders of the Cold, was now found to occur here also, but with a remarkable difference in the structure of its "test." Its shape is fusiform, generally somewhat curved, not unlike a §; and it has only one undivided cavity, with a tubiform aperture at each end. Now in the true Cretaceous area, where sand-grains are scarce, but sponge-spicules abound, this Rhizopod constructs its "test" almost entirely of sponge-spicules, laid with most extraordinary regularity, a sand-grain being interposed here and there to fill up a vacuity left by the oblique crossing of the spicules. But in the *Holtenia*-ground, where sand is abundant, "tests" of precisely the same general form and proportions are built up almost entirely of sand-grains cemented together; sponge-spicules, however, being invariably used to form the tubiform mouths, and the mouth thus formed being sometimes prolonged like a proboscis.—It is difficult to conceive how creatures which seem nothing more than particles of animated jelly, without "organs" of any kind, can exert so remarkable a power of selection and construction as is shown in the "tests" of some of these Arenaceous *Foraminifera*. There are none which are more symmetrically constructed than the triradiate *Rhabdammina*; each of its three very slender arms, which diverge at equal angles, being a cylindrical tube, built up of sand-grains of very uniform size, united by a firm cement which contains a considerable proportion of Phosphate of Iron. This tube is beautifully smoothed off internally; and it is no rougher externally, in proportion to its size, than any wall would be that is built of rough-hewn stones arranged by the hands of a most dexterous mason. The only structure with which we are acquainted that is at all comparable to it in workmanship is the sandy tube of the *Pectinaria*, one of the Tubicolar *Annelids*, a creature comparatively high in the scale of organization.

63. It was here that we employed for the first time an addition to our Dredging apparatus devised by Capt. Calver, who, having noticed that animals frequently came up attached to the part of the dredge-rope that had lain on the ground, or to the net of the dredge itself, justly reasoned that if the Sea-bottom were *swept* with hempen brushes, they would probably bring up many creatures that might escape the *scraping* of the dredge. These brushes were made of bundles of rope-yarn teazed out into their separate threads, and tied together at the top, so as closely to resemble the

* This type was described by Dr. Carpenter in a Memoir presented to the Royal Society, June 17th, "On the Rhizopodal Fauna of the Deep Sea," as a form of the *Proteonina* of Prof. Williamson. He has subsequently been led to doubt, however, whether that designation can be properly applied to it.

ordinary "swabs" used on board ship. An iron rod was attached to the bottom of the dredge, and carried out about two feet on either side of it; and it was to these projecting portions (resembling the studding-sail-booms extended from a yard-arm) that the "hempen tangles" were attached by Capt. Calver, who rightly judged that if they were attached to the bottom of the dredge itself, they would only bring up what the dredge had passed over and crushed. Though the use of these "tangles" added much to our "hauls" on the *Holtenia*-ground, especially on a subsequent occasion (§ 86), yet it was on the hard bottom of the Cold area that their value became especially apparent, the "tangles" often coming up laden with the richest spoils of the Ocean-bed, when the dredge was nearly empty (§ 74).

64. Our course was now directed slowly N.N.W., towards the southern edge of the Faroe Bank, Soundings being frequently taken, that we might determine the boundary in this region between the Warm and the Cold areas. The *minimum* temperature on the *Holtenia*-ground, as shown by the "protected" Thermometers, was a little under 44°, the depth being 540 fathoms; and this accorded very closely with the temperature of 47°·3 observed in the same spot last year, when the requisite correction was applied for pressure. A Sounding taken on the afternoon of the next day, at Station 49 (Lat. 59° 43′, Long. 7° 40′), showed a somewhat less depth, viz. 475 fathoms, and a slightly higher *minimum* temperature, 45°·4. In the evening of the same day another Sounding was taken (Station 50), and it was found that the depth had diminished to 355 fathoms, whilst the *minimum* temperature had risen to 46°·2. A Sounding taken early the next morning, however, at Station 51 (Lat. 60° 6′, Long. 8° 14′), showed a *minimum* of 40°, with a depth of 440 fathoms; and this depression of temperature led us to surmise that we were here passing from the Warm into the Cold area. The correctness of this surmise was soon proved; for a Sounding taken at about 20 miles to the north, at Station 52 (Lat. 60° 25′, Long. 8° 10′), gave a *minimum* temperature of 30°·6, though the depth had diminished to 384 fathoms!

65. In order to ascertain more particularly the conditions of this very remarkable depression, we requested Capt. Calver to ascertain the temperature at depths progressively increasing by 50 fathoms; and it was thus shown (1) that the *minimum* temperature *is* that of the bottom, as had been argued in the 'Lightning' Report (p. 189) to be probably the case; (2) that this *minimum* is nearly reached at a depth of 300 fathoms; (3) that the decrease of temperature is by no means uniform, but that whilst it takes place in the first 200 fathoms at nearly the same rate as in the most northerly stations previously tested in the First Cruise, there is a rapid and extraordinary diminution, amounting to more than 15°, between 200 and 300 fathoms. (See Table I. p. 456.) This diminution can scarcely be accounted for on any other hypothesis than that of a stream of frigid water passing under the warmer and more superficial stratum.—It is worthy of note that in this spot we found evidence, in the rounded form of the stones

and gravel brought up by the dredge, of a more decided *movement* of water than is presented in the Cold area generally, the bottom of which generally consists of fine sand, sometimes with an admixture of clay, including stones but little rolled. And as our subsequent Soundings have led us to believe that we were here on the western border of the Cold area, and that its stream of frigid water is reduced at the same time in breadth and depth, before discharging itself into the deep Oceanic basin (§ 104), a more rapid movement is precisely what might be expected.

66. Altering our course now to the E.S.E., we took another Sounding on the evening of the same day (Station 53), after a run of about 25 miles, and found the depth increased to 490 fathoms, and the *minimum* (which we shall now call the *bottom*) temperature reduced to 30°. This course having been continued during the night, we found ourselves (Station 54), early on the morning of August 19th, in Lat. 59° 56′ and Long. 6° 27′, where the depth was 363 fathoms, and the bottom-temperature 31°·4. It was thus obvious that we were still in the Cold area, although we had come back almost exactly to the latitude of Station 50, and were more than twelve miles to the south of the lowest parallel to which we had traced it last year. (We subsequently traced it, at Station 86, about nine miles still further south.) The coincidence of Depths as well as of Latitudes between Stations 50 and 54, with deeper water both north and south of them, shows that the bed of the channel here rises into a ridge, which has probably something to do with the direction of the course of the flow along its bottom.—We then again turned northwards, and in the afternoon of the same day found that our depth (Station 55) had increased to 605 fathoms, whilst the bottom-temperature was somewhat below 30°. Our Soundings were frequently repeated in this part of the Area, with great uniformity in their results, both as to Depth and Temperature; and our Dredging operations were carried on with little intermission. As the wind and swell were very moderate (although we were here almost constantly in a cold damp mist, which sometimes gave place to a mizzling rain), it was found convenient to put the dredge over soon after midnight, and to let it drag until about 4 A.M., hauling it in at the beginning of the morning watch. In this manner a rich harvest was frequently obtained. The general results of our Zoological exploration of the Cold Area may be best stated hereafter (§§ 74–80) in a collective form.

67. As we wished to examine the shallow bank of 170 fathoms in the middle of the Cold area, upon which we dredged last year ('Lightning' Report, § 13), our course was now directed to the spot on which it had been laid down in the Chart of the 'Lightning' Expedition; but we did not succeed in falling-in with it. The explanation of our failure seems to lie (1) in the extremely limited area of this bank, as shown by the great depth of water found in the 'Lightning' soundings on either side of it; (2) in the circumstance that both last year and this year, while we were working over this ground, the sky was so overcast for several days together, that it was impos-

sible to fix the place either of the 'Lightning' or of the 'Porcupine' by observation; and (3) that a "dead reckoning" cannot be kept with any considerable exactness when the ship is drifting with a dredge attached to it during a great part of the twenty-four hours.—Hence either the place of the bank may not have been precisely laid down in the 'Lightning' Chart; or a corresponding error of a few miles may have been made in estimating the place of the 'Porcupine.' How exactly accordant were the points determined by observation in the two Expeditions is shown by the precision with which Captain Calver twice placed us on the *Holtenia*-ground (§§ 61, 86), though approaching it in each case in a direction different from that in which we came upon it last year.

68. Pursuing our exploration about thirty miles further eastwards in the same parallel, we sounded on the afternoon of the 20th in 580 fathoms (Station 59, Lat. 60° 21′, Long. 5° 41′), and found the bottom-temperature 29°·7, which was nearly the lowest anywhere met with. From this point, which was on the line of Soundings between the Orkney and Faroe Islands previously taken in the 'Bull-dog,' we again turned our course northwards for Thorshavn, as it was our intention to make this our point of departure for the exploration of that north-eastern portion of the channel which lies between the Faroe and the Shetland Islands. The weather having now cleared, we had on the morning of Saturday the 21st a most beautiful run along the series of remarkably formed islands which we had last year only seen dimly through their covering of mist; and on anchoring at Thorshavn in the afternoon, we received a cordial greeting from our excellent friend Governor Holten, who, having been forewarned of our probable visit, and having had our vessel in view for some hours, at once came off in his barge to welcome us.

69. The apparently settled state of the weather encouraged us to hope that we might be able to avail ourselves of this opportunity of visiting Myling Head, the remarkable precipice which forms the North-western point of Strömoe, the principal island of the Faroe group, and which falls 2100* feet perpendicularly, its summit even slightly overhanging its base, so that a stone let fall from it drops into the sea beneath. On inquiring from the Governor as to the best means of carrying our wish into effect, he informed us that the tide runs so strongly round the islands, that if we started with the morning flood, and our vessel kept its speed in accordance with the rate of the tidal wave, we should be able to make the whole circuit in six hours; but that if we should attempt the expedition in any other mode, we should be tediously delayed by the strength of the opposing tide. As we learned that high water would occur on the following Monday morning at 4 o'clock†, we made

* The height of Myling Head is commonly stated at 2500 feet; but the above estimate is based on an observation made a few years since with an Aneroid barometer by the Authors of "The Cruise of the Yacht 'Maria' among the Faroe Islands."

† It is worthy of mention that a discrepancy between the Ship's time and the Island time (as indicated by the Church clock) having led us to inquire into the mode in which

our arrangements for an early start; and invited our kind host and hostess to give us the pleasure of their company. The fine weather lasted throughout Sunday, two consecutive days of such brightness being a most unusual occurrence in this locality; but early the next morning the Faroese climate vindicated its character by a copious downpour of rain, which put our start at 4 o'clock out of the question, and, for the reason just mentioned, obliged us to give up the excursion altogether.

70. Our good fortune in regard to weather returned to us on the following day; when we left Thorshavn (Aug. 24th) about noon, shaping our course about East by South, so as to cross the channel separating the Faroe from the Shetland Islands, the depth of which had been indicated by previous Soundings to be in some parts considerable. Our first two Soundings showed that we were still over a plateau at little more than 100 fathoms from the surface; but a third Sounding taken in the evening after a run of about 80 miles, gave us a depth of 317 fathoms, and a bottom-temperature of 30°·1. It became evident, therefore, that we were here again in the course of the frigid stream; and we looked with much interest to the phenomena it would present in a still deeper part of the channel. Having kept the same course under easy steam during the night, we took a Sounding the next morning at Station 64 (Lat. 61° 21′, Long. 3° 44′); and found that the depth had increased to 640 fathoms, and that the bottom-temperature was somewhat below 30°. The dredge having been put down, the "haul" was a less satisfactory one than usual, though one very valuable specimen (a large example of the *Pourtalesia* already mentioned, § 36) was obtained here; and in a subsequent trial the dredge came up empty. As this result appeared due to the circumstance that the drift of the ship was too great, in consequence of an increase of wind and swell, to permit the dredge to hold the ground, it was determined to devote the morning to a series of Temperature-soundings taken at every 50 fathoms from the surface downwards. This was very satisfactorily accomplished, with the result shown in Table I. (p. 456), from which it appeared that, with a lower surface-temperature than in the series previously taken (§ 65), the rate of decrease during the first 150 fathoms was nearly the same, but that the rapid descent of the thermometer which showed itself at Station 52 between 200 and 300 fathoms, here began somewhat earlier, and proceeded somewhat more gradually, with the result, however, of bringing down the temperature to 32° at a little below 300 fathoms, the whole of the water beneath that

---

the latter was regulated, we found that as there is not even a Sun-dial in the Islands, *the time is kept by the turn of the tides*, the periods of which are precisely known for each day of the lunation. As nearly all the intercouse between different villages and farm-houses is carried on by water, and as every Faroese is a boatman and fisherman as well as a farmer, it is not to be wondered at that he should be practically versed in the periodical changes of the currents by which his power of locomotion is so greatly influenced, and that these should take the place of the meridian passage of the sun (which he has no means of observing with precision) as his best time-regulators.

depth, down to the bottom of 640 fathoms, on which the temperature is 30°, being of icy coldness.—Thus the entire mass of water in this channel is nearly equally divided into an upper and lower stratum,—the *lower* being *an Arctic stream* (so to speak) of nearly 2000 feet deep, flowing in a S.W. direction, beneath an *upper* stratum of comparatively warm water moving slowly towards the N.E.; the lower half of the latter, however, having its temperature considerably modified by intermixture with the stratum over which it lies.

71. Keeping still on the same course through the following night, we took a Sounding early the next morning (Station 65), which showed that we had crossed the deepest part of the channel, the depth having here diminished to 345 fathoms; the bottom-temperature, however, was still most characteristic of the Cold area, being almost exactly 30°, the lowest we had met with at that comparatively moderate depth. This circumstance, taken in connexion with the earlier descent just noticed, corresponded well with the fact that the line between Lat. 61° and Lat. 62° on which we had now crossed this channel, is nearer the source of the frigid stream than the lines between lat. 60° and 60½° in which we had at first traversed it.

72. On the afternoon of the same day (Aug. 26), we again took a Sounding, which gave us the still further diminished depth of 267 fathoms; and here (Station 66), with a *surface*-temperature of 52½°, which was but slightly above that of the previous Sounding, we found the *bottom*-temperature to be 45°·7. Now this was very nearly 12° above the temperature taken *at the same depth* at Station 64; whilst it was nearly 16° above the temperature last taken on a bottom only 78 fathoms deeper, at a Station distant only 18 miles. Even this slight difference of depth, however, seems fully adequate to explain the remarkable contrast between the bottom-temperatures of these two Stations; for, as already shown, the Arctic stream, in virtue of its greater Specific Gravity, occupies only that portion of the channel of which the bottom lies below about 320 fathoms' depth, so that no part of it will flow over that portion of the bank of the channel which has a depth of only 267 fathoms. The bottom on this bank, therefore, will be overlaid by the upper (warm) stratum alone; and as the lower half of this is not here subjected to the reduction of temperature which it sustains when underlaid by the frigid stream, the bottom will have the temperature characteristic of the Warm area, though not geographically included in it.

73. By the next morning we had come upon the shallow plateau on which the Shetland islands are based; and as we wished to examine some points in the Geographical distribution of the Fauna inhabiting this locality, we ran past the northern point of the group, and devoted the day to dredging at about thirty miles to the east, on what is known as the Haaf, or deep-sea fishing-ground. Our dredging on this plateau was not very productive as regards variety; but it brought up certain types in such extraordinary number as to show how abundantly they must be diffused over the Sea-bed,

The most remarkable instance of this occurred in regard to the *Echinus Norvegicus*, a small sea-egg about the size of the top of the finger. The "hempen tangles" came up so laden with these, that a very moderate estimate would place the number obtained in one "haul" at 20,000, whilst some of our party deemed it to be nearer 50,000. This had formerly been accounted a rare species, of which it was considered a piece of good fortune to find one or two at a time, and was first met with in abundance in Mr. Jeffreys's Shetland Dredgings.—On the following day (Aug. 28th) we anchored in Lerwick harbour, where it was requisite for us to replenish our coal, as well as to obtain a further supply of jars and spirit, the abundance of our collections having nearly exhausted what we had supposed to be our ample provision of both.

74. Without entering into details which will be more appropriately given hereafter, we may say that our exploration of this Cold area, which we had been led by the results of our last year's dredging to regard as comparatively poor in Animal life (as, indeed, we should still have believed it to be, had our knowledge of its Fauna been restricted to the contents of the Dredge, instead of being chiefly obtained by the instrumentality of our "hempen tangles"), greatly extended our ideas of the conditions of animal existence; for we found the Sea-bottom, at depths of from 350 to 640 fathoms, at a temperature at or below the freezing-point of fresh water, almost, if not quite, as thickly covered with Animals as in the richest parts of the Warm area. These animals were mostly, however, of a very different character. In the first place, the *Globigerina*-mud was entirely wanting, its presence being sharply bounded by the limit of the Warm area, and its composition being modified even on the borders of this by an admixture of the Sand characteristic of the Cold area (§ 61). Now this fact appears to be a conclusive disproof of the hypothesis that the accumulation of the shells of *Globigerinæ* on the bottom of the ocean is due to their having fallen to the bottom after death, their lives having been passed at or near the surface. For admitting that they have been occasionally captured by the tow-net*, this only proves that they *can* float; whilst, on the other hand, our examination of specimens freshly dredged from great depths enables us to state with positiveness that their sarcodic bodies present all the attributes of life which are exhibited by those of the *Rotaline* forms whose attachment to solid bodies made it clear that *they* must pass their lives at the bottom, and of the *Arenaceous* types which can only there obtain the materials for their "tests." Now since, as we have repeatedly pointed out, the *surface*-temperature of the Cold area does not differ from that of the Warm, and this equality extends to the first 150 or 200 fathoms, there seems no reason whatever why a deposit of *Globigerina*-mud should not take place on the bottom of the Cold area, if such deposit be due to the accumulation of the dead shells of individuals which had spent their

* See Major Owen's account of the Surface-Fauna of the Atlantic; in Journal of the Linnean Society, vol. ix. p. 147.

lives at or near the surface. Whereas if they really inhabit during their lives the bottom on which they are found in such extraordinary abundance, we have at once the explanation, in the difference of temperature between the two Areas, of their definite restriction to the Warm *.

75. The simple Protozoic type represented by the *Globigerinæ*, however, has its parallel in the Cold area, though presenting itself under a very different aspect. Every Zoologist now recognizes the close Physiological relationship between *Foraminifera* and *Sponges*, notwithstanding their wide morphological divarication; and we believe them to agree in this most important particular,—that the animals of both groups are capable of obtaining their nutriment by the imbibition of the Organic matter diffused through sea-water (§ 23), just as they derive from the same source the Carbonate of Lime or the Silex which forms the Mineral basis of their skeletons. The *Sponges* of the Cold area were very diverse in type, and some of them extremely numerous individually. Magnificent specimens of most of the species hitherto known only as inhabitants of the deep water off Shetland were found to be very generally diffused; but the most peculiar and novel type of this group was met with at our very entrance upon the Cold area (Station 52), and presented itself in such abundance at almost every other Station having the same bottom-temperature, that we came to look upon it as one of the most characteristic inhabitants of this area, covering (as it seems to do) hundreds of square miles of the Sea-bed. This Sponge is distinguished by the possession of a firm branching axis, of a pale sea-green colour, rising from a spreading root, and extending itself like a shrub or a large branching *Gorgonia*. The axis is clothed with the soft pale-yellow sarcodic substance of the Sponge; and both axis and sponge-substance are crowded with Siliceous spicules, resembling those of *Esperia*, a well-known Mediterranean and Adriatic form, near which our Sponge must be placed, though it clearly forms the type of a new genus. It is curious that scarcely even fragments of this Sponge came up in the dredge, our specimens being almost entirely obtained through the instrumentality of the "hempen tangles" attached to it. We had last year obtained some minute fragments of the axial portions of the branches of this Sponge; but they were so imperfect that we had not been able to make out their true characters.

76. The most remarkable *Foraminifera* obtained in this area belonged to the *Arenaceous* Order; and it is singular that whilst very abundant in the localities in which they were met with, they seemed very restricted in Geographical range. Thus at Station 51, which was intermediate between the

* Mr. Jeffreys desires to record his dissent from this conclusion, since (from his own observations, as well as those of Major Owen and Lieut. Palmer) he believes *Globigerina* to be exclusively an *Oceanic* Foraminifer inhabiting only the superficial stratum of the sea; he considers also that the strength of the submarine current in the Cold area is sufficient to sweep away and remove these very slight and delicate organisms. According to him the protrusion of pseudopodia is the only satisfactory proof that the *Globigerina* is living.

Warm and the Cold area (§ 64), the "tangles" brought up an immense number of tubes usually from $\frac{3}{4}$ inch to 1 inch long, and about $\frac{1}{8}$ of an inch in diameter, composed of sand-grains cemented together. These tubes often presented an appearance of segmentation externally; and they were at first supposed to be a modification of the straight chambered *Lituolæ* obtained on the bank of the Cold area last year, though differing from them in having no definite prominent mouth. On breaking them open, however, it was found that the cavity is not divided into chambers by interposed septa, as in *Lituola*, but that it is continuous throughout, though traversed in every part of its length by irregular processes built up partly of sand-grains and partly of sponge-spicules, strongly resembling those which have been recently described by Mr. Brady in the gigantic Foraminiferal fossil *Loftusia*, and which present their most symmetrical arrangement in the yet more gigantic fossil *Parkeria* described in the same Memoir by Dr. Carpenter*. These arenaceous processes lie in the midst of the sarcodic body, which fills the whole of the cavity without any division into segments, and which communicates with the surrounding medium, at what appears to be the free extremity of the tube, by irregular spaces left between the agglutinated sand-grains that form a rounded termination which nearly closes it in. At the other extremity, however, the tube is so uniformly open in the numerous specimens that have been examined, and so generally presents an appearance of fracture, that there seems strong ground for believing that this type (to which we assign the generic designation *Botellina*) must grow attached by the lower end of its tube to some fixed base. It is singular that while this fabric presented itself at no neighbouring Station, the "tangles" brought up in the comparatively shallow water near Shetland a number of tubes, which, though of somewhat larger size, and having their sand-grains yet more regularly agglutinated, presented so close a general resemblance to our *Botellina*, as strongly to suggest a similarity of character. This idea, however, was soon dispelled by further examination; for the tubes, when broken open, proved to be as smooth internally as they were externally, and to be lined by a definite membrane; in addition to which they were freely open, and their edges rounded off, at what appeared to be their last-formed extremity; so that there remained no doubt that they had been constructed by some Tubicolar *Annelid*.—The true chambered *Lituolæ* found last year on the 170 fathoms bank, in the Cold area ('Lightning' Report, § 13), were not met with this year; but monothalamous "tests," closely resembling them in external appearance, were obtained in abundance at Station 64.—With the exception of these *Arenaceous* types, the *Foraminifera* met with in the Cold area were not remarkable either for number or variety; and, as compared with their extraordinary abundance in the Warm area (§ 87), were rather "conspicuous by their absence."

77. The most marked feature in the Fauna of the Cold area was undoubt-

* Philosophical Transactions, 1869, p. 806.

edly its extraordinary richness in *Echinoderms*, the prevalent types being of a decidedly Boreal and even an Arctic character. During the course of our exploration we met with nearly all the members of this group which have been described by Scandinavian naturalists as inhabiting the coast and fiords of Norway; and we were particularly struck with the abundance of the beautiful *Antedon* (*Comatula*) *Eschrichti*, which has hitherto been obtained only from the neighbourhood of Iceland and Greenland. On the other hand, such of the characteristically Southern forms as here presented themselves were so reduced in size that they might almost be accounted specifically distinct, if it were not for their exact conformity in general structure; the *Solaster papposa*, for example, being dwarfed from *six* inches in diameter to *two*, and having never more than ten rays, and the *Asteracanthion violaceus* and *Cribella oculata* being reduced in like proportion. One striking feature of the group, however, showed no modification. The *coloration* of these animals, though brought up from a depth of 500 or 600 fathoms, was as rich and beautiful as that of their littoral representatives. Their orange, violet, and scarlet blended admirably with the pale green of the large Sponge-stem when grouped together in a basin of water; and we were led to wonder, on the one hand, how such vivid hues could be produced in the absence of light, and, on the other, what purpose they can serve in the economy of animals which live on a bottom supposed to be entirely unillumined by solar rays, and which only exhibit these hues when brought within reach of daylight. Whilst our explorations in the Cold area have thus added to the British Fauna a large number of types of *Echinoderms* which had been previously supposed to lie altogether beyond its range, they have also brought up several forms which altogether are new to science, some of them of very considerable interest. Thus in the Shetland channel we procured a full-sized specimen of the remarkable Clypeastroid *Pourtalesia*, of which young examples had been obtained in the First cruise (§ 36), and a very singular Asterid allied to *Pteraster*, which is covered with a regular brush of long paxillæ. Since, for the reason formerly mentioned, we have found ourselves precluded from dedicating the former of these types (as we had intended) to our friend Capt. Calver, we propose to give the generic name *Calveria* to the latter, with the specific designation *hystrix*.

78. Of the *Crustacea* of the Cold area, many are most distinctly referable to the Fauna of Spitzbergen, whilst others are characteristically Norwegian. We were struck with finding attached to the "tangles," on nearly every occasion, numerous specimens of very large *Pycnogonids*, measuring, when their limbs were extended, as much as four or five inches across. The comparatively small forms of these animals that are common on our own shores are commonly found imbedded in the gelatinous layer that envelopes the surfaces of *Algæ*; and the suctorial character of their mouths, taken in connexion with the feebleness of their locomotive powers, seems to indicate that they are nourished by the ingestion of this material. Hence it is probable that their gigantic representatives living on the Sea-

bottom make the same use of the sarcodic substance of the *Sponges* and *Rhizopods* which they there meet with *.

79. The *Mollusca*, which in the preceding Cruises usually constituted the principal results of the dredgings, were here quite subordinate, as regards both number and variety, to the groups already alluded to; and the difference between the Molluscan Fauna of the Cold and that of the Warm area was not by any means as great as was shown in other groups. One of the most interesting types which we met with was a Brachiopod found living, at Station 65 in the Shetland channel, at a depth of 345 fathoms, and a bottom-temperature of 30°, viz. the *Terebratula septata* of Philippi, $= T.$ *septigera* of Lovén. A variety of this species, from the Pliocene beds of Messina, has been described and figured by Prof. Seguenza under the name of *Waldheimia Peloritana*; and it is clearly the same as the *Waldheimia Floridana*, found in the Gulf of Mexico by Pourtales, which our own numerous specimens so considerably exceed in size as to show that its most congenial home is in frigid water. A single specimen was found of another remarkable Brachiopod, the *Platydia anomioïdes* of Scacchi (or *Morrisia* of Davidson), hitherto supposed to be restricted to the Mediterranean. Since in this case, also, the size of our specimen greatly exceeds that of the Mediterranean examples of the same species, being nearly double, the presumption is strong that its original home is in the Boreal, perhaps even in the Arctic region.

80. Only a small number of *Fishes* were procured, but their scarcity may probably have been chiefly due to the unsuitableness of the dredges as a means of their capture. The few species taken have been examined since our return by Mr. Couch. The list includes a new generic form intermediate between *Chimæra* and *Macrourus*, which was brought up from a depth of 540 fathoms in the cold area; a new species of a genus allied to *Zeus*; a new *Gadus* approaching the common Whiting; a new species of *Ophidion*; the type of a new genus near *Cyclopterus*; *Blennius fasciatus* (Bloch), new to Britain; *Ammodytes siculus*; a fine new *Serranus*; a new *Syngnathus*; with several others, which will be described in full hereafter.

81. Having obtained the requisite supplies at Lerwick, we left the harbour about noon on the 31st of August, and ran southwards until we had passed Sumburgh Head, when we steered towards the west, our object now being to examine the southern portion of the channel between the North of Scotland and the Faroe islands with the same minute attention which we had previously bestowed on its northern portion. Early the next morning we sounded (Station 71) in Lat. 60° 17′, Long. 2° 53′, and found the depth to be 103 fathoms, and the bottom-temperature 48°·6, the tem-

* It seems worth while here calling to mind that a *Pycnogonid* of even yet more gigantic dimensions was among the specimens obtained by what was at that time considered very deep dredging in Sir James Ross's Antarctic Expedition. See 'Lightning' Report, p. 178, note.

perature of the surface being 52°·7. In the afternoon of the same day (Station 74) the water had deepened to 203 fathoms, while its bottom-temperature had diminished to 47°·6, the surface-temperature being 52°·6. Another Sounding (Station 75) taken only two and a half hours later, and at a distance of no more than 10 miles from the preceding, gave a depth of 250 fathoms, and a bottom-temperature of 41°·9—a reduction which clearly showed that the frigid current exerts no inconsiderable influence in this locality, the temperature at Station 66, at the slightly greater depth of 267 fathoms, having been 45°·7. Having run about 30 miles during the night, we found ourselves early the next morning in Lat. 60° 36′, Long. 3° 58′; and here (Station 76), with a surface-temperature of 50°·3, we found the bottom-temperature 29°·7, at a depth of 344 fathoms, as at Station 65. Keeping on our westward course for 25 miles, we took another Sounding (Station 77) at noon of the same day, which gave us a depth of 560 fathoms, and a bottom-temperature of 29°·8. This Station was only about twelve miles to the S.S.E. of the first point (Station VI.) at which we came upon the Cold area last year; and it was interesting to have so complete a confirmation of the accuracy of that observation, which had given us at the depth of 510 fathoms a temperature of 33°·7, which, when corrected for pressure, would be 31°·6.

82. Changing our course to the southward, we found on the afternoon of the same day (Station 78), after a run of about 20 miles, that the depth had diminished to 290 fathoms, and that the bottom-temperature had risen to 41°·6; from which it appeared that the influence of the frigid stream was not quite so great, in proportion to the depth, as at Station 75, though still very decided. Keeping on to the southward during the night, we crossed the 100-fathom line, and found ourselves early in the morning (Station 79) in Lat. 59° 49′ and Long. 4° 42′, where the depth was only 92 fathoms, and the bottom-temperature 49°·4, with a surface-temperature of 52°·3. It seemed obvious, therefore, that the influence of the frigid stream did not extend over this shallower portion of the bed of the channel; and this conclusion was confirmed by the Soundings which we took at short intervals after altering our course to the N.W., so as to pass again from this plateau into deep water. For after steaming 7 miles we found the depth 92 fathoms, and the bottom-temperature 49°·4; proceeding 7 miles further, the depth was found to have increased to 142 fathoms, while the bottom-temperature was still 49°·1; but a continuance of the same course for only 8 miles showed that the bottom rapidly descends here, as on other parts of the southern border of this channel, the depth at Station 82 having increased to 312 fathoms, whilst the temperature fell to 41°·3, showing a very precise accordance with the condition of Station 78. We were here only about 7 miles to the S.E. of our last year's Station VII., where the depth was 500 fathoms, and the bottom-temperature 32°·2, which when corrected for pressure, would be 30°·1; and it was thus very interesting to see how considerably the bottom-temperature

varied with the depth, on the border of that deeper portion of the channel which gives passage to the Arctic stream. In order to test this yet more completely, we proceeded about 7 miles to the northward (Station 83), so as to be almost exactly in the parallel of our last year's Station VII., but about 7 miles to the eastward of it; and here we found the depth to have increased to 362 fathoms, while the bottom-temperature had fallen to 37°·5. Comparing this, however, with the bottom-temperature of 29°·7, found at Station 76, at which the depth was rather less, it became obvious that the influence of the warm surface-current here extends to a greater depth.

83. Again changing our course to the S.W., in a direction nearly parallel to the 100-fathom line, so as to bring us to a part of the area not previously surveyed, we took a Sounding (Station 84) early in the morning of Saturday, Sept. 4th, in Lat. 59° 34′, and Long. 6° 34′; and found the depth to be 155 fathoms, and the bottom-temperature 49°·1, showing that we were again on a portion of the southern bank too near the surface to be affected by the frigid stream. And as, on sounding again (Station 85), after having run 6 miles in a northerly direction, we found the bottom-temperature to have only fallen to 48°·7, while the depth had increased to 190 fathoms, it was obvious that the same condition still existed. A further run of only 8 miles northwards, however, brought us suddenly into the Arctic stream; the depth (Station 86) being here 445 fathoms, and the bottom-temperature 30°·1.—These very rapid changes of Submarine Climate are of extreme interest in a variety of ways, but especially in their Zoological and Palæozoological relations, as will be shown hereafter.

84. As we were now again approaching a part of the Area which had been previously explored with sufficient minuteness for our present purpose, and as we desired to extend our survey into a part of the Warm area removed from the immediate influence of the Arctic stream, our ship's head was kept to the westward without any stoppage until the morning of Monday, Sept. 6th; when we reached Long. 9° 11′ in Lat. 59° 35′, this point being about 24 miles to the south of Station XIV. in our last year's Cruise. Here a Sounding gave us (Station 87) a depth of 767 fathoms, and a bottom-temperature of 41°·5; and as it thus became obvious that we were in the Warm area, we thought it desirable to obtain a set of *serial* Soundings, for comparison, on the one hand, with those obtained on the Cold area, and, on the other, with those taken in the former Cruises at similar depths on the border of the North Atlantic basin.—The results of these Soundings, given in Table I., p. 456, will be discussed hereafter; and at present it will be sufficient to state that while they show that the influence of the Warm stream here extends through the entire depth, they also indicate that this is modified below 500 fathoms by the frigid stream; the depression of temperature between 500 and 600 fathoms being almost exactly equal to that which presented itself between 100 and 500 fathoms. —Our dredge here came up with the extraordinary load mentioned in the

Introduction (§ 8) as having severely tested the efficiency of our donkey-engine; which, however, proved equal to its work, and landed on our deck *half a ton* of *Globigerina*-ooze, here showing very little intermixture with sand. Like our similar haul at Station XVI. last year, however, this mass contained but a small amount of the higher forms of Annimal life; and as a continuance of our course still further west did not seem likely to furnish any additional results of importance, and as there would have been a risk of exhausting our coal in steaming against a head-wind, we thought it better to change our course towards Stornoway, taking a direction that should bring us again on the ground which we had previously found most productive. In the afternoon of the same day we took another Sounding in Lat. 59° 26′ and Long. 8° 23′, on the line of our outward track in the second part of the 'Lightning' cruise last year, so as to establish the depth and temperature at an intermediate point between two distant stations; and we here (Station 88) found the depth to be 705 fathoms, and the bottom-temperature 42°·6, thus showing a close accordance with the nearest Soundings previously taken.

85. Continuing our easterly course during the night, but making slightly to the northward, so as once more to come upon the *Holtenia*-ground, we sounded early the next morning (Sept. 7) in Lat. 59° 38′, Long. 7° 46′; and found (Station 89) that the depth had diminished to 445 fathoms, whilst the temperature had risen to 45°·6,—thus confirming by *bottom-soundings* the inference we had been led to draw from the *serial* soundings taken at Station 87, that the influence of the frigid stream is exerted even in the Warm area at depths greater than 500 fathoms, in depressing the temperature of the body of water which it there meets, and with which it mixes. Another Sounding taken after a further run of 7 miles in the same direction, which brought us very near to Station 49, gave a similar depth and temperature; but the character of the bottom now indicated the proximity of the Cold area, the *Globigerina*-ooze being here mingled with Sand.

86. We now changed our course to the S.E., and after steaming about ten miles, put down our dredge with its "hempen tangles" upon what we were assured by Capt. Calver was the spot (as nearly as it could be determined) upon which we had made the *first* deep-sea dredging in this Cruise (§ 61); and the result of this *last* visit to our favourite ground was such as to surpass our most sanguine expectations. For the dredge and the tangles alike came up laden with such a collection of the "treasures of the deep," as we feel quite safe in asserting had never before been brought to the surface on any one occasion,—almost every specimen being such as would be accounted an important acquisition to Museums already most complete. *Holtenias* there were by the bucketful; *Hyalonemata* (one of them a new species) with their "flint-rope" covered with the parasitic *Palythoa*, and bearing at their summit the living *Sponge* of which the "flint-rope" constitutes the radix; the beautiful *Tisiphonia*, or mush-

room-shaped Sponge, in abundance; *Adrasta infundibulum*, another Vitreous Sponge allied to *Hyalonema*; and other types of the same group not yet described. The *Echinodermata* also were very numerous, and many of them very large; and they presented a great variety of most interesting types, nearly all of them being new to the British Fauna, though many had been previously described. It was especially interesting to note the very marked difference between the *Echinoderm-Fauna* of this region and that of the Cold area. With the exception of a few species which seem able to maintain their existence through almost any range of depth and temperature, they were all diverse; and whilst the mixture of decidedly Arctic forms, and the dwarfing of Southern types, gave a decidedly Boreal character to the Fauna of the Cold area, there was here a mixture of fully developed Southern types, among them a *Stichaster* either identical with or closely allied to a species described from Madeira. But the specimen of this group most interesting to us (perhaps the most remarkable capture made in the whole of our Cruise) was a large Echinid allied to *Astropyga* (belonging to the Family *Diademidæ*), having a perfectly soft and flexible test; the plates of the corona, though retaining their normal number and arrangement, being very thin and slightly separated from one another by the interposition of a flexible perisome, so that the test resembled an armour of chain-mail, instead of the cuirass with which the ordinary *Echinida* are enveloped. Two specimens of this remarkable type were obtained,—a perfect one at Station 89, and another, considerably injured, but still serving for anatomical investigation, on our *Holtenia*-ground. The perfect specimen is about 5 inches in diameter, and of a brilliant shade of crimson, altogether a most striking object. This form at once recalled the very singular fossil from the White Chalk, two specimens of which are in the British Museum, described by the late Dr. S. P. Woodward under the name of *Echinothuria floris*; and though we would not affirm the actual identity of the existing form with the old, there can be no doubt of their very close affinity, and of the persistence of this remarkable type of structure from the Cretaceous to the present epoch.—Here also we obtained a specimen of almost the only one of the Scandinavian Starfish that we had not met with on British ground, the *Asteronyx Lovéni*, a very interesting modification of the *Astrophyton* type, having the same general plan of structure, but having the arms simple, instead of being subdivided in the manner which has given occasion to the designation *Gorgonocephalus*. It is not a little curious that a dredging we subsequently took in the Minch, near the eastern shore of Skye, brought up six specimens of this rare Echinoderm, thus confirming a surmise previously formed, that the careful exploration of that channel would show that many types would be there found which have been hitherto supposed to be peculiar to Norway.

87. The *Foraminifera* obtained on this and the neighbouring parts of the Warm area presented many features of great interest. As already stated

(§ 61), several *Arenaceous* forms (some of them new) were extremely abundant; but in addition to these we found a great abundance of *Miliolines* of various types, many of them attaining a very unusual and some even an unprecedented size. As last year, we found *Cornuspiræ* resembling in general aspect the large *Operculinæ* of tropical seas, and *Biloculinæ* and *Triloculinæ* far exceeding in dimensions the littoral forms of British shores; and with these were associated *Cristellariæ* of no less remarkable size, presenting every gradation from an almost rectilineal to the Nautiloid form, and having the animal body in so perfect a state as to enable it to be completely isolated by the solution of the shell in dilute acid.—It is very interesting to remark that certain forms of this *Cristellarian* type are among the most characteristic Foraminifera of the Cretaceous as well as of various Tertiary deposits; and the similarity of some of these to existing forms is so close, that the continuity of the type from the Cretaceous epoch cannot be reasonably questioned. It is further interesting to note that it has a great bathymetrical range, no difference showing itself between the *Cristellarians* of our Warm area and those found in the preceding Cruises at nearly three times the depth.—The continuity of Foraminiferal life is further indicated by the occurrence in the *Globigerina*-ooze of a number of *Rotalian* forms which are peculiarly characteristic of the Fauna of the Cretaceous period.

88. The cumulative evidence which we have thus obtained in support of the hypothesis advanced last year ('Lightning' Report, p. 193) as to the uninterrupted continuity of the Cretaceous deposit on the North-Atlantic Sea-bed from the epoch of the Chalk-formation to the present time, will be more fully discussed hereafter. But as, with the exception of the subsequent dredging in the shallow waters of the Minch already referred to (§ 86), our Zoological exploration of the sea-bottom came to a conclusion with the extraordinary *climax* just described, we may here mention an idea which formed the subject of much discourse between us at this period.

89. It is, we believe, the general creed of modern Geologists, that all Calcareous rocks have had, either directly or indirectly, an Organic origin; and that the most perfectly mineralized condition of such rocks affords no evidence to the contrary, there being abundant evidence that all traces of organic structure may be completely obliterated by subsequent metamorphic action. Thus upheaved masses of recent Coral are frequently converted into subcrystalline Limestone, the organic origin of which would not be recognized by any feature in its molecular arrangement or composition; whilst a change often presents itself (as on the Antrim Coast) of a true Chalk into a subcrystalline Marble, under the combined influence of the heat and pressure occasioned by the intrusion of Volcanic rocks. Now since there can be no question that the Chalk-formation in its entirety owes its origin chiefly to the accumulation on the deep-sea bottom of the shells, or their débris, of successive generations of *Foraminifera* which lived and moved and had their being there,—and since there can be as little question

that there must have been deep seas at all Geological periods, and that the changes which modified the climate and depth of the sea-bottom were for the most part very gradual,—the question naturally arises whether we may not carry back the continuity of the accumulation of the Foraminiferal ooze on some part or other of the Ocean-bed into Geological epochs much more remote, and whether it has not had the same large share in the production of the earlier Calcareous deposits that it has undoubtedly had in that of the later. Though it is altogether beyond doubt that some beds of Carboniferous Limestone (for example) were simply Coral reefs, covered with waving Crinoids and swarming with Brachiopod and other Mollusks, there are other parts of this formation which seem to have been deposited in much deeper waters; and to these we should be inclined to ascribe a *Foraminiferal* origin. This hypothesis seems not only probable on general grounds, but is supported by several remarkable facts. It has long been known that certain beds of Limestone of Carboniferous age, in Russia and elsewhere, are almost entirely made up of an aggregation of Foraminiferal shells belonging to the genus *Fusulina**; and Prof. Phillips has described under the name *Endothyra Bowmanni*† a Foraminiferal type which seems nearly allied to *Fusulina*, and which he states to occur in great abundance with *Textularia* in the Mountain-limestone beds of the North of England. Again, Mr. H. B. Brady has lately shown us, in a thin layer of Clay occurring in the midst of Carboniferous-limestone beds near Newcastle, an accumulation of *Arenaceous* Foraminifera closely corresponding in type with the *Saccammina* of Sars, which we found to be abundant, in many of the deeper dredgings of the earlier Cruises, on the eastern border of the North-Atlantic Sea-bed.—To this question, however, we shall recur in the discussion of the General Results of our Deep-Sea explorations.

90. Thoroughly well satisfied with the success of our third Cruise, both in the confirmation and extension it afforded of the conclusions as to the climate we had ventured to draw from the comparatively few and scanty data we had obtained last year, and in the large mass of Zoological novelties we had collected, we now made for Stornoway, and arrived there on the evening of Wednesday, September 8th. If we had been free to dispose of the 'Porcupine,' we might have taken the opportunity of connecting the Third with the First Cruise, by exploring the deep bottom to be found about 200 miles to the west of the Hebrides, as far south as the Rockall bank, which had been the northern limit of the First Cruise. But as our vessel was under orders to make a Hydrographic Survey in the neighbourhood of Valentia, as soon as the scientific work of our Expedi-

* The true zoological position of this Genus, at present only known as a Carboniferous type, has lately been settled by the microscopic examination of the minute structure of the shell of specimens preserved in a *clayey* stratum of the Carboniferous series in Iowa, U.S., kindly forwarded to Dr. Carpenter by Mr. Meek, of Washington. See the Monthly Microscopical Journal for April, 1870.

† Proceedings of the Geological and Polytechnic Society of Yorkshire, 1846, p. 227.

tion should have been accomplished, we did not feel justified in interfering with that duty; since we had no reason to anticipate that such exploration would add any scientific results of importance to those we had already obtained. After coaling and refitting at Stornoway, therefore, we proceeded direct to Belfast, where we landed our collections, and took our leave of the 'Porcupine' and her highly valued Captain and Officers, with an earnest hope that we may again be brought into the same congenial companionship and hearty cooperation in future explorations of the like kind.

GENERAL RESULTS.

Physics and Chemistry.

[For this portion of the Report, Dr. Carpenter holds himself specially responsible; his Colleagues, while concurring generally in his views, being desirous of reserving their liberty to dissent from some of his conclusions.]

91. Among the most important results of the 'Lightning' Expedition was the discovery of the fact that two very different Submarine Climates exist in the deep channel (from 500 to 600 fathoms) lying E.N.E. and W.S.W. between the North of Scotland and the Faroe banks; a *minimum* temperature of 32° being registered in some parts of this channel, whilst in other parts of it, *at the same depths*, and *with the same surface-temperature* (never varying much from 52°) the *minimum* temperature registered was never lower than 46°, thus showing a difference of at least 14°. Though it could not be positively asserted that these *minima* were the *bottom*-temperatures of the Areas in which they respectively occurred, it was argued that they must almost necessarily be so: *first*, because it is highly improbable that Sea-water at 32° should overlie water at any higher temperature, which is specifically lighter than itself, unless the two strata have a motion in different directions sufficiently rapid to be recognizable; and *second*, because the nature of the Animal life found on the bottom of the Cold area exhibited a marked correspondence with its presumed depression of temperature, whilst the drift of which its Sea-bed is composed includes particles of distinctly Volcanic minerals, probably derived from a northern source, — the Sea-bed of the Warm area, on the other hand, being essentially composed of *Globigerina*-mud, and supporting a Fauna of a warmer temperate character.—This conclusion, it is obvious, would not be invalidated by any error arising from the effect of Pressure on the bulbs of the Thermometers; since, although the *actual* temperatures might be (as was then surmised) from 2° to 4° below the *recorded* temperatures, the *difference* between them would remain unaffected, the pressure exerting exactly the same influence at the same depth, whether the Soundings were taken in the Cold or in the Warm area.

92. The existence in the Cold area of a *minimum* temperature of 32°,

with a Fauna essentially Boreal, could not, it was argued, be accounted for in any other way than by the supposition of an under-current of Polar water coming down from the North or North-east; whilst, conversely, the existence in the Warm area of a *minimum* temperature of 46°, extending to 500 or 600 fathoms' depth in the Latitude of 60° (of which the normal deep-water temperature would be at least 8° less), together with the warmer temperate character of its Fauna, seemed equally indicative of a flow of Equatorial water from the South or South-west. How far this flow is part of the "Gulf-stream" proper,—that is, of the current of heated water which issues through the "Narrows" from the Gulf of Mexico,—or is attributable to some more general cause, was reserved as a matter still open to discussion; but it was urged that the existence of two such different Submarine Climates in such close proximity may be taken as an example of that continual interchange between the *Ocean-waters* of Equatorial and Polar regions, which is as much a Physical necessity as that interchange of *Air* which has so large a share in the production of winds. For the water that is cooled by the Polar atmosphere must sink and displace the water that is warmer than itself, pushing it away towards the Equator, so that in the *deepest* parts of the Ocean there will be a progressive movement in the *Equatorial* direction; whilst, conversely, the water heated by the Tropical sun, being the lighter, will spread itself north and south over the *surface* of the ocean, and will thus move towards the *Polar* regions, losing its heat as it approaches them, until it is there so much reduced in temperature as to sink to the bottom, and thence return towards its source.

93. The doctrine of the *Warm and Cold Areas*, and of the probable source of their difference, has been fully and carefully tested by the Temperature-soundings taken during the Third Cruise of the 'Porcupine;' and the result has been a complete confirmation of it in every particular; whilst an entirely new and important set of data has been afforded by the Temperature-soundings taken during the First and Second Cruises, in support of the doctrine that *a general interchange of Equatorial and Polar waters* is continually taking place in the great Oceanic basins.

94. The total number of Temperature-soundings taken during the 'Lightning' Expedition, in water of more than 100 fathoms' depth, was only 15; of which 8 were in the Warm area, and 6 in the Cold. These were all Bottom-soundings only. The total number of Stations at which Temperature-soundings were taken during the Third Cruise of the 'Porcupine,' in water of more than 100 fathoms, was 36; of these, 17 were in the Cold area, and 14 in the Warm, whilst 5 showed an intermediate range, in accordance with their border position. But besides these Bottom-soundings, *Serial* Soundings were taken at different depths in three Stations; of which No. 87 was in the Warm area, and Nos. 52 and 64 in the Cold. In the *first* of these, which was at a point about 125 miles to the N.W. of Stornoway, the temperatures were taken at 50, 100, 150,

200, 300, 400, 500, 600, and 767 fathoms (bottom) respectively, with the result of showing a reduction of only 11°·2 at the last-mentioned depth; in the *second*, which was near the S.E. border of the Faroe Bank, the temperature was taken at every 50 fathoms down to 300, and then at 384 fathoms (bottom), showing a reduction of 21°·5; while in the *third*, which was nearly midway between the Faroe and the Shetland Islands, the temperature was taken at every 50 fathoms down to 600, and then at 640 fathoms (bottom), showing a reduction of 20°·1. Of these Serial Soundings there were in all 26, making, with the 36 Bottom-soundings, a total of 62.

95. With these results, obtained with Thermometers upon which complete reliance can be placed, those obtained last year with the best ordinary Thermometers are found to be in close accordance, when the proper correction for pressure is applied to them. Thus No. 47 Sounding of the 'Porcupine' having been taken in almost exactly the same spot of the Warm area as No. xii. of the 'Lightning,'—namely, on what we now call the "*Holtenia*-ground" (§ 61),—the former gave 43°·8 as the *minimum* temperature at 542 fathoms, while the latter gave 47°·3 as the *minimum* at 530 fathoms: and the difference of 3°·5 exceeds by scarcely more than a degree—which may be a mere seasonal variation—the error (about 2°·1) which the pressure of water at that depth would produce in the unprotected thermometers. On the other hand, No. 55 Sounding of the 'Porcupine' having been taken in the same part of the Cold area as No. viii. of the 'Lightning,'—the distance between the two being only about 8 miles,—the former gave 29°·8 as the *minimum* at 605 fathoms, while the latter gave 32° as the *minimum* at 550 fathoms; and the difference of 2°·2 is exactly equivalent to the correction for pressure at that depth in the unprotected thermometers. Thus the difference between the two 'Lightning' Soundings in the Warm and Cold areas respectively having been 15°·3, the difference between the two corresponding 'Porcupine' Soundings was 14°. This very near accordance gave us, of course, a feeling of great satisfaction in our last year's work; and it fully justified our conclusion that whatever might be the pressure-correction required by the instruments then employed, it would not affect the *differences* obtained at nearly approximating depths. It further justifies us in assuming the correctness (when thus rectified) of the *minimum* temperatures taken last year at stations considerably westward of the ground over which we worked in the 'Porcupine.'

96. The data thus obtained respecting the Temperatures at different Depths in the Warm and Cold areas respectively, are correlated in Table I., which includes, with the three sets of *Serial* Soundings, all the *Bottom*-sounding that accord with them. The localities of the several Soundings are indicated by their Numbers in Diagram III.

Table I.

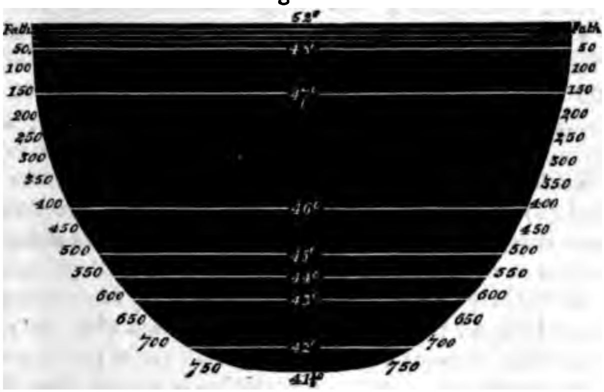
Temperature of the Sea at different Depths in the Warm and Cold Areas lying between the North of Scotland, the Shetland Isles, and the Faroe Islands; as ascertained by *Serial* and by *Bottom*-Soundings. (N.B. The Roman Numerals indicate the 'Lightning' Temperature-Soundings, corrected for pressure.)

Warm Area.						Cold Area.						
Series 87.		Station No.	Depth.	Surface Temperature.	Bottom Temperature.	Series 64.		Ser. 52.	Station No.	Depth.	Surface Temperature.	Bottom Temperature.
Depth.	Temperature.					Depth.	Temperature.	Temperature.				
fathoms.	° Fahr.		fathoms.	° Fahr.	° Fahr.	fathoms.	° Fahr.	° Fahr.		fathoms.	° Fahr.	° Fahr.
0	52·6					0	49·7	52·1				
50	48·1	73	84	52·7	48·8	50	45·5	48·5	70	66	53·4	45·2
		80	92	53·2	49·4				69	67	53·5	43·8
100	47·3					100	45·0	47·3	68	75	52·5	44·0
		71	103	53·0	48·6				61	114	50·4	45·0
		81	142	53·3	49·1				62	125	49·6	44·6
150	47·0	84	155	54·3	49·2	150	43·3	46·5	60	167	49·5	44·3
		85	190	53·9	48·7				IX.	170	52·0	41·0
200	46·8					200	39·6	45·6				
		74	203	52·5	47·7							
						250	34·3	38·4				
300	46·6					300	32·4	30·8				
									63	317	49·0	30·3
									65	345	52·0	29·9
									76	344	50·3	29·7
		50	355	52·6	46·2	350	31·4	...	54	363	52·5	31·4
		46	374	53·9	46·0	384	...	30·6				
400	46·1					400	31·0	...	86	445	53·6	30·1
		89	445	53·1	45·6	450	30·6	...				
		90	458	53·1	45·2				56	480	52·6	30·7
		49	475	53·6	45·4				53	490	52·1	30·0
500	45·1					500	30·1	...	X.	500	51·0	30·8
		XII.	530	52·5	44·8				58	540	51·5	30·8
		47	542	54·0	43·8				VIII.	550	53·0	29·8
		XV.	570	52·0	43·5	550	30·1	...	77	560	50·9	29·8
									59	580	52·7	29·7
600	43·0					600	29·9	...				
		XVII.	620	52·0	43·5				55	605	52·6	29·8
		XIV.	650	53·0	42·5				57	632	52·0	30·5
						640	29·6	...				
700												
		88	705	53·5	42·7							
767	41·4											

On examining the Series taken at Station 87 in the *Warm* area, we notice (1) that, the Surface-temperature being 52°·6, there is a fall of 4°·5 in the first 50 fathoms; (2) that from 50 to 500 fathoms there is a slow progressive and nearly uniform descent amounting in the whole to 3°, which is at the average rate of about 0°·7 per 100 fathoms; and (3) that this descent increases to 2°·1 in the next 100 fathoms, and amounts to 1°·6 in the interval of 167 fathoms between 600 fathoms and the bottom. The re-

lation between Depth and Temperature in the Warm area is represented diagrammatically in the accompanying Figure; in which (omitting fractional parts) each line marks a descent of 1° Fahr.:—

Diagram I.

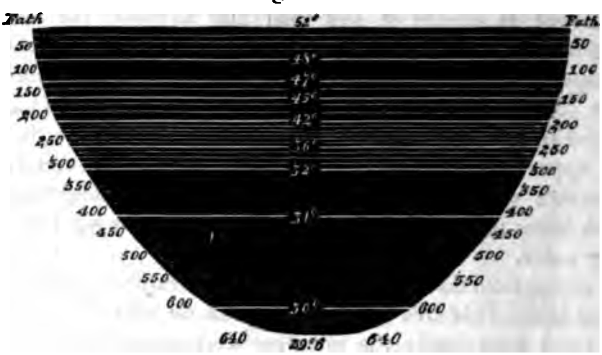


Further, on comparing this Series with the *bottom*-soundings taken in various parts of the same area, the accordance is found to be extremely close; no difference of more than a degree presenting itself anywhere, except at depths of less than 200 fathoms, the bottom-temperatures of which are higher by from 1° to 2°·2 than the temperatures at corresponding depths in the *serial* sounding. This accordance becomes at once evident when the upper curve of Diagram IV., which is constructed from twelve *bottom*-soundings in the Warm area, is compared with the upper curve in Diagram III. which represents the *serial* soundings at Station 87; while the slight difference is just what might be expected, when it is borne in mind that the superficial stratum is not here underlaid by colder water.

97. Turning from these to the Series of Temperature-soundings taken at Station 52 in the *Cold* area (distant less than 60 miles from Station 87), which begins from nearly the same surface-temperature (52°·1), we see (1) that the descent during the first 50 fathoms corresponds so closely with that observed in Series 87, that the two temperatures at that depth are almost precisely the same; (2) that at 100 fathoms the temperatures in the two series are identical; (3) that at the depths of 150 and 200 fathoms there is only a very slight difference; but that (4) whilst the reduction between 200 and 300 fathoms in the Warm area is only 0°·2, it amounts to not less than 14°·8 in the Cold area, bringing down the temperature at that depth to 30°·8; and that (5) this is further reduced to 30°·6 at the bottom of 384 fathoms.—Thus it is evident that a temperature of 32° would have been reached at somewhat less than 300 fathoms, and that the temperature of the water occupying the 100 fathoms beneath was absolutely below the freezing-point of fresh water.

98. This result comes out even more strikingly in another Series (No. 64) taken about 180 miles to the N.E. of the preceding, in the deep channel between the Faroe and Shetland Islands. For we observe (1) that the surface-temperature is here 49°·7, or 2°·4 below that of No. 52; (2) that this difference is maintained with slight variation down to 150 fathoms; (3) that a rapid descent of the thermometer here begins, a fall of 3°·7 taking place between 150 and 200 fathoms, and a further fall of 5°·3 between 200 and 250 fathoms, making a reduction of 9° in 100 fathoms, and bringing down the temperature at 250 fathoms to 34°·3; whilst (4) the fall between 250 and 300 fathoms is only 1°·9, and between 300 and 350 fathoms is 1°, bringing down the temperature at the latter depth to 31°·4; and (5) that in descending through the lowest 290 fathoms, the temperature is reduced to 30°·1 at 500 fathoms, and stands as low as 29°·6 on the bottom at 640 fathoms. The relation between Depth and Temperature in the Cold area is represented diagrammatically in the accompanying Figure; in which, for the sake of better comparison with the preceding, the upper portion is constructed from Series 52 (so as to commence from the surface-temperature of 52°), and the lower portion from Series 64, each line marking a descent of 1° Fahr.

Diagram II.



99. Hence it is evident that a temperature of 32° would have been reached at something more than 300 (say 320) fathoms; so that the *lower half of the water occupying the deepest part of this channel forms a stream nearly* 2000 *feet in depth, having a temperature below the freezing-point of fresh water*; and this notwithstanding that the temperature of its surface and of its first 150 fathoms' depth does not differ more from the temperature of the surface and of the first 150 fathoms in the Warm area (Series 87) than is accounted for by the difference of Latitude (nearly 2°) between the two stations.—These remarkable facts are expressed by the *two lower* curves in Diagram III., which are constructed from the Serial soundings in the Cold area, as the *upper* curve is from the Serial sounding in the Warm area.

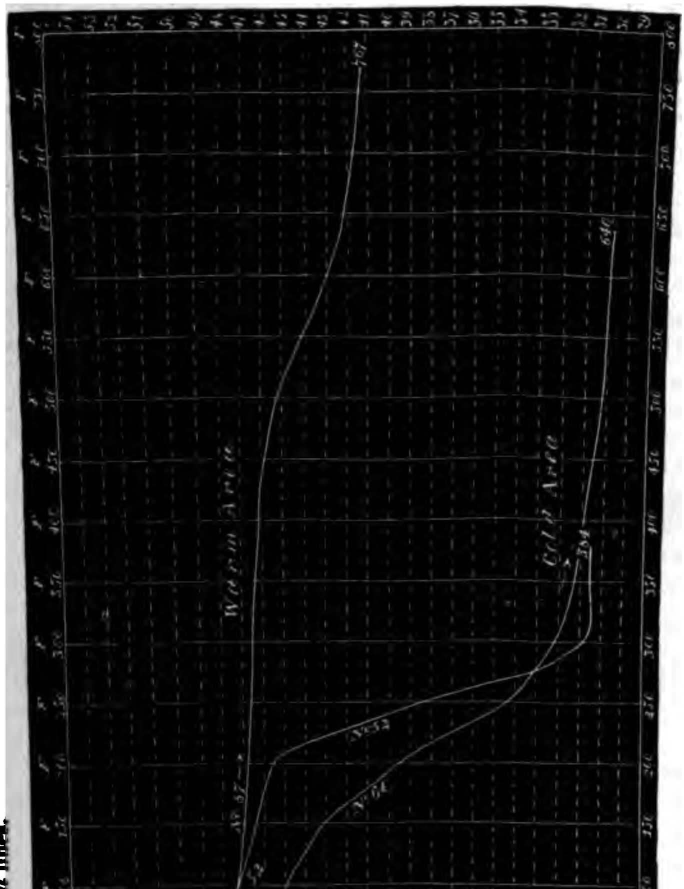


Diagram III.—Curves constructed from Serial Soundings in the Warm and Cold Areas, the Depths being represented by the *vertical* lines, and the Degrees of Fahrenheit's Thermometer by the *horizontal* lines.

100. Now on comparing these two series of Soundings with the *Bottom-soundings* taken at different parts of the Cold area, the accordance is found to be extremely close, no difference of more than a degree being found anywhere at depths greater than 300 fathoms. It is worthy of note that at the shallower depths of from 114 to 167 fathoms (Nos. 60, 61, 62), the bottom-temperatures correspond more closely to the temperatures of the same depths in Series 64 than to those of Series 52, the cold water coming nearer to the surface; and this was still more remarkably the case with No. IX., the sounding obtained last year on a bank at 170 fathoms ('Lightning' Report, § 13). On referring to the Chart it will be found that these four stations

TABLE II.

ntermediate Bottom-temperatures, showing the intermixture of Warm and Cold Currents on the Borders of the Warm and Cold Areas.

Station No.	Depth.	Surface Tempe-rature.	Bottom Tempe-rature.	Station No.	Depth.	Surface Tempe-rature.	Bottom Tempe-rature.
	fathoms.	°	°		fathoms.	°	°
72	76	52·3	48·8	75	250	51·5	41·9
79	76	52·2	48·9	78	290	52·2	41·6
73	84	52·7	48·8	82	312	52·3	41·3
71	103	53·0	48·6	83	362	53·2	37·5
74	203	52·5	47·7				
66	267	52·4	45·7	51	440	51·6	42·0

rface-current that comes up from the S.W. obviously extends over that nk, so as to modify in greater or less degree, according to the depth, the ect of the deep *cold* current coming down from the N.E. The intermixture the two is well seen in Nos. 75, 78, 82, and 83, the depths of which nge from 250 to 362 fathoms; but at the shallower depths, ranging om 76 to 203 fathoms, at which Nos. 66, 71, 72, 73, 74, and 79 were ken, the influence of the *warm* surface-current is obviously predominant. n the other hand, the position of No. 51 marks it as just on the border ound between No. 50, which was taken at or near the northern margin the *warm* current, and Nos. 52, 53, which clearly lie within the southern argin of the *cold*; and we thus see how the *southern* and *deeper* portion the cold current may here lose itself by intermixture with the warm; whilst e northern portion seems to flow onwards unchanged over the shallower ttom, until, having passed the Faroe Banks, it runs down the slope rming the eastern margin of the great Atlantic basin, to the deeper waters which it helps to impart the coldness by which they will presently be own to be characterized.

101. Although we have spoken of "currents," it is not to be inferred that e have detected any actual opposing movements in the waters of the two reas respectively, or in the warm superficial stratum of the Cold area compared with its deep frigid layer. But it may be assumed as a ysical necessity that a great body of ice-cold water could not be always read over the bottom of a large area between Lat. 59½° and Lat. 62°, ten to a depth of 2000 feet, unless it had arrived thither from within the rctic circle; and, conversely, it can scarcely be conceived that the upper ratum of this very area should maintain a temperature equal to that of e Warm area (a slight allowance being made for difference of Latitude), ithout a continual flow of a warmer stream from some southerly quarter. A further indication of the derivation of the deep water of the Cold rea from a northern source is afforded by the presence, among the nall stones and sand brought up from this bottom, of Volcanic detritus, hich seems to have been brought southwards either from the Faroe lands or from some more remote source, such as Jan Meyen. The

presence of Volcanic detritus on that part of the floor of the channel between the Faroe Islands and Iceland which lies between its deepest point and the S.E. shore of Iceland has been already urged by Dr. Wallich *, with great force, as an argument for the existence of "an offshoot of the Arctic current slowly moving downwards" in a line about 250 miles to the westward of that which we consider ourselves to have now conclusively established; and it can scarcely be doubted that a set of Temperature-soundings taken in "the 682-fathom locality about forty miles from the southern shore of Iceland" would give Thermometric results similar to those we have obtained in the corresponding channel between the Faroe and Shetland Isles.—The import of the presence of similar Volcanic detritus on the bed of the Mid-Atlantic, as first pointed out by Prof. Bailey, will be considered hereafter (§ 117).

102. Although the thermal condition of the Warm area does not afford the like striking evidence of the derivation of its whole body of water from a Southern source, yet a careful examination of its phenomena seems fairly to justify such an inference. For it has been shown by the Serial sounding No. 87 in Lat. 59° 35′, that while the surface-water is about 4½° warmer than the water at 50 fathoms' depth, the latter is only 0°·8 warmer than the water at 100 fathoms; and that below this the thermometer remains almost stationary down to 400 fathoms. Now at that depth it is only 2°·4 colder than water at the same depth (Station 42) at the northern border of the Bay of Biscay, in a Latitude more than 10° to the south, where the surface-temperature was 62°·7; and the approximation of the two temperatures is yet nearer at still greater depths, the bottom-temperature at 767 fathoms at Station 87 being 41°·4, whilst the temperature at 750 fathoms' depth at Station 42 is 42°·5. So great an excess above the Isotherm of Lat. 59° 35′ can scarcely be attributed to the summer atmosphere of the locality, which we scarcely ever observed to be above 54°, and of which the effect, if exerted at all, seems limited to the "superheating" of the superficial stratum. It is obvious, again, that the surface-drift caused by the prevalence of South-westerly winds, to which some have attributed the phenomena usually assigned to the extension of the Gulf-stream to these regions, cannot account for such an elevation of temperature in a stratum altogether removed from its agency; and it seems equally difficult to conceive that in a region so remote from the source of the Gulf-stream proper, *its* influence, even if exerted in an elevation of the surface-temperature, should extend to a depth of at least 400 fathoms. It may be pretty certainly affirmed, indeed, that the effect of the warm current is exerted to the *very bottom* of the Warm area: for its temperature even at 767 fathoms is 41°·4, which is several degrees above the theoretical isotherm of the latitude; and such a temperature could scarcely be maintained at this elevation against the depressing influence of the Polar current which here mingles with it, were it not for a continual influx of warm water from a Southern source.

* North-Atlantic Sea-bed, pp. 5–7.

103. Thus the doctrine of *a general interchange between Polar and Equatorial Waters* (§ 92) seems the only hypothesis that is competent to account for the facts of this case*; and it will be found to derive further support from the Temperature-phenomena of the North-Atlantic basin, which we shall presently discuss on the basis of the Thermometric observations taken in the *First* and *Second* Cruises of the 'Porcupine,' with additional evidence from other sources.—Before proceeding to these, however, we shall inquire whether any *rationale* can be given for the special peculiarity of the Arctic current, which produces the depression of temperature to from 32°–30° everywhere noticeable at depths of from 300–640 fathoms in our Cold area.

104. A glance at the North Polar region, as laid down either on a Globe, or on any projection of which the Pole is the centre†,—as in the accompanying Chart (Plate 7) shows that the Polar Basin is so much shut-in by the northern shores of the European, Asiatic, and American Continents, that its only outlet, besides the narrow and shallow channel of Behring's Straits, and the circuitous passages leading into Hudson's and Baffin's Bays, is the space which intervenes between the eastern coast of Greenland and the north-western coast of the Scandinavian Peninsula. If, therefore, there be any such general movement of ice-cold water towards the Equatorial regions as that for which we have argued, this movement must take place mainly through the deeper portions of this interspace; at the north of which lies Spitzbergen, whilst Iceland and the Faroes lie in the middle of its southerly expanse. Now in the western portion of this channel, lying between Greenland and Iceland, the depth of water for the most part ranges from 800 fathoms to nearly double that amount; and there will here, therefore, be a free exit to the water which has been cooled down within the Arctic basin, and has consequently subsided to its deeper portions. But on the eastern side of Iceland the case is very dif-

* The existence of "Polar Currents" beneath the heated waters of Tropical regions had been indicated by various observers (see 'Lightning' Report, p. 186); but they seem to have been generally, if not universally, regarded as local peculiarities. Conversely, a movement of Equatorial water in the Polar direction, quite independent of such local accidents as those which produce the Gulf-stream proper, had been noticed in several localities; particularly between the Indian and Antarctic Oceans (see Maury's 'Physical Geography of the Sea,' §§ 748–750), where the whole movement is forced to take place towards the *South* pole, by the barrier interposed by the Continent of Asia to any flow in a *northerly* direction.—The real import of such facts as these could not be recognized by Physical Geographers, so long as they were under the "dominant idea" of a *uniform deep-sea temperature of* 39°; and our present endeavour is simply to show that the doctrine of Oceanic circulation, being at the same time in accordance with Physical theory (as laid down by Prof. Buff), and consonant with all the reliable facts yet observed, is entitled to the same rank as a fundamental principle in the science of Physical Geography, as the parallel doctrine of Atmospheric circulation holds in Meteorology.

† The ordinary Hemispherical projection of our Atlases does not give by any means a correct idea of this Polar Basin; and the Mercator's projection (which is employed by Dr. Wallich) so exaggerates the Longitude-distances in high Latitudes, as to give an entirely fallacious conception of it.

ferent. Save in the narrow channel of 682 fathoms already mentioned as existing near the S.E. of Iceland, there is no depth as great as 300 fathoms along the whole bottom as far as the Faroe Islands*; and an effectual barrier is thus interposed to any current moving southwards at a depth exceeding this. A similar barrier is presented, not merely by the plateau on which the British Islands rest, but also by the bed of the North Sea; which (as its depth nowhere exceeds 100 fathoms between the coast-line of the British Isles from Shetland to Dover on one side, and the coast-line of Norway, Denmark, and Holland from Bergen to Ostend on the other) must give to such a movement a not less effectual check than would be afforded by an actual coast-line uniting the Shetland Islands with Norway. Consequently it is obvious that a flow of ice-cold water at a depth exceeding 300 fathoms from the surface, down the north-eastern portion of this interspace, *can* only find its way southwards through the deep channel between the Faroe and Shetland Islands, which will turn it into a S.W. course, and finally discharge it into the great North-Atlantic basin, where it will meet the Icelandic and Greenland currents, and unite with them in spreading over the deepest portions of the sea-bed.

105. Hence it is obvious that if a subsidence were to take place in the area now covered by the North Sea and the British Channel, so as to depress their bottom *below* the level of that of the channel between the Faroe and Shetland Islands, the course of the Arctic current would be deflected from the latter to the former, *lowering* its bottom-temperature by at least 14°; and as the warmer current coming up from the S.W., and now ocupying our Warm area, would then meet with no check, it would extend itself over the whole of what is now our Cold area, and would *raise* its temperature at least 12°. This would have the general effect of altering almost the entire Fauna of both regions; and of modifying the characters of the deposit forming on the bottom of each.

106. *Atlantic Basin.*—During the First and Second cruises of the 'Porcupine,' the Temperature of the eastern border of the great North-Atlantic basin was examined at various depths and in widely different localities. Serial soundings were taken at no fewer than *seven* stations; the most Northerly of these being not far from Rockall Bank in Lat. 56° 8′, whilst the most Southerly was at the northern border of the Bay of Biscay, nearly 300 miles to the west of Ushant, and in Lat. 47° 38′. At Station 42 the temperature was taken at every 50 fathoms, from the surface downwards to the bottom at 862 fathoms; at Station 23 the temperature was taken at every 100 fathoms, to the bottom at 630 fathoms; and at the other Stations, at which the depths ranged from 1263 to 2090 fathoms, the Soundings were taken at every 250 fathoms.—Besides these, the *Bottom*-temperature was taken at upwards of 30 Stations, ranging in Latitude from 56° 58′ to 47° 38′, and in Depth from 54 to 2435 fathoms.—The most important of the results thus obtained are presented in Table III.

* See Dr. Wallich's 'North-Atlantic Sea-bed,' chap. i.

Table III.

Temperature of the Sea at different Depths near the Western margin of the North-Atlantic Basin, as ascertained by *Serial* and by *Bottom*-Soundings.

Serial Soundings.								Bottom-Soundings.			
Depth.	Temperature. Ser. 23.	Temperature. Ser. 42.	Temperature. Ser. 22.	Temperature. Ser. 19.	Temperature. Ser. 20.	Temperature. Ser. 21.	Temperature. Ser. 38.	Station No.	Depth.	Surface Temperature.	Bottom Temperature.
fathoms.	° Fahr.	° Fahr.	° Fahr.	° Fahr.	° Fahr.	° Fahr.	° Fahr.		fathoms.	° Fahr.	° Fahr.
0	57·3	62·6	56·9	54·8	55·5	56·2	64·0				
50	...	53·2						27	54	55·6	48·3
								34	75	66·0	49·7
								6	90	54·0	50·0
								35	96	63·4	51·3
100	48·5	51·1						8	106	54·2	51·2
								24	109	57·7	46·5
150	...	50·9						7	159	53·2	50·4
								14	173	53·2	49·6
								18	183	53·2	49·4
200	48·0	50·5						13	208	53·6	49·6
250	...	50·2	48·5	48·0	48·5	48·3	50·5	4	251	53·5	49·5
300	47·8	49·6						26	345	57·4	46·7
350	...	49·1						1	370	54·0	49·0
400	47·5	48·5						15	422	52·2	47·0
450	...	47·6						45	458	60·7	48·1
500	45·8	47·4	46·7	46·7	46·9	47·5	47·8	40	517	63·4	47·7
550	...	46·4						39	557	63·0	47·0
								41	584	63·4	46·5
600	44·5	45·5									
630	43·4										
650	...	44·3						23*b*	664	57·4	41·6
700	...	43·6						12	670	52·2	42·6
								3	723	54·5	43·0
								36	725	63·9	43·9
750	...	42·5	42·0	41·2	41·6	42·4	41·3				
800	...	42·0									
								2	808	54·1	41·4
862	...	39·7						16	816	53·0	39·5
1000	...	...	38·8	38·5	38·8	38·5	38·3	44	865	61·2	39·4
1263	...	...	37·3					43	1207	61·7	37·7
								28	1215	57·7	37·1
1250	...	...	...	...	37·7	37·9	37·7	17	1230	53·2	37·8
1300								29	1264	56·9	36·9
1360	...	...	...	37·4				32	1320	55·9	37·4
1400								30	1380	56·0	37·1
1443	...	...	...	...	37·0						
1476	...	...	...	...	...	36·9					
1500	...	...	...	...	...	...	37·2				
1750	...	...	...	...	...	...	36·7				
2090	...	...	...	...	...	...	36·3	37	2435	65·6	36·5

107. Amongst all these the coincidence of Temperatures at corresponding Depths is extraordinarily close; the chief differences show themselves, as might be expected, in Surface-temperature. This was peculiarly high in the most Southerly stations, which lay between Lat. 47° and 49° 51′, rising to 62°·6 at Station 42, to 64°·8 at Station 38, to 65°·6 at Station

37 (which was that of the 2435 fathoms' dredging), and to 66° at Station 34: whilst it fell in the more Northerly Stations, which lay between Lat. 53° 41′ and 54° 53′, to 54°·8 at Station 19, to 53°·2 at Stations 17 and 18, though these were rather to the southward of the preceding, and to 52°·2 at Station 12, which was yet further south. A comparison of the temperature of the Surface-water with that of the Air at each Station indicates that a large part of the variation in the former is due, on the one hand, to the heating effect of the solar rays, and on the other to the cooling influence of winds. Thus at three Stations at which the Surface-temperatures were 64°·8, 65°·6, and 66° respectively, the thermometers in Air showed 63°·5, 70°, and 72°; whilst at four Stations at which the Surface-temperatures ranged downwards from 54°·8 to 52°·2, the temperature of the Air ranged from 55°·5 to 53°. In only one instance was the temperature of the Air decidedly lower than that of the Surface-water; and this was at Station 42, where, although the Surface-temperature was 62°·6, the temperature of the Air was only 59°. But as this observation was made at 4^{h} 30^{m} on the morning of July 27, and as the wind was from the N.W., the discrepancy may be regarded as accidental.

108. At the last-mentioned Station, in Lat. 49° 12′, about 250 miles to the S.W. of Cork, a Series of Temperature-soundings was taken at every 10 fathoms from the surface to 50 fathoms, with the view of determining the rate of thermal decrease at successive depths in the superficial stratum. The total decrease in this descent amounted to 9°·4; the most rapid diminution being between 20 and 30 fathoms, within which vertical space the reduction amounted to 3°·4. In the next 10 fathoms it was only 1°·6, and in the 10 following only 1°·2. Between 50 and 100 fathoms the total reduction was only 2°·1; and it may be fairly surmised that a large part of this occurred in the upper 20 fathoms; for below 100 fathoms the rate of diminution becomes extremely slow, the total reduction between 100 and 500 fathoms being only 3°·7, or at an average of 0·9 per 100 fathoms. The rate of diminution then again becomes more rapid, the total reduction between 500 and 800 fathoms being 5°·4, or 1°·8 per 100 fathoms; and between 800 and 862 fathoms (bottom) there is a still more rapid diminution, a reduction of 2°·3 taking place in this comparatively small descent.

109. On comparing with this the Series taken at every 100 fathoms at Station 23, in Lat. 56° 13′, we see a very close general accordance in the rate of descent; although the actual temperatures of the latter are from 2° to 3° lower than those of the former at corresponding depths, as might be expected from its higher Latitude. In the Surface-temperature, indeed, the difference amounts to 5°·3; but this becomes reduced to 2°·6 at 100 fathoms, to 2°·5 at 200 fathoms, to 1°·8 at 300 fathoms, and to 1°·0 at 400 and 600 fathoms. The total reduction in the first 100 fathoms is here 8°·8, as against 11°·5 in the preceding case; while the reduction between 100 and 500 is also rather less, being 2°·7.

110. Extending the comparison to a Series taken still further northwards,

namely at Station 87 in Lat. 59° 35′ (more than 10° to the north of Station 42), the same general accordance presents itself in the rate of descent; while the actual temperatures at the several depths below 100 fathoms are by no means as different as might be expected from the difference in the geographical position of the Stations, as will be apparent from the following Table:—

TABLE IV.

Comparative Rates of Reduction of Temperature with Increase of Depth, at three Stations in different Latitudes, all of them on the Eastern Margin of the Atlantic Basin.

Depth.	Station 42. Lat. 49° 12′.		Station 23. Lat. 56° 13′.		Station 87. Lat. 59° 35′.	
	Temperature.	Difference.	Temperature.	Difference.	Temperature.	Difference.
fathoms. Surface	° 62·6		° 57·3		° 52·5	
		° 11·5		° 8·8		° 5·2
100......	51·1		48·5		47·3	
		0·6		0·5		0·5
200......	50·5		48·0		46·8	
		0·9		0·2		0·2
300......	49·6		47·8		46·6	
		1·1		0·3		0·5
400......	48·5		47·5		46·1	
		1·8		1·7		1·0
500......	46·7		45·8		45·1	
		1·2		1·3		2·1
600......	45·5		44·5	......	43·0	
		3·0				
750......	42·5	......	......	......	......	1·6
767......	......	......	......	......	41·4	

Although the difference between the Surface-temperatures at Stations 42 and 87 amounts to 10°·1, the difference is reduced to 3°·8 at 100 fathoms, to 3° at 300 fathoms, to 2°·5 at 600 fathoms, and to 1°·1 at 750 fathoms. So, again, the reduction in the first 100 fathoms at Station 87 being only 5°·2, the total reduction between 100 and 500 fathoms is only 2°·2, or at the rate of 0°·55 per 100 fathoms. But the rate of depression then undergoes nearly as marked an increase as at the corresponding depth in Station 42; for whilst the diminution of temperature between 500 and 750 fathoms amounts at Station 42 to 4°·2, or 1°·7 per 100 fathoms, it amounts at Station 87 to 3°·7, or 1°·5 per 100 fathoms.

111. It becomes obvious, therefore, in the *first* place, that there is a decided *superheating* of the superficial stratum, not extending to a depth much greater than 70 or 80 fathoms, and that this is more considerable (as might be expected) at the Southern than at the Northern stations. Whether this "superheating" is entirely due to the direct influence of solar heat, or depends in any degree (especially in the southern portion of this area) on an

extension of the Gulf-stream, is a question which can only be resolved by the determination of its relative amount in summer and in winter; and as this solution could be very easily obtained (sets of Temperature-soundings at every 10 fathoms down to 100 fathoms, taken in these opposite periods of the year, being all that is requisite), it may be hoped that the cause of this "superheating" will not long remain undetermined.

112. With regard, *secondly*, to the Temperature of the 400 fathoms beneath the superficial 100, which ranges between 51°·1 and 46°·7 in Lat. 49° 12′, between 48°·5 and 45°·8 in Lat. 56° 13′, and between 47°·3 and 45°·1 in Lat. 59° 35′, it may be pretty certainly affirmed that whilst it is somewhat higher than the Isotherm of the Southern station, it is so considerably above that of the Isotherms of the Northern stations, as decidedly to indicate that the body of water between these depths has found its way thither from a Southern source (see § 102).

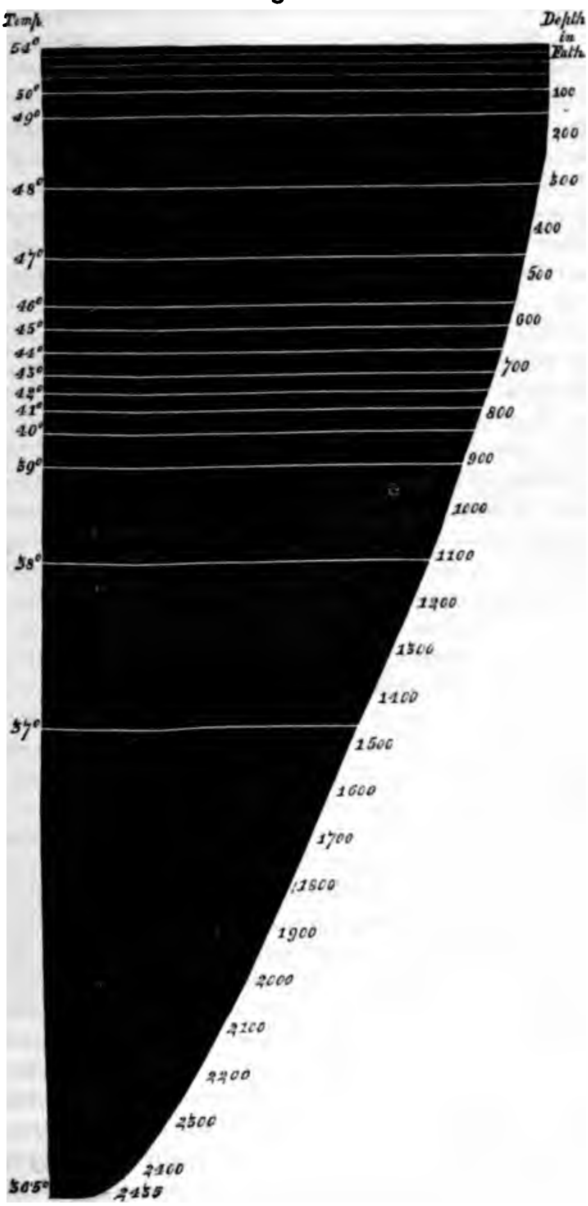
113. Proceeding, *thirdly*, to the still greater depths of which the Temperatures are recorded in Series 22 (1263 fath.), Ser. 19 (1360 fath.), Ser. 20 (1443 fath.), Ser. 21 (1476 fath.), and Ser. 38 (2090 fath.), all which are in remarkably close accordance with each other, we meet with a decided change in the rate of decrease of temperature at equal intervals of depth; for whilst the average of the whole five gives a reduction of no more than 1°·6 between 250 and 500 fathoms (that is, 0°·6 per 100 fathoms), the reduction between 500 and 750 fathoms is 5°·4, or at the rate of 2°·1 per 100 fathoms; while between 750 and 1000 fathoms it amounts to 3°·1, bringing down the temperature at the latter depth to an average of 38°·6. Though the rate of diminution of temperature then becomes slower, there is still a progressive decrease of temperature with increase of depth, the total reduction between 1000 and 2090 fathoms being just 2°, so as to bring down the temperature at the latter depth to 36°·3.

114. With these Series the numerous *Bottom*-temperatures taken in the First and Second Cruises, and tabulated in Table III., are for the most part in remarkably close accordance. This accordance is greatest at depths between 1000 and 2435 fathoms; the temperature at the last-mentioned depth showing no reduction* below that of the 2090 fathoms' sounding. The accordance between the Serial and the Bottom-soundings is not so constant, however, at smaller depths; the temperature of the *bottom* being in several instances from two to four degrees lower than that of the corresponding stratum in the serial soundings. Thus in No. 24, at a depth of only 109 fathoms, the bottom-temperature was 46°·5, or 4° below the ordinary temperature at that depth. In No. 26, at a depth of 345 fathoms, the bottom temperature was 46°·7; at least 2° below the average. In No. 23 *b*, at 664 fathoms, the bottom-temperature was 41°·6, and in No. 12, at 670 fathoms, the bottom-temperature was 42°·6, being in the one case about 2½° and in the other about 1½° below what might have been expected at those depths. These differences suggest the hypothesis that variations in

* Its apparent *excess* of 0°·2 is quite within the limit of error of observation.

the contour of the sea-bed may bring an admixture of frigid water nearer to the surface in particular localities than in the basin generally.

Diagram V.



115. The general results obtained by the correlation of all these data are represented diagrammatically in Diagram V., which, being constructed upon

the same scale as the two preceding figures of the same kind (pp. 457, 458) enables the relation between Depth and Temperature in the Atlantic Basin to be compared with the like relation in the Warm and Cold Areas respectively. For the sake of convenience, the Surface-temperature is here taken at 54°, this (as shown in Table III.) having been its average at those Stations in which the "superheating" did not conspicuously manifest itself.

116. When the rates of decrease of Temperature in successive strata of this deep Atlantic Basin are compared with those which have been shown to exist in the thinner strata of our comparatively shallow Cold area, a very remarkable relation presents itself, the Thermometric changes requiring in the former case a much greater Bathymetric descent than in the latter, but corresponding very closely with them when this allowance is made. This relation may be presented to the mind by ideally extending Diagram II. in a vertical direction, so that its horizontal lines should be separated by four times their interval. It has been shown (§ 98) that in the latter the stratum of about 100 fathoms which lies below the superficial 50 shows but a very slight decrease of temperature, presenting almost exactly the same rate of descent as the stratum between similar depths in the neighbouring Warm area. Now with this 100 fathoms' stratum, a stratum of about 500 fathoms beneath the superficial 100 in the deep Atlantic very closely corresponds, the reduction down to 500 fathoms being at an extremely slow rate. Between 150 and 300 fathoms in the Cold area, however, the rate of reduction becomes very much greater; and this is just what presents itself in the Atlantic Basin between 500 and 1000 fathoms; so that as in the Cold area we come down at very little below 300 fathoms upon a stratum of ice-cold water, so in the Atlantic basin we come down at 1000 fathoms upon a stratum averaging 38°·6. And further, as there is below this a slow progressive diminution of about 2° as we descend through the lower 300 fathoms of the Cold area, so a like progressive diminution is shown as we descend through the lower 1000 fathoms of the deep Atlantic Basin.

117. The significance of these facts becomes yet more apparent, when the varying rates of diminution of temperature in successive strata of the deep Atlantic Basin are reduced to a curve (Diagram VI.), in the same manner as the corresponding rates in successive strata of the Cold area; but with a reduction in the scale of depths in the former case, so as to make 500 fathoms in the deep basin correspond with 150 in the comparatively shallow channel. It is true that there is by no means the same *absolute* reduction in the one case as in the other; but this difference is just what would be anticipated on the hypothesis we have been advocating. For if it be supposed that the body of ice-cold water brought down from the Arctic basin by the various Polar currents is discharged into the wide and deep Atlantic Basin, it will tend to diffuse itself over its bottom, partly displacing and partly mingling with the water which previously occupied it, so as to form a stratum of considerable thickness, which, while much colder than the

Polar stream which seems to occupy all the deeper parts of the basin to within about 1000 fathoms of the surface, and thus carries back Polar water to the Equatorial area.

118. These inferences are fully borne out by the Temperature-soundings recently taken by Commander Chimmo, R.N., and Lieut. Johnson, R.N., at various points of the North-Atlantic Basin; for although the temperatures of these Soundings were recorded by unprotected Thermometers, yet the error to which the best of those instruments are subject from the effects of pressure at different depths can now be estimated, and the requisite correction applied to each observation, so as pretty certainly to give the true temperature in each case within a degree. These Soundings give a temperature of about 39° at 1000 fathoms, which is almost exactly accordant with the average of our own; but the "stratum of intermixture," indicated by the rapid reduction of temperature with increase of depth, seems to lie rather nearer the surface, the rapid reduction commencing at about 400 fathoms instead of at about 500. Below 1000 fathoms, at depths progressively increasing to 2270 fathoms, the temperatures are in extraordinarily close accordance with our own, the *minimum*, however, apparently falling a little lower. Thus at 2270 fathoms, the temperature recorded by an unprotected Casella thermometer was 44°; but the estimated correction for the instrument at that depth being 9°, the real temperature would be 35°.

119. It has thus been shown that the hypothesis advanced in our preceding Report, when worked out in connexion with the peculiar Geographical relations of the Arctic to the North-Atlantic basin, goes far to account for the two orders of phenomena which have now been examined, namely:—

(I.) The movement of a vast body of *warm* water, extending to a depth of several hundred fathoms, in a north-east direction, which moderates the cold of the Boreal area by bringing into it the warmth of that vast expanse of the North-Atlantic Ocean which is heated beneath the Tropical sun.

(II.) The existence of a flow of *ice-cold* water, at depths greater than 300 fathoms, in a south-west direction along the floor of the channel between the North of Scotland and the Faroe Islands, which contributes, with other frigid streams from the Arctic basin, to diffuse over the North-Atlantic sea-bed, at depths greater than 1000 fathoms, a Temperature below 39°, ranging downwards with increase of depth to about 35°.

And it further appears:—

(III.) That the "Gulf-stream" may be regarded as a kind of intensification of the ordinary flow of Surface-water from the Equatorial to the Polar area, this intensification being due to the peculiar local conditions which produce an extraordinary "superheating" of water in the Gulf of Mexico, and the diffusion of this superheated water thence over a vast proportion of the North-Atlantic area, raising its Surface-temperature by several degrees.

(IV.) That the Frigid stream which imparts to our Cold area, in the latitude of the Shetland Islands, a Bottom-temperature below 30°, may in like manner be considered as an intensification of the ordinary flow of deep water from the Polar to the Equatorial area, this intensification being due to the peculiar local conditions which limit the flow into the Atlantic basin of the water that has been cooled in the Polar basin, and thus keep it from intermixture with warmer water, whilst, by the narrowing of its channel, it is forced up nearer to the surface.

(V.) That as the temperature of the Gulf-stream is reduced, and the depth of its stratum diminished, the further it diffuses itself over the surface-water of the Atlantic, so the temperature of the Frigid Stream is raised by admixture with the warmer water through which it diffuses itself in the Atlantic basin, whilst it descends deeper and deeper beneath the surface with the increasing depth of the floor on which it rests.

120. It may be questioned, however, whether the low temperature thus shown to prevail, not only over the deepest portion of the North-Atlantic sea-bed, but throughout the enormous mass of water which lies below the "stratum of intermixture" (§ 117), is attributable solely, or even principally, to the cooling effect of the comparatively small quantity of frigid water discharged from the Arctic basin into this vast area, through the narrow channels previously indicated (§ 104). For it is to be remembered that the converse heating-action exerted by the solar rays over the southern portion is continually pumping up this cold water (so to speak) from the depths to the surface; and that this movement will be aided from below by the heat continually imparted from the solid ocean-bed to the colder water which rests upon it. Now as the most trustworthy observations on Deep-sea Temperatures under the Equator, though few in number *, indicate that even there a temperature not much above 32° prevails, it seems probable that part of the cooling effect is due to the extension of a flow of frigid water from the *Antarctic* area, even to the north of the Tropic of Cancer. It seems impossible to give any other explanation of the low temperatures observed in the 'Hydra' soundings across the Arabian Gulf †, since no frigid water from the Arctic basin could be supposed to find its way to that locality.

121. The unrestricted communication which exists between the *Antarctic* area and the great *Southern* Ocean-basins would involve, if the doctrine of a general Oceanic circulation be admitted, (1) a much more considerable interchange of waters between the Atlantic and the Equatorial areas than is possible in the Northern hemisphere; and (2) a reduction in the tem-

* See 'Lightning' Report, p. 186.

† 'Lightning' Report, p. 187, note:—The lowest Temperature *actually observed* in these Soundings, with Thermometers protected on Admiral Fitzroy's plan, was 36½°. The temperature of 33½° given in the 'Lightning' Report as existing below 1800 fathoms, proves to have been only an *estimate* formed by Captain Shortland, under the idea that the rate of reduction observed at smaller depths would continue uniform to the bottom, which the Serial soundings of the 'Porcupine' prove to be by no means the case.

perature of the deepest parts of the great Southern Ocean even below that of the North-Atlantic Sea-bed. Now so far as our present knowledge extends, both these inferences are in accordance with fact; for it is well known to Navigators that in all the Southern Oceans there is a perceptible "set" of warm surface-water towards the Antarctic Pole (this "set" being so decided in one part of the Southern Indian Ocean as to be compared by Capt. Maury to the Gulf-stream of the North Atlantic); and it is obvious that such a constant flow of surface-water cannot be maintained without an equivalent flow of deeper water in the opposite direction. Of the great depression of temperature which would be produced by such an unrestricted spread of frigid water over the deeper parts of the Southern Oceanic basins, indications are afforded by the deep Temperature-soundings taken in Sir James C. Ross's Antarctic Expedition, the Voyage of the 'Venus,' &c.; for when, as in several of these observations, the indicated Temperature was from 39° to 36° at depths greater than 1500 fathoms, the probable correction for pressure would reduce these to *actual* temperatures of from 32° to 29°, or even lower.

122. It would appear from the foregoing considerations that the Temperature of the Deep Ocean will everywhere depend upon the amount of Frigid water which can find its way from the Polar towards the Equatorial area; and that this will be mainly regulated by the Distribution of Land and Water, any considerable alteration in which may produce a wide-spread general change of submarine climate over vast areas, besides modifying, in the manner already pointed out (§ 105), the distribution of submarine climate over the parts of the Sea-bed traversed by special Polar or Equatorial Polar currents. And thus great additional force is added to the remark made in the 'Lightning' Report (p. 194) that a considerable modification of Submarine Climate might depend upon alterations in the contour of the land, or on the level of the sea-bottom, *at a great distance*. For when the South Polar basin was in great part shut in by the Antarctic Continent which (as appears from Dr. Hooker's Botanical researches) must have formerly united South America, New Zealand, and Australia, the Deep-sea temperature of the Southern Oceanic area generally must have been *higher* by some degrees than we have reason to believe it to be at present; whilst, on the other hand, if there ever was a time when the present North Pacific Area had a more free communication with the Arctic Basin than the present narrow and shallow channel of Behring's Straits affords, its Deep-sea temperature must have been *lower* by some degrees than it is likely to be found at present.

123. It is obvious that the distribution of Submarine Climate must exert a most important influence on the distribution of Animal Life; and of such influence the Deep-sea Dredgings carried on in this Expedition through a wide Geographical range have afforded most convincing evidence; as will be fully set forth in the Second Part of this Report. For many species of Mollusca, Crustacea, and Echinodermata previously supposed to

to be purely Arctic have been found to range southwards in deep water as far as those dredgings extended—namely, to the northern extremity of the Bay of Biscay; and the considerations already urged render it highly probable that an extension of the same mode of exploration would bring them up from the abysses of even Intertropical seas, over which a similar Climate prevails, and that an actual continuity may thus be found to exist between the Arctic and the Antarctic Faunæ. This idea was well put forth some years since by our excellent friend Prof. Lovén of Stockholm, in his discussion of the results of the deep-sea Dredgings executed by the Swedish Spitzbergen Expedition of 1861, under Torell. "Considering," he says, "the power of endurance in these lower marine animals, and recollecting the facts that properly Arctic species which live also on the coast of Europe, are generally found there at greater depths than in their proper home, and that certain Antarctic species very closely agree with Arctic species, the idea occurs that, while in our own seas and those of warm climates, the surface, the coast-line, and the lesser depths are peopled with a rich and varied Fauna, there exists in the great Atlantic depression, perhaps in all the abysses of our globe, and continued from Pole to Pole, a Fauna of the same general character, thriving under severe conditions, and approaching the surface where none but such exist, in the coldest seas." It had, moreover, been long previously suggested by Sir James C. Ross, on the basis of observations made during his Antarctic voyage; for these observations had led him to believe that water of similar temperature to that of the Arctic and Antarctic seas exists in the depths of the Equatorial Ocean, and that Arctic species may thus find their way to the Antarctic area, and *vice versâ*.—The "similar temperature" believed by Sir James Ross to have had this general prevalence seems to have been 39°; whereas our observations distinctly prove that a temperature even below 30° may be conveyed by Polar streams far into the Temperate zone, and that the general temperature of the deepest part of the North-Atlantic sea-bed has more of a Polar character than he supposed. Further, as there must have been deep seas at all Geological epochs, and as the Physical forces which maintain the Oceanic circulation must have been in operation throughout, though modified in their local action by the particular distribution of land and water at each period, it is obvious that the presence of Arctic types of animal life in any Marine formation can no longer be accepted as furnishing evidence *per se* of the general extension of Glacial action into Temperate or Tropical regions.

124. Whilst the question of Deep-Sea Temperature is one of the greatest Biological interest, its determination is of even greater importance to the Geologist, as affecting his interpretation of the phenomena on which his belief in a former general prevalence of a Glacial climate is founded. For if a Glacial temperature should be found now to prevail, and types of Animal life conformable thereto should prove to be diffused, over the deeper portion of the existing sea-bed in all parts of the Globe, it is obvious that the same

may have been the case at *any* Geological epoch; for there must have been deep seas in all periods, and the Physical forces which maintain the Oceanic circulation at the present time must have been *always* in operation, though modified in their local action by the distribution of land and water existing at any particular date. And as the elevation of the present deep sea-bed of even the intertropical Oceanic area would (if we have correctly interpreted the results of our own and others' observations) offer to the study of the Geologist of the future a deposit characterized by the presence of Polar types, so must the Geologist of the present hesitate in regarding the occurrence of Boreal types in any marine deposit as adequate evidence *per se* of the general extension of Glacial action into Temperate or Tropical regions. At any rate, it may be considered as having been now placed beyond reasonable doubt that *a Glacial Submarine Climate may prevail over any Area, without having any relation whatever to the Terrestrial Climate of that Area* *.

125. *Composition of Sea-Water.*—A considerable number of samples of Sea-water were collected in different localities and at different depths, for the purpose of being submitted, on our return home, to the complete analysis which Dr. Frankland had been kind enough to undertake. As the quantities collected in the first two cruises, however, proved insufficient for his special purpose—the determination of the quantity of Organic matter,—a set of Winchester quart bottles was taken out in the Third cruise; and these were filled from *surface-* and from *bottom-*waters in four localities, two in the Warm and two in the Cold area. The important results of Dr. Frankland's analyses of these samples are given in Appendix II. The differences in *Specific Gravity*, and in the proportion of the ordinary *Saline* constituents (as indicated by that of the chlorine) are scarcely as great as might have been anticipated; but in so far as they extend, they are generally conformable to the doctrine of Forchammer, that Polar water is more dilute than Equatorial. In particular it may be noted that the lowest Specific Gravity (1·0262), which coincided with a still lower proportion in the total of Solid matter, presented itself in the waters taken from the Arctic stream nearest its presumed source (§ 70). But the most novel and important feature in these analyses is the large quantity of *Organic Matter* indicated by them as universally present in the water of the open Ocean at great distances from land and at all depths. This has a direct bearing on a question of the greatest Biological interest,—What is the *source of Nutriment* for the vast mass of Animal life covering the abyssal Sea-bed?

126. That Animals have no power of themselves generating the Organic Compounds which serve as the materials of their bodies—and that the production of these materials from the carbonic acid, water, and ammonia

* Since the above was written, we have learned from Prof. Liveing, of Cambridge, that Mr. Lucas Barrett, formerly Assistant to Prof. Sedgwick, ascertained, not long before his lamented death, the existence of a temperature not far above the freezing-point in the deepest part of the sea near Jamaica.

f the Inorganic world, under the influence of Light, is the special attribute f Vegetation—is a doctrine so generally accepted, that to call it in ques- ion would be esteemed a Physiological heresy. There is no difficulty in ccounting for the alimentation of the higher Animal types, with such an nlimited supply of food as is afforded by the *Globigerinæ* and the *Sponges* n the midst of which they live, and on which many of them are known to ed. Given the *Protozoa*, everything else is explicable. But the ques- ion returns,—On what do these *Protozoa* live?

127. The hypothesis has been advanced that the food of the abyssal *rotozoa* is derived from *Diatoms* and other forms of minute Plants, which, rdinarily living at or near the surface, may, by subsiding to the depths, arry down to the animals of the sea-bed the supplies they require. Our xamination of the surface-waters, however, has afforded no evidence of the xistence of such Microphytic vegetation in quantity at all sufficient to upply the vast demand; and the most careful search in the *Globigerina*- mud has failed to bring to light more than a very small number of speci- nens of these Siliceous envelopes of Diatoms, which would most assuredly ave revealed themselves in abundance had these Protophytes served as a rincipal component of the food of the *Protozoa* that have their dwelling- lace on the sea-bed.—Another hypothesis has been suggested, that these *rotozoa*, which are so near the border of the Vegetable kingdom, may be ble, like Plants, to generate Organic Compounds for themselves,—manufac- uring their own food, so to speak, from Inorganic materials. But it is carcely conceivable that they could do this without the agency of Light; nd, as it is obviously the want of that agency which excludes the possibi- ity of Vegetation in the abysses of the ocean, the same deficiency would revent Animals from carrying on the like process.

128. A possible solution of this difficulty, first offered by Professor Wyville homson in a Lecture delivered in the spring of 1869, has received so remark- ble a confirmation from the researches made in the 'Porcupine' expedi- ion, that it may now be put forth with considerable confidence. It is, he re- narked, the distinctive character of the *Protozoa*, that "they have no pecial organs of nutrition, but that they absorb water through the whole urface of their jelly-like bodies. Most of these animals secrete exquisitely ormed skeletons, sometimes of Lime, sometimes of Silica. There is no loubt that they extract both of these substances from the Sea-water, lthough Silica often exists there in quantity so small as to elude detec- ion by chemical tests. All Sea-water contains a certain amount of Organic natter in solution. Its sources are obvious. All rivers contain a large uantity; every shore is surrounded by a fringe, which averages about a nile in width, of olive and red Seaweeds; in the middle of the Atlantic here is a marine meadow, the Sargasso Sea, extending over 3,000,000 f square miles; the sea is full of Animals which are constantly dying and ecaying; and the water of the Gulf-stream especially courses round coasts here the supply of organic matter is enormous. It is, therefore, quite

intelligible that a world of animals should live in these dark abysses: but it is a necessary condition that they should chiefly belong to a class capable of being supported by absorption through the surface of matter in solution, developing but little heat, and incurring a very small amount of waste by any manifestation of vital activity. According to this view, it seems highly probable that at all periods of the earth's history some form of the Protozoa (Rhizopods, Sponges, or both) predominated over all other forms of animal life in the depths of the Sea, whether spreading, compact, and reef-like, as in the Laurentian and Palæozoic *Eozoon*, or in the form of myriads of separate organisms, as in the *Globigerinæ* and *Ventriculites* of the Chalk "*.

129. During each Cruise of the 'Porcupine,' samples of Sea-water obtained from various depths, as well as from the surface, at stations far removed from land, were submitted to the Permanganate test after the method of Prof. W. A. Miller, with an addition suggested by Dr. Angus Smith for the purpose of distinguishing the Organic matter *in a state of decomposition* from that which is only *decomposeable*; with the result of showing the uniform presence of an appreciable quantity of matter of the latter kind, which, not having passed into a state of decomposition, may be assimilable as food by animals,—being, in fact, Protoplasm in a state of extreme dilution. —Until, therefore, any other more probable hypothesis shall have been proposed, the sustenance of Animal life on the ocean-bottom at any depth may be fairly accounted for on the supposition of Prof. Wyville Thomson, that the Protozoic portion of that Fauna is nourished by direct absorption from the dilute Protoplasm diffused through the whole mass of Oceanic waters, just as it draws from the same mass the Mineral ingredients of the skeletons it forms. This diffused Protoplasm, however, must be continually undergoing decomposition, and must be as continually renewed; and the source of that renewal must lie in the *surface-life* of Plants and Animals, by which (as pointed out by Prof. Wyville Thomson) fresh supplies of Organic matter must be continually imparted to the Oceanic waters, being carried down even to their greatest depths by that *liquid diffusion* which was so admirably investigated by the late Professor Graham.

130. The analysis of the *Gases* contained in Sea-water, collected not only at the surface but from various depths beneath it, was systematically carried on during the whole of the Expedition. The results cannot be considered as entirely satisfactory; since it is by no means certain that the relative proportions of the gases obtained by boiling water taken up from great depths may not have been affected by the liberation of a portion of these gases when the superincumbent pressure was removed. But they will be found extremely suggestive, and seem to have a tolerably definite relation to the *Respiration* of the Abyssal Fauna. Referring to Appendix I. for a fuller statement of details, we may here call attention to their general

* "The Depths of the Sea," a Lecture delivered in the theatre of the Royal Dublin Society, April 10, 1869.

bearing.—The general average of thirty analyses of *surface*-water gives the following as the percentage proportions:—25·1 Oxygen, 54·2 Nitrogen, 20·7 Carbonic Acid. This proportion, however, was subject to great variations, as will be presently shown. As a general rule, the proportion of Oxygen was found to diminish, and that of Carbonic Acid to increase, with the depth, the results of analyses of *intermediate* waters giving a percentage of 22·0 Oxygen, 52·8 Nitrogen, and 26·2 Carbonic Acid; whilst the results of analyses of *bottom*-waters gave 19·5 Oxygen, 52·6 Nitrogen, and 27·9 Carbonic Acid. But *bottom*-water at a comparatively small depth often contained as much Carbonic Acid and as little Oxygen as *intermediate* water at much greater depths; and the proportion of Carbonic Acid to Oxygen in *bottom*-water was found to bear a much closer relation to the abundance of Animal life (especially of the more elevated types), as shown by the Dredge, than to its depth. This was very strikingly shown in an instance in which analyses were made of the gases contained in samples of water collected at every 50 fathoms, from 400 fathoms to the bottom at 862 fathoms, the percentage results being as follows:—

	750 fath.	800 fath.	Bottom, 862 fath.
Oxygen	18·8	17·8	17·2
Nitrogen........	49·3	48·5	34·5
Carbonic Acid....	31·9	33·7	48·3

The extraordinarily augmented percentage of Carbonic Acid in the stratum of water here immediately overlying the Sea-bed was accompanied by a great abundance of Animal life. On the other hand, the lowest percentage of Carbonic Acid found in bottom-water (viz. 7·9) was accompanied by a "very bad haul." In several cases in which the depths were nearly the same, the analyst ventured a prediction as to the abundance, or otherwise, of Animal life, from the proportion of Carbonic Acid in the bottom-water; and his prediction proved in every instance correct.

131. It would appear probable, therefore, that the increase in the proportion of Carbonic Acid, and the diminution in that of the Oxygen, in the abyssal waters of the Ocean, is due to the Respiratory process; which is no less a necessary condition of the existence of Animal life on the sea-bed than is the presence of food-material for its sustenance. And it is further obvious that the continued consumption of Oxygen and liberation of Carbonic Acid would soon render the stratum of water immediately above the bottom completely irrespirable (in the absence of any antagonistic process of Vegetation) were it not for the upward diffusion of the Carbonic Acid through the intermediate waters to the surface, and the *downward* diffusion of Oxygen *from* the surface to the depths below. A continual interchange will take place *at* the surface between the gases of the Sea-water and those of the Atmosphere; and thus the Respiration of the Abyssal Fauna is provided for by a process of diffusion, which may have to operate through *three miles* or more of intervening water.

132. The varying proportions of Carbonic Acid and Oxygen in the *surface*-waters are doubtless to be accounted for in part by the differences in the amount and character of the Animal life existing beneath; but a comparison of the results of the analyses made during the agitation of the surface by wind, with those made in calm weather, showed so decided a reduction in the proportion of Carbonic Acid, with an increase in that of Oxygen, under the former condition, as almost unequivocally to indicate that superficial disturbance of the sea by Atmospheric movement is absolutely necessary for its purification from the noxious effects of Animal decomposition. Of this view a most unexpected and remarkable confirmation has been afforded by the following circumstance:—In one of the analyses of Surface-water made during the Second cruise, the percentage of Carbonic Acid fell as low as 3·3, while that of Oxygen rose as high as 37·1; and in a like analysis made during the Third cruise, the percentage of Carbonic Acid was 5·6, while that of Oxygen was 45·3. As the results of every other analysis of Surface-water were in marked contrast to these, it became a question whether they should not be thrown out as erroneous; until it was recollected that whilst the samples of surface-water had been generally taken up from the *bow* of the vessel, they had been drawn, in these two instances, from *abaft the paddles*, and had thus been subjected to such a violent agitation in contact with the Atmosphere as would preeminently favour their thorough aëration.—Hence, then, it may be affirmed that every disturbance of the Ocean-surface by Atmospheric movement, from the gentlest ripple to the most tremendous storm-wave, contributes, in proportion to its amount, to the maintenance of Animal life in its Abyssal depths; doing, in fact, for the aëration of the fluids of their inhabitants just what is done by the heaving and falling of the walls of our own chest for the aëration of the blood which courses through our lungs. A perpetual calm would be as fatal to their continued existence as the forcible stoppage of all Respiratory movement would be to our own; and thus universal stagnation would become universal death.

APPENDIX.

I.—*Summary of the Results of the Examination of Samples of Sea-water taken at the Surface and at Various Depths.* By Wm. Lant Carpenter, B.A., B.Sc.

Surface-waters.—Care was taken to obtain these samples as pure as possible, and free from any contamination caused by matters derived from the vessel, by dipping them up in clean vessels at a few inches below the surface at or near the bow of the ship. In two instances, however, the samples were taken from abaft the paddles.

Waters taken at depths below the surface.—It was found desirable to coat the brass Water-Bottles (§ 19) internally with sealing-wax varnish, owing to the corrosive action of the sea-water. The apparatus was then found to work perfectly satisfactorily in all cases in which there was sufficient weight on the Sounding-line to which they were attached to keep the bottles perpendicular, or nearly so. When, from the smallness of the attached weight, or the roughness of the sea, the sounding-line was at an acute angle with the general level of the sea-surface while it was being drawn up, the results of the examination of water thus obtained rendered it highly probable that some water at or near the surface had found its way into the bottle, and that its contents were not to be relied on as coming from the lowest depths.

When Bottom-water was obtained from depths beyond 500 fathoms, it was almost invariably charged with a quantity of very fine mud in suspension, rendering it quite turbid. Many hours' standing was necessary for the deposit of this; but it was readily removed by filtration. In no instance was there any evidence of water from great depths being much more highly charged with dissolved gases than Surface-waters; a considerable elevation of temperature being in *all* cases necessary for the evolution of any dissolved gas.

Mode of Examining Samples.—The samples of water thus taken were examined with as little delay as possible, with a view to determine:—

(1) The Specific Gravity of the water.

(2) The total quantity of dissolved Gases contained in them, and the relative proportions of Oxygen, Nitrogen, and Carbonic Acid.

(3) The quantity of Oxygen necessary to oxidize the Organic matter contained in the water; distinguishing between

a, the decomposed organic matter, and

b, the easily decomposable organic matter.

(1) The Specific-Gravity determinations were made at a temperature as near 60° Fahr. as possible, with delicate glass Hydrometers, so graduated that the Specific gravity could be read off directly to the fourth decimal place with ease.

(2) The apparatus for the analysis of the Gases dissolved in the sea-water was essentially that described by Prof. Miller in the second volume

of his 'Elements of Chemistry.' It was found necessary to make several modifications in it, to adapt it to the motion of the vessel. These consisted chiefly in suspending much of it from the cabin-ceiling, instead of supporting it from beneath, and in rendering all the parts less rigid by a free use of caoutchouc tubing, &c., the utmost care being taken to keep all joints tight.

It was found possible to make correct analyses, even when the vessel was rolling sufficiently to upset chairs and cabin-furniture.

The method of Analysis may be thus summarized:—From 700 to 800 cubic centimetres of the sample to be examined were boiled for about 30 minutes, in such a way that the steam and mixed gases evolved were collected over mercury in a small graduated Bunsen's Gas-holder, all access of air being carefully guarded against. The mixed gases were then transferred to two graduated tubes in a mercurial trough, where the Carbonic Acid was first absorbed by a strong solution of caustic potash; and subsequently the Oxygen was absorbed by the addition of pyrogallic acid, the remaining gas being assumed to be Nitrogen.

The results of the analyses were always corrected to the standard Temperature of 0° Cent., and to 760 millimetres Barometric pressure, for comparison among themselves and with others. In nearly every case the duplicate analyses from the same gaseous mixture agreed closely, if they were not identical.

(3) The examination of the Sea-water for Organic matter was made according to the method detailed by Prof. Miller in the Journal of the Chemical Society for May 1865, with an addition suggested by Dr. Angus Smith. Each sample of water was divided into two; to one of these a little free acid was added, and to both an excess of a standard solution of Permanganate of potash. At the end of three hours the reaction was stopped by the addition of Iodide of potassium and Starch, and the excess of Permanganate estimated by a standard solution of Hyposulphite of soda. The portion to which free acid was added gave the Oxygen required to oxidize the decomposed and easily decomposable organic matter; the second portion gave the oxygen required by the decomposed organic matter alone, which was usually from about one-half to one-third of the whole.

The following is a Summary of the total number of observations, analyses, &c. made during the Three Cruises respectively:—

	First Cruise.	Second Cruise.	Third Cruise.	Total.
Specific-Gravity determinations	72	27	26	125
Duplicate Gas-analyses	45	23	21	89
Organic-matter tests	137	26	32	195

Specific Gravity.—The Specific Gravity of Surface-water was found to diminish slightly as land was approached; but the average of 32 observations upon water at a sufficient distance from land to be unaffected by local disturbances was 1·02779, the maximum being 1·0284
and the minimum.... 1·0270.

It was almost always noticed that, during a high wind, the specific gravity of surface-water was *above* the average.

The average of 30 observations upon the Specific Gravity of Intermediate water was 1·0275,
the maximum being.......... 1·0281,
and the minimum .... 1·0272.

The Specific Gravity of Bottom-waters at depths varying from 77 to 2090 fathoms, deduced from an average of 43 observations, was
1·0277,
the maximum being.......... 1·0283,
and the minimum............ 1·0267.

It will be noticed that the average Specific Gravity of Bottom-water is slightly less than that of Surface-water. In several instances the Specific Gravities of Surface- and of Bottom-waters taken at the same place having been compared, that of the Bottom-water was found to be appreciably less than that of the Surface-water. Thus

At 1425 fathoms depth (Station 17) it was .. 1·0269
Surface at the same .... 1·0280

And

At 664 fathoms depth (Station 26 *b*) it was .. 1·0272
Surface at the same..................... 1·0280

According, however, to a Series of observations made at the same spot (Station 42) at intervals of 50 fathoms, from 50 to 800, the Specific Gravity increased with the depth from 1·0272 at 50 fathoms to 1·0277 at 800 fathoms*.

Several series of Sp.-Gr. observations were made near the mouths of rivers and streams; showing the gradual mixture of fresh and salt water, and the floating of lighter portions above the denser sea-water, as well as the reverse effect produced by the influence of tidal currents. Thus outside Belfast Lough a rapid stream of water of Sp. Gr. 1·0270 was found above water which at a depth of 73 fathoms had a Sp. Gr. of 1·0265.

Gases of Sea-water.—The analyses of the Gaseous constituents of sea-water may be divided into two groups: (1) Analyses of Surface-waters. (2) Analyses of waters below the surface; and these last may be again subdivided into (*a*) Intermediate, and (*b*) Bottom-waters.

The total quantity of dissolved gases in sea-water, whether at the surface or below it, was found to average about 2·8 volumes in 100 volumes of water.

* My own experience of the difficulty of making accurate Hydrometric determinations when the ship was rolling prevents me from attaching much value to the above results.—W. B. C.

The average of 30 analyses of Surface-waters made during the Expedition gave the following proportions:—

	Percentage.	Proportion.
Oxygen	25·046	100
Nitrogen	54·211	216
Carbonic acid	20·743	80
	100·000	

These were thus distributed over the three Cruises, and the maxima and minima of each constituent are thus shown.

	Number of analyses	Average percentage.			Average proportion.			Oxygen.		Nitrogen.		Carbonic acid	
		Oxygen.	Nitrogen.	Carbonic acid.	O.	N.	CO_2	Max. per cent.	Min. per cent.	Max. per cent.	Min. per cent.	Max. per cent.	Min. per cent.
First Cruise	19	24·47	52·95	22·58	100	216	92	28·78	19·60	62·95	46·35	32·0	12·72
Second Cruise	2	31·33	54·85	13·82	100	175	44	37·10	25·56	59·63	50·07	24·37	3·27
Third Cruise	9	24·86	56·73	18·41	100	228	74	45·28	13·98	68·67	41·42	27·14	5·64

It is interesting to remark that Surface-water contains a greater quantity of Oxygen and a less quantity of Carbonic acid during the prevalence of strong wind. The following is an average of 5 analyses made under such conditions:—

		Per cent.	Proportion.	General average.	
5	Oxygen.........	29·10	100	25·046	100
	Nitrogen	52·87	182	54·211	216
	Carbonic acid ..	18·03	62	20·743	83

In the two cases which presented the remarkable small *minima* of Carbonic acid with a great excess of Oxygen, the water had been accidentally taken from immediately abaft the paddles, where it had been subject to violent agitation in contact with air.

Of water at various depths beneath the surface, 59 analyses were made. Those in the First cruise, 26 in number, were chiefly from Bottom-water at depths from 25 to 1476 fathoms. In the Second cruise the 21 analyses chiefly belonged to two Series,—the first of samples taken at intervals of 250 fathoms, from 2090 to 250 fathoms, inclusive; and the second of samples taken at intervals of 50 fathoms from 862 to 400 fathoms inclusive. In the Third cruise 12 analyses were made,—8 of Bottom-water, of which one-half were in the "cold area," and 4 at Intermediate depths.

The general average of the 59 analyses of water taken below the surface gives:—

Oxygen	20·568	100
Nitrogen	52·240	254
Carbonic acid	27·192	132
	100·000	

It will be seen from this that while the quantity of Nitrogen is only 1·97 per cent. less than in surface-water, the quantity of Oxygen is diminished by 4·48 per cent., and the quantity of Carbonic acid increased by 6·45 per cent. This difference is greater if Bottom-waters only are compared with Surface-waters.

	30 Surface.		24 Intermediate.		35 Bottom.	
	Per cent.	Proportion.	Per cent.	Proportion.	Per cent.	Proportion.
Oxygen	25·05	100	22·03	100	19·53	100
Nitrogen	54·21	216	51·82	235	52·60	261
Carbonic acid	20·74	83	26·15	119	27·87	143
	100·00		100·00		100·00	

The two Series of analyses, before referred to, performed during the Second cruise upon Intermediate waters at successive depths over the same spot both show a regular increase of the Carbonic acid, and diminution of the Oxygen, as the depth increases, the percentage of Nitrogen varying but slightly.

These general results appear to show that the Oxygen diminishes and the Carbonic acid increases with the depth until the bottom is reached; but that *at* the bottom, whatever the depth from the surface, the proportions of Carbonic acid and of Oxygen do not conform to this law, Bottom-water at a comparatively small depth often containing as much carbonic acid and as little oxygen as Intermediate water at a greater depth. No instance occurred during the first two Cruises in which (where samples of surface and intermediate or bottom-waters were taken at the same place) the quantity of Carbonic acid was less and of Oxygen greater than at the surface; the only exception occurred in the Third cruise, at a place where, it is believed, currents of water were meeting.

It was frequently noticed that a large percentage of Carbonic acid in Bottom-water was accompanied by an abundance of Animal life, as shown by the dredge; and that where the dredge-results were barren, the quantity of Carbonic acid was much smaller. The greatest percentage of Carbonic acid ever found was accompanied by an abundance of life; while at a short distance (62 fathoms) above the bottom, the proportion of Carbonic acid was conformable to the law of variation with depth before referred to:

	Bottom, 862 fms.	800 fms.	750 fms.
Oxygen..........	17·22	17·79	18·76
Nitrogen	34·50	48·46	49·32
Carbonic acid	48·28	33·75	31·92
	100·00	100·00	100·00

The lowest percentage of Carbonic acid (7·93) ever found in Bottom-water, occurring at a depth of 362 fathoms, was accompanied by a "very bad haul."

In crossing the wide channel from the N.W. of Ireland towards Rockall, where the water for some distance is over 1000 fathoms depth, so that the other circumstances varied very little, if at all, the proportion of Carbonic acid appeared to vary with the dredge-results; so that the analyst ventured to predict whether the collection would be good or not before the dredge came to the surface—drawing his inference from the results of his analyses of the gases of the Bottom-water. In each case his prediction was justified by the result.

	Station 17. 1425 fms.	Station 19. 1360 fms.	Station 20. 1443 fms.	Station 21. 1476 fms.
Oxygen	16·14	17·92	21·34	16·68
Nitrogen	48·78	45·88	47·51	43·46
Carbonic acid ..	35·07	36·20	31·15	39·86
	100·00	100·00	100·00	100·00
	Good haul.	Good haul.	Bad haul.	Good haul.

In the analyses made of the water in the Cold Area, and generally in the Third cruise, there appears, as might be expected from the various currents &c., a greater variation in the results than in the other series. In the Bottom and Intermediate waters the Nitrogen appears to be rather in excess of the average, and the Carbonic acid has a large range of variation—from 7·58 per cent. at Station 47 (540 fathoms, Temp. 43°·8) to 45·79 per cent. at Station 52 (384 fathoms, 30°·6 Fahr.). The average of the Surface-waters is much the same as in the other parts of the cruise.

It may be worth notice that in localities where the greatest depth did not exceed 150 fathoms, the results of the gas-analysis of Bottom and Surface-water were frequently so nearly the same, whatever the amount of Animal life on the bottom, as to lead to the supposition that there might be at that limit a sufficient circulation, either of the particles of the water itself or of the gases dissolved in it, to keep the gaseous constitution alike throughout. The coincidence of this depth with the extreme depth at which Fish are usually found to exist in these seas is suggestive.

Organic matter.—With a view to test the method of analysis by Permanganate of potash, two or three series of analyses were made where fresh and salt water mixed together, as in Killibegs Harbour, Donegal Bay, &c.; and the results in all cases justified the expectation formed, that the amount of permanganate was an index of the comparative purity of the water, both as regards the "decomposed" and the "decomposable" organic matter.

Disregarding the above series, a total of 134 experiments were made upon Sea-water, which may be thus divided:—

56	upon	Surface-water,
18	,,	Intermediate water,
60	,,	Bottom-water,
134		

during the First and Third cruises.

The results are given in the quantity of Oxygen in fractions of a gramme required to oxidize the Organic matter in a litre of water.

Average of 56 analyses of Surface-water :—

No.			
28.	Decomposed......	0·00025	Total 0·00095.
28.	Decomposable....	0·00070	

	Maximum.	Minimum.	
Decomposed......	0·00094	0·00000	4 cases.
Decomposable....	0·00100	0·00000	1 case.
Total	0·00194	0·00000	1 case.

Average of 18 analyses of Intermediate water :—

No.			
9.	Decomposed	0·00005	Total 0·00039.
9.	Decomposable	0·00034	

In 7 out of 9 there was no "decomposed" Organic matter; and in 3 out of 9 there was no Organic matter at all, as indicated by this test.

In this series the analyses of the observations made during the Second cruise are not included, as the calculations have been differently made.

Average of 60 analyses of Bottom-water :—

No.			
26.	Decomposed......	0·00047	Total 0·00088.
34.	Decomposable....	0·00041	

	Maximum.	Minimum.	
Decomposed......	0·00105	0·00000	2 cases.
Decomposable	0·00148	0·00000	1 case.
Total	0·00253	0·00000	1 case.

These figures appear to show (1) that Intermediate waters are more free from Organic contamination than either Surface- or Bottom-waters, as might be expected from the comparative absence of animal life in these waters; (2) that the total absence of Organic matter is least frequent in Bottom-waters, and most frequent in Intermediate waters, Surface-waters occupying a middle place in this respect; and (3) that there is not much difference between Bottom- and Surface-waters, either in the total quantity of Organic contamination or in the relative proportions of the "decomposed" and "easily decomposable" organic matter.

It may be worth notice that when the Bottom-water from great depths was muddy, tests made before and after filtration showed that some of the Organic matter was removed by this operation.

II.—*Results of the Analysis of Eight Samples of Sea-Water collected during the Third Cruise of the 'Porcupine.'* By Dr. Frankland, F.R.S.

Royal College of Chemistry.
November 15th, 1869.

Dear Dr. Carpenter,—Herewith I enclose results of analyses of the samples of sea-water collected during your recent cruise in the 'Porcupine.'

I shall not attempt to draw any general conclusions from these results; your own intimate knowledge of the circumstances under which the different samples were collected will enable you to do this much better than I.

There is, however, one point which is highly remarkable and to which I would draw your attention; it is the large amount of very highly nitrogenized Organic matter contained in most of the samples, as shown by the determinations of organic Carbon and organic Nitrogen, and the proportion of organic Carbon to organic Nitrogen. For the purposes of comparison, I have appended the results of analyses of Thames-water and of the water of Loch Katrine, the former representing probably about a fair average of the proportion of organic nitrogen reaching the sea in the rivers of this country, but being presumably considerably greater than that contributed by rivers in other parts of the world. If this be so, it follows either that soluble nitrogenous organic matter is being generated from inorganic materials in the sea, or that this matter is undergoing concentration by the evaporation of the ocean,—the rivers and streams continually furnishing additional quantities whilst the water evaporated takes none away.

The amounts of Carbonate of Lime given in the Table are obtained by adding the number 3 (representing the solubility of carbonate of lime in pure water) to the temporary hardness which denotes the carbonate of lime thrown down on boiling. As the determination of temporary hardness in water containing so much saline matter is not very accurate, the numbers in the columns headed "temporary hardness" and "carbonate of lime" must only be regarded as rough approximations to the truth; moreover, a small proportion of carbonate of magnesia is mixed with the carbonate of lime and estimated with it.

In all their peculiar features these analytical results agree with those which I have previously obtained from numerous samples of sea-water collected by myself off Worthing and Hastings.

Yours very truly,
E. Frankland.

Results of Analysis expressed in Grammes per 100,000 Cubic Centimetres of Water.

Number of Sample.	Description.	Sp. Gravity at 15°·5 C.	Total Solid matter in solution.	Organic Carbon.	Organic Nitrogen.	Proportion of organic N. to organic C.	Ammonia.	Nitrogen as Nitrates and Nitrites.	Total combined Nitrogen.	Silica.	Chlorine.	Hardness. Temporary.	Hardness. Permanent.	Hardness. Total.	Carbonate of Lime, Approximate.
No. 47	Surface, Temp. 54°	1·0268	4074	·647	·134	1 : 4·83	·022	·030	·182	·90	2028·1	70·7	818·3	889·0	73·7
47	Bottom, 542 fathoms, Temp. 43°·8	1·0268	4070	·331	·163	1 : 2·03	·022	·032	·213	—	2034·4	42·4	832·4	874·8	45·4
87	Surface, Temp. 52°·6	1·0269	4036	·321	·098	1 : 3·38	·017	·056	·168	2·10	1987·5	98·9	804·2	903·1	101·9
87	Bottom, 767 fathoms, Temp. 41°·4	1·0268	4132	·313	·096	1 : 3·26	·020	·061	·173	1·10	2026·9	84·8	818·3	903·1	87·8
54	Surface, Temp. 52°·5	1·0266	4110	·281	·169	1 : 1·61	·007	·025	·200	·75	2017·5	42·4	818·3	860·7	45·4
54	Bottom, 363 fathoms, Temp. 31°·4	1·0258	4030	·136	·161	1 : ·84	·004	·041	·205	·10	2014·4	56·5	860·7	917·2	59·5
64	Surface Temp. 49°·7	1·0265	4116	·170	·217	1 : ·78	·005	·043	·264	·30	1996·2	56·5	860·7	917·2	59·5
64	Bottom, 640 fathoms, Temp. 29°·6	1·0262	3920	·217	·252	1 : ·86	·008	·039	·298	·10	1988·1	56·5	846·6	903·1	59·5
	Thames, mid-stream at low water, London Bridge, April 27th, 1869.	...	30·35	·455	·075	1 : 6·07	·032	·181	·282	...	1·95	...	...	22·7	
	Water of Loch Katrine	...	3·00	·161	·011	1 : 14·64	·001	·000	·012	...	·905	...	...	·3	

III.—*Notes on Specimens of the Bottom collected during the First Cruise of the 'Porcupine' in* 1869. By David Forbes, F.R.S.

Atlantic Mud contained in a small bottle marked "Soundings No. 20, 1443 fathoms."

A complete analysis of this sample shows its Chemical Composition to be as follows:—

Carbonate of lime	50·12
Alumina * ("soluble in acids")	1·33
Sesquioxide of iron ("soluble in acids")	2·17
Silica (in a soluble condition)	5·04
Fine insoluble gritty sand (rock débris)	26·77
Water	2·90
Organic matter	4·19
Chloride of sodium and other soluble salts	7·48
	100·00

If we compare the chemical composition as above with that of ordinary Chalk, which consists all but entirely of carbonate of lime, and seldom contains more than from 2 to 4 per cent. of foreign matter (clay, silica, &c.), it will be seen that it differs chiefly in containing so very large an amount of rock-matter in a fine state of division. If we subtract the water, organic matter, and marine salts, which would probably in greatest part be removed before such mud could in process of ages be converted into solid rock, even then the amount of carbonate of lime or pure chalk would not be more than at highest some 60 per cent. of the mass.

As such deposits must naturally be expected to vary greatly in mechanical character and chemical composition, it would be premature to generalize as to the actual nature of the deposits now in course of formation in the depths of the Atlantic, before a careful examination had been made of a series of such specimens from different localities. The soluble silica is principally from siliceous organisms.

[Mr. Hunter's analysis of the Atlantic Mud brought up from the 2435 fathoms' dredging, will be found in p. 428].

As regards the probable origin of the pebbles and gravel found in the various dredgings, it will be at once seen, from the description, that they consist principally of fragments of volcanic rocks and crystalline schists. The former of these have in all probability come from Iceland or Jan Mayen; whilst the latter, associated as they are with small fragments of grey and somewhat altered calcareous rock, would appear to have proceeded from the north-west coast of Ireland, where the rocks are quite identical in mineral character. The north of Scotland and its islands also contain similar rocks; but without being at all positive on this head, I am

* With phosphoric acid.

rather inclined to the opinion that they have been derived from Ireland, and not necessarily connected with any glacial phenomena, believing that their presence may be accounted for by the ordinary action of marine currents.

"Pebbles from 1215 fathoms (Station 28)."

The stones were all subangular, the edges being all more or less worn or altogether rounded off. The specimens were 38 in number, and upon examination were found to consist of:—

5 Hornblende schist; the largest of these (which also was the largest in size of the entire series) weighed 421 gr. ($\frac{7}{8}$ of an ounce), was extremely compact, and was composed of black hornblende, dirty-coloured quartz, and some garnet.

2 Mica schist; quartz with mica, the largest weighing 20 grains.

5 Grey pretty compact limestone, the largest being 7 grains in weight.

2 Fragments (showing the cleavage faces rounded off on edges) of orthoclase (potash felspar), evidently derived from granite; the largest of the two fragments weighed 15 grains.

5 Quartz, milky in colour or colourless; the largest of these weighed $90\frac{3}{4}$ grains, and showed evidence of having been derived from the quartz-veins so common in clay-slate.

19 Fragments of true volcanic lava, most of which were very light and scoriaceous (vesicular), although some small ones were compact and crystalline; and in these the minerals augite, olivine, and glassy felspar (Sanadine) could be distinctly recognized. Amongst these were fragments of trachytic trachydoleritic, and pyroxenic (basaltic) lavas, quite similar to those of Iceland or Jan Mayen of the present period, from which they had probably been derived.

—

38

"Gravel from 1443 fathoms (Station 20)."

This sample of gravel consisted of 718 subangular fragments, in general not above from $\frac{1}{4}$ to $\frac{1}{2}$ grain in weight, with occasionally some of a little greater size; but the most considerable of all (a fragment of mica schist) only weighed 3 grains. They consisted of:—

3 Fragments of orthoclase felspar.

4 Bituminous or carbonaceous shale (? if not accidental).

5 Fragments of shell (undistinguishable species).

4 Granite, containing quartz, orthoclase, and muscovite.

15 Grey compact limestone.

62 Quartzose mica schist.

317 Hornblende schist; sometimes containing garnets.

273 Quartzite fragments, with a very few fragments of clear quartz; the majority of the pieces being of a dirty colour, often cemented together, were evidently the débris of quartzite rocks or beds of indurated sandstone, and not from granite.

28 Black compact rock containing augite, most probably a volcanic basalt.

—

718

"From 1263 fathoms (Station 22)."

A single rounded pebble, weighing 18 grains, chiefly quartz, with a little of a black mineral hornblende or tourmaline, probably from a metamorphic schist.

"Gravel from 1366 fathoms (Station 19*a*)."

Consisted of 51 small subangular pieces of rock, all less than ½ grain in weight, excepting only one fragment (angular) of quartz, which weighed 2 grains; they consisted of:—

19 Fragments of quartz, all of which appeared to have proceeded from the disintegration of crystalline schists, and not from granite.
9 Hornblende schist.
8 Mica schist.
7 Loose, dirty-white tufaceous limestone.
3 Small fragments of augite or tourmaline (? which).
1 Fragment of quartz, with tourmaline.
4 Fragments of indistinct and uncertain character.
51

"Gravel from 1476 fathoms (Station 21)."

Six small subangular fragments, the largest of which did not exceed two grains in weight; they were respectively:—

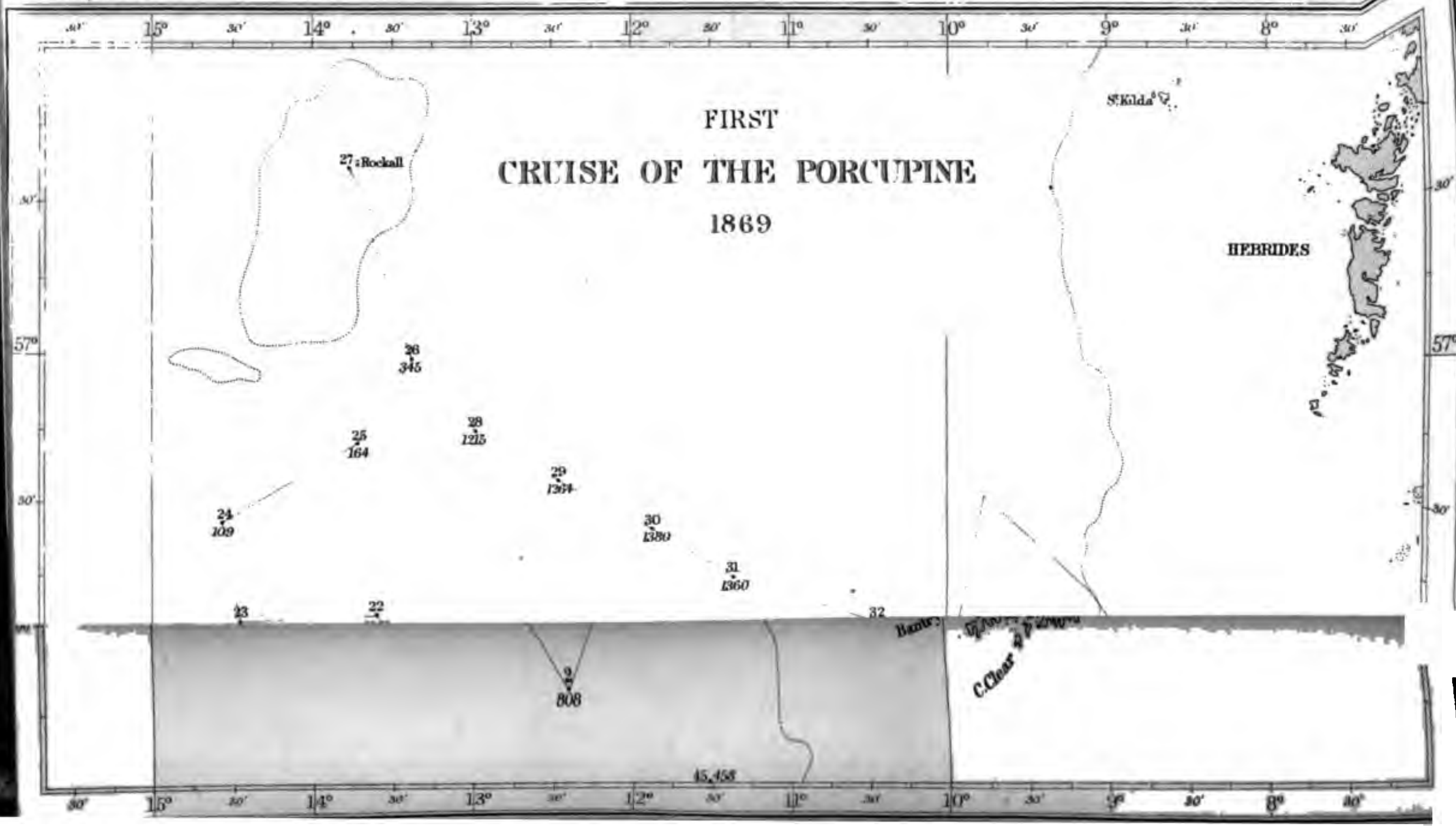
1 Yellow quartz.
1 Quartzose chlorite schist.
3 Mica schist.
1 Small fragment, apparently of volcanic lava.
6

The specimen from Rockall is not a fragment of any normal rock, but is only a brecciaform aggregate, principally consisting of quartz, felspar, and crystals of green hornblende, held together by a siliceous cement. It has evidently been broken from the projecting edge of a fault or vein fissure; and although it cannot settle the matter definitely as to what rocks this islet may really be composed of, it would indicate that it most probably is a mass of hornblendic gneiss or schist, and certainly not of true volcanic origin. I may mention that it does not at all resemble any of the fragments found in the deep-sea dredgings which I have as yet examined.

[illegible]

First Cruise of the 'Porcupine' (Plate 4).

Station No.	North Latitude.	West Longitude.	Depth in Fathoms.	Surface Temperature, Fahrenheit.	Bottom Temperature, Fahrenheit.
1.	51° 51′	11° 50′	370	54°·2	49°·0
2.	51 22	12 25	808	54·2	41·4
3.	51 38	12 50	722	54·5	43·0
4.	51 56	13 39	251	53·5	49·5
5.	52 7	12 52	364	54·0	48·8
6.	52 25	11 40	90	54·0	50·0
7.	52 14	11 48	159	53·2	50·4
8.	53 15	11 51	106	54·2	51·2
9.	53 16	12 42	165	53·5	49·7
10.	53 23	13 29	85	54·6	49·5
11.	53 24	15 24	1630	......	......
12.	53 41	14 17	670	52·2	42·6
13.	53 42	13 55	208	53·6	49·6
14.	53 49	13 15	173	53·2	49·6
15.	54 5	12 17	422	52·2	47·0
16.	54 19	11 50	816	53·0	39·5
17.	54 28	11 44	1230	53·2	37·8
18.	54 15	11 9	183	53·2	49·4
19.	54 53	10 56	1360	54·8	37·4
20.	55 11	11 31	1443	55·5	37·0
21.	55 40	12 46	1476	56·2	36·9
22.	56 8	13 34	1263	56·9	37·3
23.	56 7	14 19	630	57·3	43·5
23*a*.	56 13	14 18	420	56·8	46·4
24.	56 26	14 28	109	57·7	46·4
25.	56 41	13 30	164	56·8	46·5
26.	56 58	13 17	345	57·4	46·7
27.	Rockall Bank.	Rockall Bank.	54	55·6	48·3
28.	56 44	12 52	1215	57·6	37·1
29.	56 34	12 22	1264	56·9	36·9
30.	56 24	11 49	1380	56·0	37·1
31.	56 15	11 25	1360	56·9	37·2
32.	56 5	10 23	1320	55·9	37·4

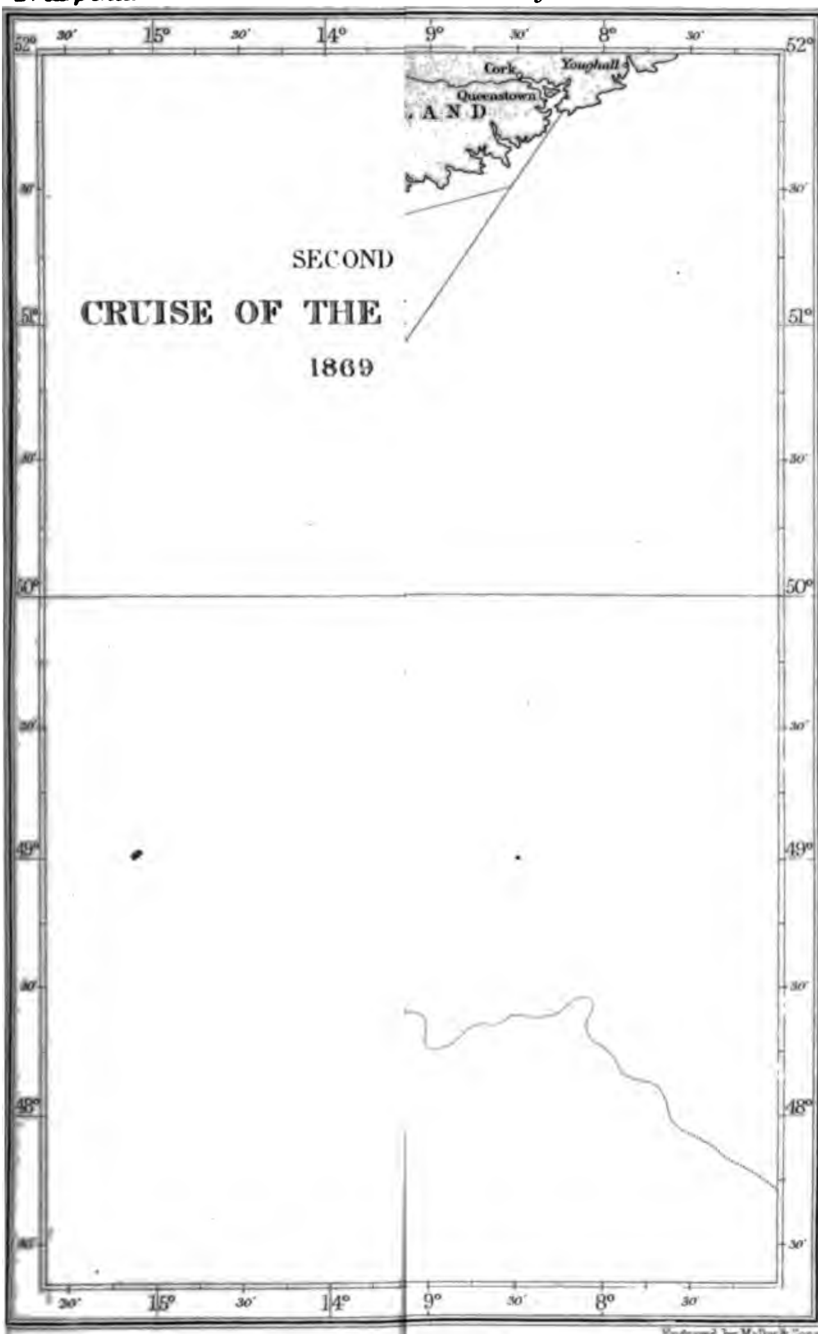

FIRST
CRUISE OF THE PORCUPINE
1869
27 Rockall
St. Kilda
HEBRIDES
26
345
25
164
28
1215
29
1364
24
109
30
1380
31
1360
23
22
32
Bantry
C. Clear
9
808
45,458
15°
14°
13°
12°
11°
10°
9°
8°
57°
30'

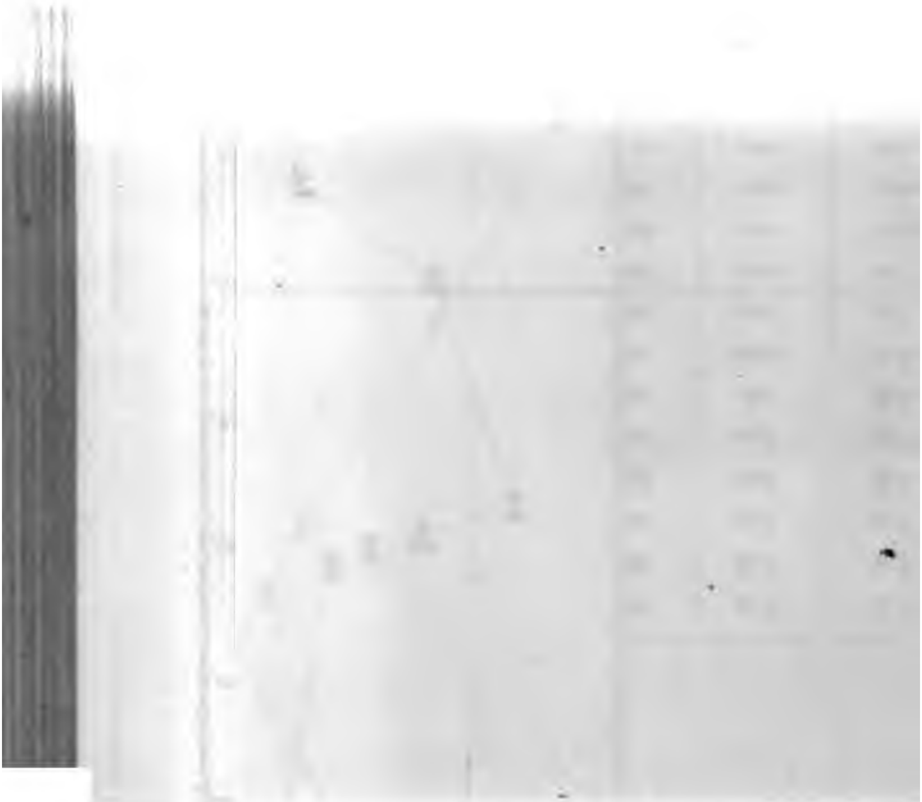
SECOND CRUISE OF THE 'PORCUPINE' (Plate 5).

Station No.	North Latitude.	West Longitude.	Depth in Fathoms.	Surface Temperature, Fahrenheit.	Bottom Temperature, Fahrenheit.
33.	50° 38′	9° 27′	74	65°·2	49°·6
34.	49 51	10 12	75	66·0	49·6
35.	49 7	10 57	96	63·4	51·3
36.	48 50	11 9	725	64·0	43·9
37.	47 38	12 8	2435	65·6	36·5
38.	47 39	11 33	2090	64·2	36·3
39.	49 1	11 56	557	63·0	47·0
40.	49 1	12 5	517	63·4	47·7
41.	49 4	12 22	584	63·4	46·5
42.	49 12	12 52	862	62·6	39·7
43.	50 1	12 26	1207	61·7	37·7
44.	50 20	11 34	865	61·2	39·4
45.	51 1	11 21	458	60·6	48·1

Dr. Carpenter

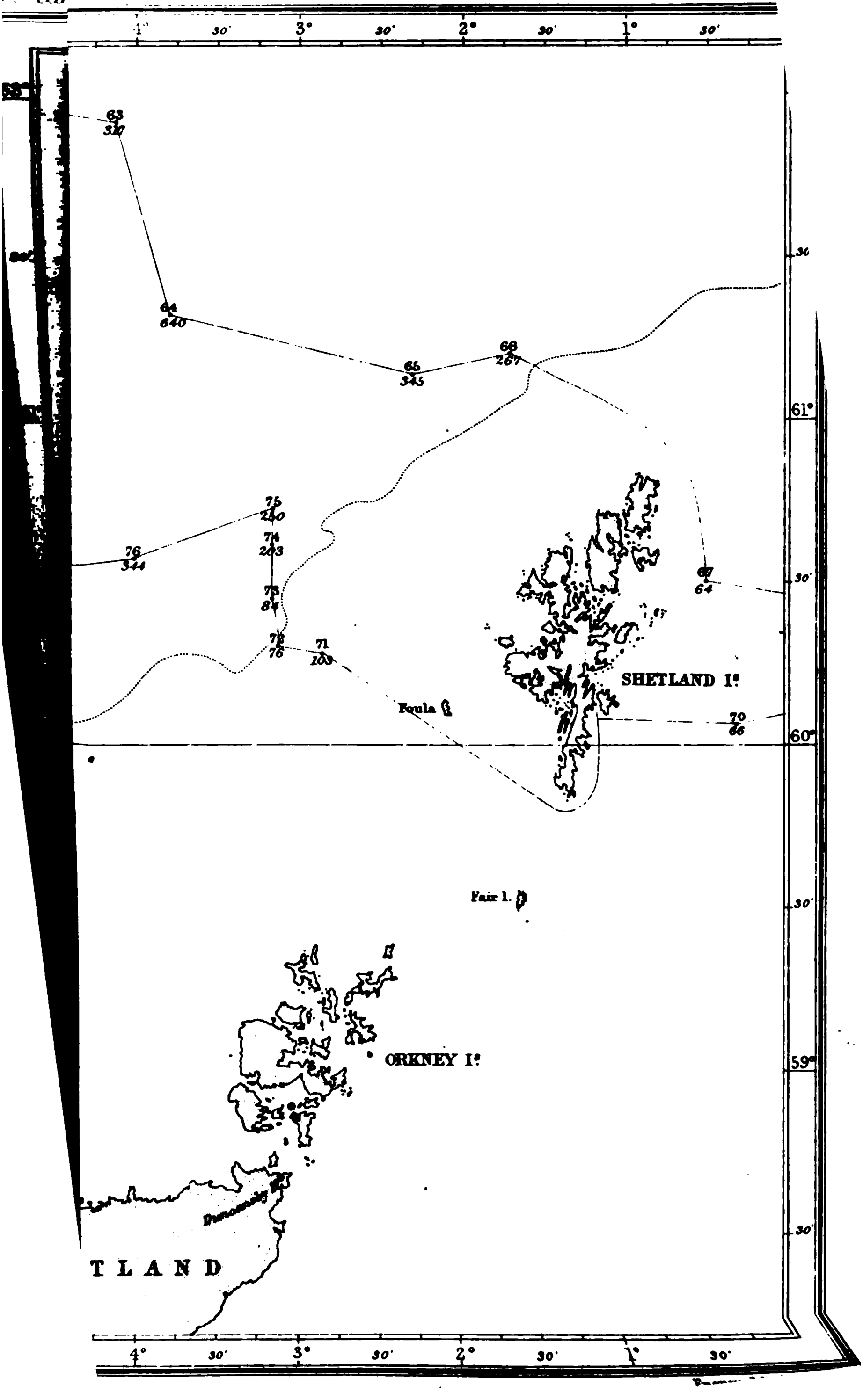
Proc. Roy. Soc. Vol. XVIII Pl. 5

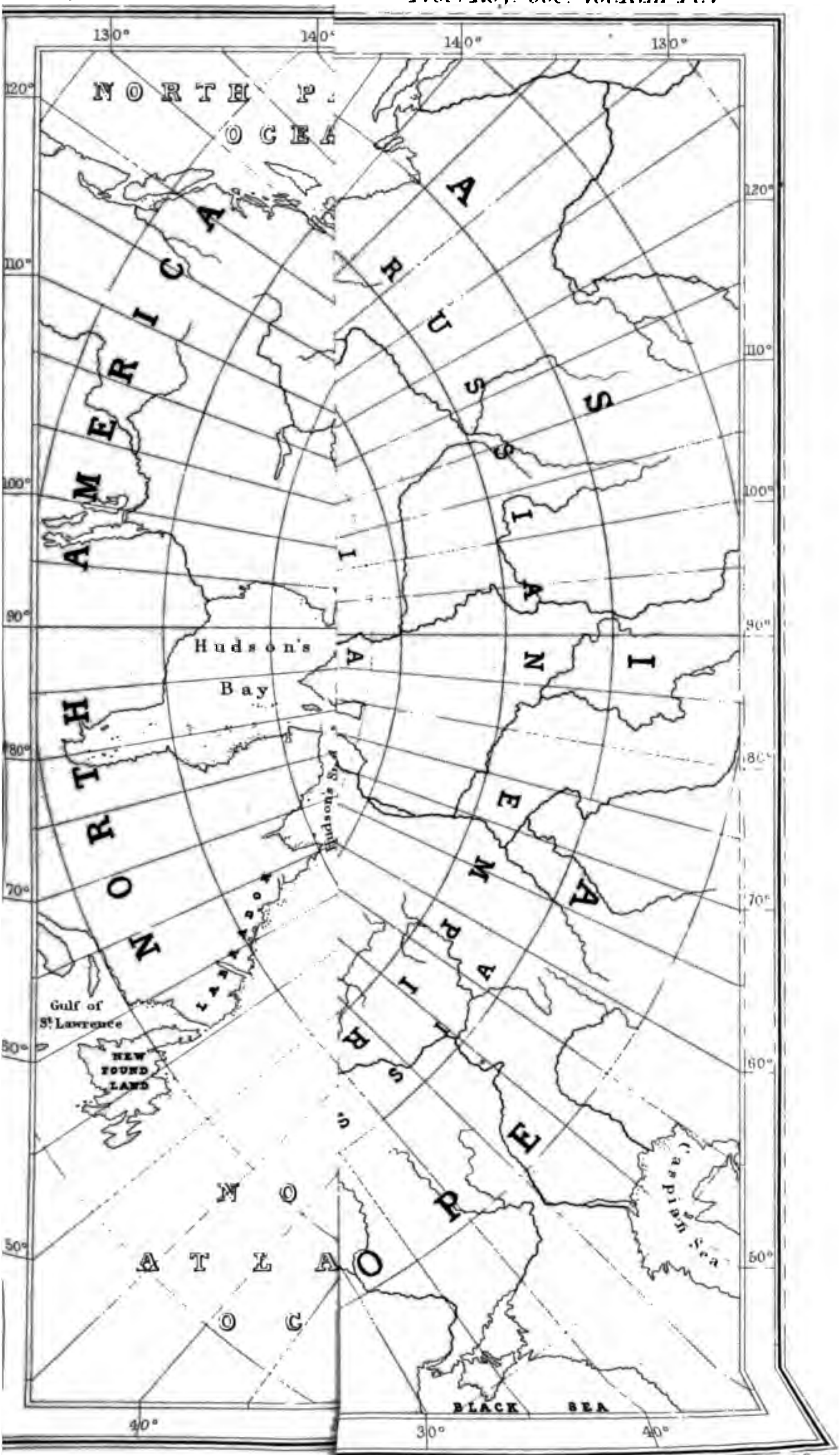




THIRD CRUISE OF THE 'PORCUPINE' (Plate 6).

Station No.	North Latitude.	West Longitude.	Depth in Fathoms.	Surface Temperature, Fahrenheit.	Bottom Temperatu[re,] Fahrenhei[t.]
46.	59° 23′	7° 4′	374	53°·9	46°·0
47.	59 34	7 18	542	54·0	43·8
48.	59 32	6 59	540	...	...
49.	59 43	7 40	475	53·6	45·4
50.	59 54	7 52	355	52·6	46·2
51.	60 6	8 14	440	51·6	42·0
52.	60 25	8 10	384	52·1	30·6
53.	60 25	7 26	490	52·1	30·0
54.	59 56	6 27	363	52·5	31·4
55.	60 4	6 19	605	52·6	29·8
56.	60 2	6 11	480	52·6	30·7
57.	60 14	6 17	632	52·0	30·5
58.	60 21	6 51	540	51·4	30·8
59.	60 21	5 41	580	52·7	29·7
60.	61 3	5 58	167	49·5	44·3
61.	62 1	5 19	114	50·4	45·0
62.	61 59	4 38	125	49·6	44·6
63.	61 57	4 2	317	49·0	30·3
64.	61 21	3 44	640	49·7	30·0
65.	61 10	2 21	345	52·0	30·0
66.	61 15	1 44	267	52·4	45·7
67.	60 32	0 29	64	51·9	49·1
68.	60 23	0 33 E.	75	52·5	44·0
69.	60 1	0 18 E.	67	53·5	43·8
70.	60 4	0 21	66	53·4	45·1
71.	60 17	2 53	103	53·0	48·6
72.	60 20	3 5	76	52·3	48·8
73.	60 29	3 6	84	52·7	48·8
74.	60 39	3 9	203	52·6	47·6
75.	60 45	3 6	250	51·5	41·9
76.	60 36	3 58	344	50·3	29·7
77.	60 34	4 40	560	50·9	29·8
78.	60 14	4 30	290	52·2	41·5
79.	59 44	4 44	76	52·1	48·9
80.	59 49	4 42	92	53·2	49·4
81.	59 54	5 1	142	53·3	49·1
82.	60 0	5 13	312	52·3	41·4
83.	60 6	5 8	362	53·1	37·5
84.	59 34	6 34	155	54·3	49·1
85.	59 40	6 34	190	53·9	48·6
86.	59 48	6 31	445	53·6	30·1
87.	59 35	9 11	767	52·5	41·4
88.	59 26	8 23	705	53·5	42·6
89.	59 38	7 46	445	53·1	45·5
90.	59 41	7 34	458	53·1	45·2
VI.	60 45	4 49	510	52	31·7
VII.	60 7	5 21	500	51	30·2
VIII.	60 10	5 59	550	53	29·8
X.	60 28	6 55	500	51	30·8
XI.	60 30	7 16	450	50	31·2
XII.	59 36	7 20	530	52·5	44·8
XIV.	59 59	9 15	650	53	42·5
XV.	60 38	11 7	570	52	43·5
XVII.	59 49	12 36	620	52	43·5


SHETLAND Iˢ
ORKNEY Iˢ
Foula I.
Fair I.
TLAND
61°
60°
59°
4°
3°
2°
1°
30'



Engraved by Malby & Sons

June 16, 1870.

General Sir EDWARD SABINE, K.C.B., President, in the Chair.

Dr. E. H. Greenhow, Dr. J. Jago, Prof. N. S. Maskelyne, the Rev. Dr. Parkinson, Capt. R. M. Parsons, Dr. W. H. Ransom, Mr. R. H. Scott, Dr. A. Voelcker, and Dr. S. Wilks were admitted into the Society.

The following communications were read:—

I. "On Compounds Isomeric with the Cyanuric Ethers." By A. W. Hofmann and Otto Olshausen. Received April 29, 1870.

Some time ago M. Cloëz * described a remarkable body, which has the composition but none of the properties of ethylic cyanurate. This substance, which he called *cyanetholine*, is distinguished from cyanuric ether by its behaviour with alkalies, which, according to the observations of Cloëz, evolve from it ammonia and not ethylamine. Cyanetholine, according to Cloëz, combines with acids, forming crystallizable salts, none of which, however, up to the present time, has been more carefully investigated. It is rather strange how little the attention of chemists has been directed to this interesting compound. M. Cloëz contented himself with the discovery of cyanetholine and establishing its composition, but has not again reverted to the subject. Of the researches of other chemists who have touched upon cyanetholine, the only ones known to us are the few but rather important experiments of M. Gal †. According to his observations, cyanetholine is changed by treatment with potash solution into potassium cyanide and alcohol, and by the action of hydrochloric acid into cyanuric acid and ethylic chloride; and Gal and Cloëz, in consequence of these reactions, are of opinion that cyanetholine is the true ether of cyanuric acid constructed upon the water type—

$$\left.\begin{matrix}H\\H\end{matrix}\right\}O \qquad \left.\begin{matrix}CN\\H\end{matrix}\right\}O \qquad \left.\begin{matrix}C_2H_5\\H\end{matrix}\right\}O \qquad \left.\begin{matrix}CN\\C_2H_5\end{matrix}\right\}O,$$

whilst the earlier known ethylic cyanate of M. Würtz corresponds to the ammonia type—

$$\left.\begin{matrix}H\\H\\H\end{matrix}\right\}N \qquad \left.\begin{matrix}(CO)''\\H\end{matrix}\right\}N \qquad \left.\begin{matrix}C_2H_5\\H\\H\end{matrix}\right\}N \qquad \left.\begin{matrix}(CO)''\\C_2H_5\end{matrix}\right\}N.$$

It need scarcely be mentioned how completely this view has been confirmed by the subsequent discovery of the isonitriles and of the series of mustard oils isomeric with the sulphocyanic ethers.

The formation of cyanetholine, which, as is known, is obtained by the action of chloride of cyanogen on sodium ethylate, proves a close connexion

* Compt. Rend. xliv. p. 482, and Ann. Chem. Pharm. cii. p. 354.

† Compt. Rend. lxi. p. 527, and Ann. Chem. Pharm. cxxxvii. p. 127.

between this body and the ethylcyanamide, discovered by Messrs. Cahours and Cloëz *, which is formed by treating ethylamine with chloride of cyanogen. The same reagent acting on ethylated water and ethylated ammonia causes in the one case the formation of ethylic cyanate, and in the other that of ethylcyanamide. The close analogy between cyanetholine and ethylcyanamide, which is, perhaps, best represented by the formulæ

$$CN(C_2H_5)O \quad \text{and} \quad CN(C_2H_5)HN,$$

cannot possibly be doubted, and accordingly the easy polymerization of ethylcyanamide, which is readily converted into triethylmelamine, naturally raised the question as to whether cyanetholine could not be polymerized in a similar manner; in other words, whether there might not exist a series of combinations isomeric with the known cyanuric ethers.

The experiments undertaken for the solution of this question have been performed in the methyl-, ethyl-, amyl-, and phenyl-series.

We begin the communication of our observations by a description of the experiments made in the methyl-series; for although in the first instance we had worked in the ethyl-series, it was the investigation of the methylic compounds which yielded at once results that could not be mistaken.

Experiments in the Methyl-series.

When a stream of gaseous chloride of cyanogen is passed through a dilute solution of sodium methylate in methylic alcohol (we have generally dissolved 20 grms. of sodium in about 400 grms. of anhydrous methylic alcohol), a considerable quantity of common salt is separated. If the current of gas be continued until the solution smells of chloride of cyanogen, and the excess of methyl alcohol then distilled off, a brown oil remains behind, similar to that which Cloëz obtained by the corresponding experiment in the ethyl-series, and which he described as cyanetholine. This oil sometimes remains fluid for a long time, but generally solidifies on standing. Frequently, however, little or no oil is formed, and when the methyl alcohol is distilled off, there remains a residue which solidifies to a brown crystalline mass. The purification of this substance offers no difficulties; one or two crystallizations from boiling water, in which it is easily soluble, whilst it dissolves but slightly in cold water, and a final treatment with animal charcoal remove the colour. But these crystals, though perfectly colourless, prove, under the microscope, to be a mixture of two compounds, of which the one, crystallizing in fine needles, is the more easily soluble, whilst the other, consisting of rhombic tables, dissolves with greater difficulty. If an intermediate mixed product be sacrificed, they may, by repeated crystallizations from boiling water, be both obtained in a pure state. They are, however, better separated by the extraordinary difference of their solubility in ether, which dissolves the needles and leaves the rhombic tables behind.

* Ann. Chem. Pharm. xc. p. 91.

Methylic Cyanurate.—When the ether which has been poured off the crystals is evaporated, a crystalline mass is left, which may be recrystallized from alcohol, or, better, from hot water. The needles thus obtained possess the characters of a pure substance. Determination of the carbon, hydrogen, and nitrogen, the latter of which can easily be weighed as ammonia, yields as the simplest atomic expression for this body the formula

$$C_2H_3NO;$$

but it only required a somewhat closer examination to prove that this is not the methylic cyanate but the methylic cyanurate, not the monomolecular but the trimolecular combination. The melting-point of the crystals is 132°, the boiling-point (we were only in possession of a moderate quantity) between 160° and 170°. These properties unmistakably characterize the trimolecular compound, the cyanurate.

It would have, nevertheless, been desirable experimentally to confirm these indications by the vapour-density determination; but this was prevented by a peculiar comportment of the new body, which, however, furnished evidence almost as conclusive as the vapour-density for the molecular weight of the compound. When the new cyanurate is heated in a retort, it distils without leaving an appreciable residue, the distillate solidifying to a white crystalline mass. But these crystals are no longer the unchanged body; their melting-point has risen from 132° to 175°, and their crystalline form is entirely changed; in the place of fine needles we have now short, thick prisms with sharply defined summits. It is easily perceived that the new cyanuric ether, by an atomic migration within the molecule, which may be represented by the equation

$$\left.\begin{matrix}(CN)_3\\(CH_3)_3\end{matrix}\right\}O_3 = \left.\begin{matrix}\overset{\text{II}}{(CO)_3}\\(CH_3)_3\end{matrix}\right\}N_3,$$

has become converted into the long known ether. If the careful investigation of the physical properties were not deemed sufficient proof of this transformation, it would suffice to compare the reactions of the body *before* and *after* distillation. Before being distilled, it yields cyanuric acid and methyl alcohol when heated with potash,—

$$\left.\begin{matrix}(CN)_3\\(CH_3)_3\end{matrix}\right\}O_3 + 3H_2O = \left.\begin{matrix}(CN)_3\\H_3\end{matrix}\right\}O_3 + 3(CH_3HO).$$

If submitted to the same treatment after distillation, methylamine and carbonic acid are obtained,—

$$\left.\begin{matrix}(CO)_3\\(CH_3)_3\end{matrix}\right\}N_3 + 3H_2O = 3\left[\left.\begin{matrix}(CH_3)\\H_2\end{matrix}\right\}N\right] + 3CO_2.$$

These experiments are sufficient to establish the nature of the new cyanuric ether. In order to obtain further data as to the constitution of this body, the changes which it undergoes by the action of ammonia had to be examined.

Whilst the ether of a monobasic acid, when treated with ammonia gas,

by an interchange of the primary alcohol fragment with the primary ammonia fragment, is directly converted into the amide, whilst, on the other hand, the ether of a bibasic acid yields, in the first instance, the ether of an amidic acid, the production of the true amide of a tribasic acid must necessarily be preceded by the formation of the ethers of a first and of a second amidic acid. According to this view, the action of ammonia on the methylic cyanurate

$$C_3N_3\left\}\begin{array}{l}CH_3O\\CH_3O\\CH_3O\end{array}\right.$$

may be expected to give rise to the formation of the following bodies:—

$$\underset{\text{Dimethylic amido-cyanurate.}}{C_3N_3\left\}\begin{array}{r}CH_3O\\CH_3O\\H_2N\end{array}\right.}\qquad \underset{\text{Methylic diamido-cyanurate.}}{C_3N_3\left\}\begin{array}{r}CH_3O\\H_2N\\H_2N\end{array}\right.}\qquad \underset{\text{Triamide of cyanuric acid.}}{C_3N_3\left\}\begin{array}{r}H_2N\\H_2N\\H_2N\end{array}\right.}$$

not to speak of the possibility of alcohol fragments being simultaneously exchanged for water fragments.

Hitherto we have met only with one of the above-mentioned bodies, viz.:—

Dimethylic Amido-cyanurate.—This compound is formed by the action of ammonia on the new methylic cyanurate; but it is not easy to obtain it pure by this means, as the reaction generally goes further, and a mixture of substances is produced, the separation of which we have not hitherto been able to effect. The compound in question, however, is always formed in larger or smaller quantity as a by-product in the preparation of the trimethylic cyanurate; it is, in fact, the substance insoluble in ether mentioned above, and as no other product is formed but these two bodies, it is easy to obtain the dimethylated amidic acid in a pure state.

The new compound crystallizes from hot water in fine rhombic tables, odourless and tasteless, and melting at 212°. It is much more difficultly soluble in cold water than the cyanuric ether, soluble with difficulty in cold alcohol, more easily in hot, almost insoluble in cold ether. The composition

$$C_5H_8N_4O_2 = C_3N_3\left\}\begin{array}{r}CH_3O\\CH_3O\\H_2N\end{array}\right.$$

was established by a determination of the carbon, hydrogen, and nitrogen, and also by the analysis of a silver-salt,

$$C_5H_8N_4O_2,\ AgNO_3,$$

which, crystallizing in fine needles, is obtained by adding silver nitrate to the nitric solution of the amido-ether and recrystallization of the precipitate.

By treatment with aqueous ammonia in sealed tubes, the same products are obtained as are furnished by the original ether when submitted to the

action of ammonia. They have not yet been investigated, but it has been ascertained that methyl alcohol is liberated, as might have been expected.

Finally, as regards the formation of the amido-ether by the action of chloride of cyanogen upon sodium methylate, this is obviously due to the presence of traces of water, which could scarcely be avoided in this process. Water causes first the formation of hydrochloric and cyanic acids, the latter of which splits up into carbonic acid and ammonia; ammonia and methylic cyanurate coming together in the nascent state form methyl alcohol and the amido-ether.

In fact the common salt which separates during the reaction contains a considerable quantity of cyanate and carbonate.

Experiments in the Ethyl-series.

Our first experiments were conducted in this series, and we have actually worked more in it than in the methyl group. We have, however, not yet been able to obtain the ethylic cyanurate in a pure state; on the other hand, we have succeeded in obtaining the ethers of both amidic acids.

The action of chloride of cyanogen upon sodium ethylate present the same phenomena as the analogous treatment of the methylate, and which moreover have been well described by M. Cloëz. We have sometimes obtained at once a solid body; but generally there is formed an oil, which after some time deposits crystals, the quantity of which in different operations varies exceedingly. The idea naturally suggested itself that they were the trimolecular modification of cyanetholine; but analysis showed that these crystals, in spite of their beauty, were but a mixture containing the desired cyanurate, when at all, only in small quantity. They contain, as numerous analyses have proved, the ethylic ethers of the two amidic acids, the separation of which has cost indeed very considerable trouble.

Diethylic Amido-cyanurate.—By treatment with animal charcoal and numerous recrystallizations of considerable quantities of the crystals obtained from the crude cyanetholine, we succeeded in obtaining thin white prismatic crystals melting at 97°; this melting-point remained unchanged after several recrystallizations from water, a sign of the purity of the substance. The same body was obtained when crude cyanetholine was heated for some hours with aqueous ammonia in a sealed tube. The digestion, however, must not be carried too far, as then other products are formed, amongst these an amorphous substance quite insoluble in water.

The analysis of the crystals, which are soluble both in alcohol and ether, especially when warm, has proved them to be the ethylic compound corresponding to amido-ether of the methyl series, having the composition

$$C_7 H_{12} N_4 O_2 = C_3 N_3 \left\} \begin{matrix} C_2 H_5 O \\ C_2 H_5 O \\ H_2 N \end{matrix} \right.$$

The diethylic amido-cyanurate combines in two proportions with silver

nitrate. According as the substance dissolved in nitric acid or the silver nitrate is in excess, we obtain the compounds—

$$2C_7 H_{12} N_4 O_2, Ag NO_3,$$
$$\text{or}\quad C_7 H_{12} N_4 O_2; Ag NO_3.$$

Both salts crystallize in needles; the latter can be recrystallized from boiling water without appreciable change, but the former is decomposed, being converted into the second salt.

Ethylic diamido-cyanurate.—White crystals were deposited from a solution of the above-described but not fully purified compound, which had been standing for a long time with concentrated solution of ammonia.

These melted between 190° and 200°, and were much more difficultly soluble in alcohol. Numbers were obtained by the analysis of these crystals (carbon, hydrogen, and nitrogen determinations) which indicated it to be the ethylic diamido-cyanurate,—

$$C_5 H_9 N_5 O = C_3 N_3 \left.\begin{array}{l} C_2 H_5 O \\ H_2 N \\ H_2 N \end{array}\right\}$$

This compound also, when dissolved in nitric acid, gives fine crystalline needles on the addition of silver nitrate. These, however, have not yet been analyzed.

Experiments in the Amyl-series.

Up to the present time we have only worked qualitatively in this series. The product of the action of chloride of cyanogen on sodium amylate is oily; it distils at about 200°, but not, as it would appear, without being thoroughly altered. The last portions of the distillate solidify to a mass of white lustrous crystals, which can easily be obtained pure by solution and recrystallization. We are inclined to consider this substance as amylic cyanurate, but at present we have no numbers to confirm this opinion.

Experiments in the Phenyl-series.

Lastly, we may here mention some experiments which were made in the phenyl-series. Chloride of cyanogen acts with the same energy upon sodium phenylate (which in this case was dissolved in absolute alcohol) as on the other sodium compounds. The solution poured off from the common salt which had separated gave, on the addition of water, an oil heavier than water, which was submitted to distillation. What first came over was almost pure phenol; the distillation was interrupted as soon as a drop of the residue solidified to a crystalline mass. The residue in the retort was then mixed with cold alcohol, thrown on a filter and washed for some time with the same liquid. The white crystalline mass thus obtained was then recrystallized from a very large quantity of boiling alcohol. When the solution was allowed to cool slowly, long thin needles separated which were almost insoluble in alcohol and ether, but were found to dissolve, though sparingly, in benzol.

The analysis of these crystals leads to the formula

$$C_7H_5NO.$$

But from their formation as well as their general properties we are convinced that they are the trimolecular combination, the phenylic cyanurate,

$$C_{21}H_{15}N_3O_3 = C_3N_3\left\{\begin{array}{l}C_6H_5O\\C_6H_5O\\C_6H_5O\end{array}\right.$$

which corresponds to the methyl compound described in the beginning of this note.

The melting-point of the crystals was found to be 224°, somewhat lower than that of the isomeric compound (264°) which one of us * has lately examined.

The latter, which must now be regarded as the phenylic isocyanurate, is easily distinguished from the new cyanurate, both by its crystalline form and behaviour with solvents. It has yet to be determined whether the phenyl compound, like the methyl one, is changed under the influence of heat into the cyanurate already known.

We cannot close this communication without thanking Messrs. R. Bensemann and K. Sarnow for the assistance they have rendered us in carrying out these experiments.

II. "Contributions towards the History of Thiobenzamide." By A. W. Hofmann, LL.D., F.R.S. Received May 27, 1870.

When a stream of sulphuretted hydrogen is passed through a solution of benzonitrile in alcoholic ammonia, the liquid, after the lapse of a few hours, deposits fine yellow needles, which are the thiobenzamide,

$$C_7H_7NS = \left.\begin{array}{c}C_7H_5S\\H\\H\end{array}\right\}N,$$

discovered by M. Cahours. It can be obtained in a pure state by recrystallization from boiling water.

When a cold saturated alcoholic solution of this body is mixed with an alcoholic solution of iodine, the latter is immediately decolorized with separation of sulphur. If the addition of iodine solution be continued until even after a short boiling free iodine remains, which can readily be detected by starch-paste, the solution filtered from the sulphur, and poured into water, solidifies to a mass of white interlaced needles, which can readily be freed from adhering hydriodic acid by washing with cold water.

This substance can be obtained pure by repeated crystallization from boiling alcohol. In this state it forms long shining snow-white needles, which melt at 90°, and distil without decomposition at a very high temperature. The compound also dissolves in ether, chloroform, and benzol. At first I believed it to be free from sulphur. Its alcoholic solution can be

* Hofmann, Berichte der Chem. Gesellsch. z. Berlin, III. 268.

boiled for hours with a lead salt and an alkali without the formation of lead sulphide; also, after treating with moderately concentrated nitric acid, the sulphur contained in the body remains unchanged. Only after several days' boiling with alcoholic soda, the sulphur separates as sodium sulphide, and as it appears sodium hyposulphite. The determination of the sulphur, however, offers no difficulty when the vapour of the compound is passed over a red-hot mixture of nitre and sodium carbonate.

The careful analysis of the new crystals leads to the formula

$$C_{14}H_{10}N_2S.$$

They are consequently derived from two molecules of thiobenzamide from which one atom of sulphur and four atoms of hydrogen have been removed, the latter in the form of hydriodic acid,

$$2C_7H_7NS+2II=C_{14}H_{10}N_2S+4HI+S.$$

Chlorine, bromine, and moderately diluted nitric acid act upon thiobenzamide in the same way as iodine. These reagents, however, are not to be recommended for the preparation of the new compound, as the action easily goes too far, causing the formation of chlorinated, brominated, and nitro-products, which contaminate the normal compound. In fact, Mr. Richard Dunklenberg, whilst studying thiobenzamide last summer in the Berlin Laboratory, has had already in his hands the new sulphuretted compound; but as he employed bromine for its preparation, the substance was obtained in a less pure state, and consequently he did not succeed in interpreting the reaction.

With regard to the constitution of the new body, it may be considered as consisting of two molecules of benzonitrile, which are held together directly by the sulphur. Different views may be taken of the arrangement of the atoms in the molecule. Probably the carbon atoms outside the phenyl group are joined together by the sulphur; and there is then also connexion between the nitrogen atoms. This latter supposition is strengthened by the behaviour of the crystals with nascent hydrogen, described below. But I will not go further into this question at present, since the prosecution of the new reaction in other series promises to yield further experimental data for a profitable discussion of the subject. For the same reason, I refrain at present from proposing a name for the new sulphur-compound.

The stability of this substance is remarkable. It can be heated for a long time to 150° in sealed tubes with hydrochloric acid, dilute sulphuric acid, and even moderately strong nitric acid, without undergoing decomposition. It dissolves in concentrated sulphuric acid by the aid of a gentle heat, and the addition of water precipitates it again unchanged. The compound is decomposed rather more readily by alkalies, although, as already mentioned, it is necessary, even in this case, to boil for days. In this case benzoic acid is reproduced with slow evolution of ammonia. Evidently the sulphur here first separates from the molecule, and is dis-

solved in the ordinary manner by the alkali; the benzonitrile set free at the same time yields ammonia and benzoic acid. The separated acid, which was recognized as benzoic acid by the sparing solubility of its sodium salt, was further identified by a determination of its melting-point and an analysis of the silver salt. The sulphur-compound suffers a very interesting change by the action of nascent hydrogen. I have already* called attention to the fact how much more readily the thioamides are converted into the corresponding amine bases than the nitriles. This experience has again been verified in the new body. When its alcoholic solution is decomposed by zinc and hydrochloric acid, sulphuretted hydrogen is evolved in abundance. After ten or twelve hours the total decomposition of the sulphur-compound is recognized by the addition of water no longer producing any precipitate in the alcoholic solution. The new product is collected and purified by a process repeatedly proved to be successful; the addition of an excess of alkali until the zinc hydrate at first thrown down is redissolved leaves the base in the supernatant alcoholic layer. After evaporation of the alcohol, the base still containing fixed alkali is dissolved in ether, and withdrawn from this by hydrochloric acid, whereby a small quantity of brown resin is separated and remains dissolved in the ether. On evaporating the hydrochloric solution on the water-bath, the hydrochlorate of the base is left behind as an oil, which in a short time solidifies to a mass of indistinct crystals. When the aqueous solution of this salt is decomposed by ammonia, oily drops immediately separate and sink to the bottom; in the course of a day these solidify to a crystalline mass, the supernatant fluid being filled with iridescent plates.

The analysis of the hydrochlorate, purified by recrystallization from water, and dried at 100°, which it can be without change, leads to the formula

$$C_{14}H_{15}N_2Cl = C_{14}H_{14}N_2, HCl,$$

which was satisfactorily confirmed by a determination of the crystalline platinum salt dried at 100°, containing

$$C_{28}H_{30}N_4PtCl_6 = 2(C_{14}H_{14}N_2, HCl), PtCl_4.$$

In the formation of the above-mentioned base 4 at. hydrogen have taken the place of 1 at. of sulphur,

$$C_{14}H_{10}N_2S + 3HH = H_2S + C_{14}H_{14}N_2.$$

If the molecular arrangement of the sulphur body formerly indicated be correct, the action of the hydrogen would remove the sulphur bond from between the carbon atoms, and by simultaneously loosening the attraction between these carbon atoms and the nitrogen atoms, the union of two hydrogen atoms to each carbon atom would be rendered possible; both nitrogen atoms would then be doubly linked together. That these are, in fact, very strongly united is shown by the circumstance that the new base undergoes no further change under the influence of hydrogen. I had

* Hofmann, Proc. Roy. Soc. vol. xvi. p. 445.

hoped that by continued treatment with nascent hydrogen it might have taken up four atoms more of hydrogen and split up into benzylamine,—

$$C_{14}H_{14}N_2+2HH=2C_7H_9N.$$

Up to the present time I have not succeeded in effecting this transformation, although I have continued the action of zinc and hydrochloric acid for several days. I do not, however, by any means consider this transformation impossible.

It is worthy of remark that the base here described, and for which I also refrain from proposing a name until its constitution is better ascertained, is isomeric with a body which I formerly obtained *. Ethenyldiphenyldiamine,

$$\left.\begin{matrix}(C_2H_3''')\\ C_6H_5\\ C_6H_5\\ H\end{matrix}\right\} N_2=C_{14}H_{14}N_2,$$

formed by the action of trichloride of phosphorus on one molecule of acetic acid and two molecules of aniline with separation of two molecules of water, has not only the same composition and the same molecular weight, but is monacid like the base derived from the sulphur body. It requires however, scarcely more than a cursory comparison of the two substances to be convinced that this is a case of isomerism and not of identity. The crystalline forms, both of the two bases and also of their salts, differ widely from one another. Besides the previously mentioned salt, I have also compared the nitrate of the new base, which is, though with difficulty, obtained in six-sided tables, with the beautiful nitrate of the previously studied base. Ethenyldiphenyldiamine is quite neutral, whilst the alcoholic solution of the unnamed base has a distinctly alkaline reaction. The melting-points also of the two bases differ widely; the old one melts at 137° and the new at 71°. Lastly, the behaviour of the two bodies with concentrated sulphuric acid leaves no doubt but that they are different; ethenyldiphenyldiamine is changed under these circumstances without blackening into sulphanilic acid and acetic acid. The base derived from thiobenzamide is charred with evolution of sulphurous acid.

I am indebted to Mr. K. Sarnow for his valuable assistance in prosecuting the above researches.

III. "Contributions to the History of the Acids of the Sulphur Series.—I. On the Action of Sulphuric Anhydride on several Chlorine and Sulphur Compounds." By Henry E. Armstrong, Ph.D. Communicated by E. Frankland, Ph.D., F.R.S. Received May 2, 1870.

Kuhlmann †, in a comprehensive memoir on the formation of ether, mentions incidentally the direct combination of sulphuric anhydride with

* Hofmann, Proc. Roy. Soc. vol. xv. p. 55.

† Ann. Ch. Pharm. xxxiii. p. 108.

ethylic chloride to a liquid, which fumes strongly in the air; this treated with water yielded him an oily product, which, however, could not be distilled without undergoing decomposition.

Robert Williamson * also made experiments on the formation of this compound, and, from the amount of anhydride and ethylic chloride entering into the reaction, came to the conclusion that it was the ethylic compound, homologous with Williamson's sulphuric chlorhydrate, which he also, by an analogous process, succeeded in obtaining by the action of hydric chloride on sulphuric anhydride.

Excepting these two short notices, nothing was known of the properties and constitution of this body; and it therefore appeared to me of interest to submit it to a more close examination.

Whilst occupied therewith, a paper of Pourgold's † appeared on the same subject, by the series of reactions described in which it is proved that the formula ascribed to it by Robert Williamson is in reality the rational one.

Thus further experiments of mine in this direction were rendered unnecessary.

According to my observations, however, the simple formation of this one chloride is not the only phase of the reaction. One always obtains, as Pourgold also mentions, considerable quantities of products of higher boiling-point; and I have found that, by heating the same some time with water in a sealed tube at 120° C., afterwards evaporating to drive off the hydric chloride formed, and neutralizing with baric carbonate, a permanent salt was obtained, which by analysis was proved to have the composition of baric isethionate.

The formation of the chloride from which this salt has resulted is certainly remarkable, although easily explicable, as seen by the following equation:—

$$C_2H_5Cl + SO_3 = C_2H_4 \left.\begin{matrix} HO \\ Cl \end{matrix}\right\} SO_2.$$

Again, on one occasion the impure liquid obtained directly by passing the ethylic chloride into a flask containing the sulphuric anhydride, kept cool by being surrounded with ice, had, on standing over night, deposited a quantity of large, irregular, prismatic needles, of an exceedingly decomposable nature, the composition of which I unfortunately did not succeed in fixing, and on no future occasion was I able to obtain the same again. I intend, however, shortly to renew the study of these by-products, and also of the analogous reactions on employment of methylic and amylic chlorides.

The extension of these experiments to the chlorinated chlorides of the C_nH_{2n+1} series was full of interest, as, commencing with carbonic tetrachloride, there was a certain possibility of arriving by this means at a trichlormethylic alcohol. I therefore entered upon the investigation with

* Chem. Soc. Quart. Journ. x. p. 100.

† Comp. Rend. lxvii. p. 452.

the intention of applying the reaction to members of the several series of organic haloid compounds. In the following are contained the results of most of the experiments hitherto made; and although in a less complete form than I could wish, I am induced to make them now public, as for several reasons it will be some time before I shall be enabled to continue my experiments in this direction.

Action of Sulphuric Anhydride on Carbonic Tetrachloride.

In this, as in all the following experiments, the liquid was added to the sulphuric anhydride by means of a drop-funnel provided with a glass stop-cock. The anhydride was prepared by distillation of Nordhausen sulphuric acid, and condensed in a wide-mouthed flask. This flask was connected, by means of a cork provided with two borings, with an inverted Liebig's condenser, and with the drop-funnel.

The action set in immediately on allowing the carbonic tetrachloride to drop on to the anhydride, and was accompanied from the beginning by a constant evolution of gas. The smell and suffocating properties of this gas characterized it at once as carbonic oxychloride; it was entirely absorbed by absolute alcohol, the absorption being accompanied by a great rise in temperature. On the subsequent addition of water a heavier layer was precipitated, which was separated from the wash-water and dried over calcic chloride. It was then obtained as a colourless, mobile liquid, boiling between 90° and 95° C., traces of which exercised a most irritating action on the eyes. The B.P. of chlorocarbonic ether, with which it agrees in all its properties, is given as 94° C.

The rise in temperature on adding $C\,Cl_4$ to the anhydride was very considerable; and after one equivalent of the former to two of the latter was present, it was only necessary to apply the heat of a water-bath for a short time in order to complete the reaction; there then remained a heavy, dark brown-coloured liquid in the flask, on subjecting which to distillation a small quantity $C\,Cl_4$ first passed over, whereupon the thermometer rose rapidly, and between 130° and 150° the whole distilled over. After repeated rectification the pure product was obtained of B.P. 141°–145° (uncorrected) under a normal pressure. Thus prepared it is a colourless, heavy, mobile liquid, constantly fuming in the air, and which refracts light strongly.

On analysis, the following numbers were obtained:—

·342 grm. gave ·7366 grm. $Ba\,SO_4$=29·56 S.
·296 grm. gave ·3922 grm. $Ag\,Cl$ =32·8 Cl.

which results correspond with the formula $S_2\,O_5\,Cl_2$, as is evident from the following comparison of the analytical with the calculated numbers:—

		Calculated.	Found.
$S_2 =$	64............	29·7	29·56
$Cl_2 =$	71............	33·0	32·80
$O_5 =$	80............	—	—
	215		

The formation of this chloride, and of carbonic oxychloride, is explained by the following equation :—

$$CCl_4+2SG_3=COCl_2+S_2O_5Cl_2.$$

This body, which I, for reasons to be mentioned later on, call pyrosulphuric chloride, was first discovered by H. Rose, who, in his description of its properties, especially calls attention to its being but slowly decomposed in contact with water at ordinary temperatures,—a statement which I can thoroughly endorse.

Schützenberger *, however, who in the meantime has also published a series of observations on this reaction, with which on the whole mine agree, differs very considerably in his description of this chloride, which, according to him, boils at 130° C.†, and is decomposed *immediately* by water. On this he lays particular stress, and draws the conclusion that either his substance is isomeric with Rose's, or that Rose worked with an impure substance.

It seems to me, however, that the contrary is the case,—that Rose describes the properties of the pure substance, although, to judge from his analyses, his was not a chemically pure one. To make the chlorine and sulphur estimations, I broke a very thin glass bulb, filled with a weighed quantity of the liquid, under water ; but so great is the relative stability of the chloride, that a considerable time elapsed before the small quantity employed was decomposed, even when I used a dilute potassa solution, which was warmed to 50°–60° C.

The direct substitution of two chlorine atoms by one oxygen atom, which has taken place in carbonic tetrachloride, is, as far as I know, the first instance of this nature among organic compounds. The formation of phosphoric oxychloride from phosphoric chloride by means of sulphuric anhydride is, I believe, the only analogous reaction,—

$$PCl_5+SO_3=POCl_3+SO_2Cl_2.$$

The contrary substitution is often enough met with—is, in fact, one of the general reactions of phosphoric chloride. Thus we have :—

$$\left\{\begin{matrix} CH_3 \\ CCl_2 \\ CH_3 \end{matrix}\right. \text{ from } \left\{\begin{matrix} CH_3 \\ CO \\ CH_3 \end{matrix}\right. , \left\{\begin{matrix} CH_3 \\ CCl_3 \end{matrix}\right. \text{ from } \left\{\begin{matrix} CH_3 \\ COCl \end{matrix}\right. \text{ and } \left\{\begin{matrix} C_6H_5 \\ CCl_3 \end{matrix}\right. \text{ from } \left\{\begin{matrix} C_6H_5 \\ COCl \end{matrix}\right. .$$

It may be predicted that carbonic tetrabromine treated in the above

* Compt. Rend. lxix. p. 350.

† Rose gives 145° C. as the B.P.

manner will give rise to carbonic oxybromide, together with sulphurous anhydride and free bromine. I do not believe that a pyrosulphuric bromide will be formed.

Action of Sulphuric Anhydride on Chloroform.

The experiment was made under exactly the same conditions as the former one. The action set in immediately on addition of the chloroform, and was also accompanied by a rise in temperature and an evolution of gas, which gas proved to be pure carbonic oxide, and, as was ascertained by experiment, free from hydric chloride, whose formation by the reaction was not impossible.

The reaction was easily completed by aid of a gentle heat, and the liquid remaining in the flask was then subjected to distillation. After repeated rectification the greater part boiled constantly at 139°–140°, and was obtained as a colourless, mobile liquid, differing only in B.P. from the chloride obtained by the action of the anhydride on the tetrachloride.

The analysis yielded the following results:—

·1051 grm. gave ·138 grm. AgCl = 32·4 Cl.
·1150 grm. gave ·242 grm. $BaSO_4$ = 28·8 S.

		Found.	Calculated for SO_2HOCl.	Calculated for $S_2O_5Cl_2$.
S	=	28·8	27·46	29·7
Cl	=	32·4	30·47	33·1

It is evident from the above comparison of the numbers obtained with those calculated for each of the formulæ SO_2HOCl and $S_2O_5Cl_2$, that they lie midway between the two, and there is therefore no doubt that this liquid is a mixture of both.

Pyrosulphuric chloride and sulphuric chlorhydrate are therefore derived from sulphuric anhydride and chloroform, as is explained by the following equations,—

$$2SO_3 + CHCl_3 = CO + HCl + S_2O_5Cl_2.$$
$$SO_3 + HCl = SO_2HOCl.$$

There was a probability of formylic chloride, HCOCl, instead of the products of its decomposition, CO + HCl, being obtained by this reaction, as is evident from the equation,—

$$2SO_3 + CHCl_3 = HCOCl + S_2O_5Cl_2.$$

This is not the case, however, as is proved, 1, by the fact that carbonic oxide is evolved immediately on adding chloroform to the anhydride, which is also the case on reversing the experiment, and allowing sulphuric anhydride vapour to act on an excess of chloroform; 2, had it resulted it must have been detected on distillation, either as such, or if it became decomposed, by an evolution of CO and HCl; but the distillation only yielded the above-mentioned chloride, and was not accompanied by any further disen-

gagement of gas. If it be formed at all, which I certainly do not hold to be the case, it is immediately decomposed again in contact with the anhydride into carbonic oxide and sulphuric clorhydrate.

Action of Sulphuric Anhydride on Hexachlorbenzol.

This experiment was instituted in the hope of obtaining tetrachlorquinon, which, as is known, Graebe * succeeded in converting into hexachlorbenzol by the action of phosphoric chloride. The first step in the reaction would be the formation of the body C_6Cl_4O, which probably, by the intervention of another molecule of the anhydride, would then be converted into $C_6Cl_4\left\langle\begin{matrix}O\\|\\O\end{matrix}\right.$. I say probably, because there seems no tendency in the benzol series to form derivatives in which two of the monovalentic hydrogen atoms are replaced by a single divalentic atom.

However, no action whatever took place on heating the hexachlorbenzol in sealed tubes with sulphuric anhydride alone, or with addition of pyrosulphuric chloride as dissolvent, to a temperature over 200° C. The tubes burst frequently, but in all cases it was possible to separate out the hexachlorbenzol perfectly unaltered on addition of water. Possibly better results might be obtained with tri- or tetrachlorbenzol in which is still replaceable hydrogen.

Experiments were also made with chlorobenzol, benzotrichlorid, and dichlorhydrin, from the first two of which respectively benzoic aldehyde and chlorbenzol might have been formed,—a view which was favoured by Oppenheim's † having obtained benzoic aldehyde by the action of concentrated sulphuric acid on chlorobenzol and after-treatment with water.

Dichlorhydrin was perfectly carbonized by the action of the anhydride, HCl and SO_2 being evolved, and the other two chlorides were converted, also with evolution of HCl, into that peculiar resin-like substance which is a so characteristic product in many reactions with these bodies.

There seems, therefore, to be no doubt, as was indeed probable, that the substitution of Cl_2 by O can only be effected in such compounds as are capable of resisting the action of the pyrosulphuric chloride formed thereby, and that consequently it is only attainable in its purity in those cases where the whole or greater part of the hydrogen is replaced by chlorine.

Action of Sulphuric Anhydride on Perchlorinated Chloride of Ethylene.

On bringing these two bodies together and slightly warming, the reaction soon set in, the latter melting; a disengagement of gas also took place, an examination of which proved it to be a mixture of carbonic oxychloride and sulphurous anhydride. On continuing the application of heat, the contents of the flask gradually became liquefied, and on subjecting afterwards to distillation, the liquid commenced to boil at about 60° C.; the thermometer

* Ann. Ch. Pharm. cxlvi. p. 12.

† Berichte d. Deutschen. Chem. Ges. ii. p. 213.

rose, however, rapidly, and but a small quantity had passed over under 130° C. The portion boiling from 130° upwards consisted entirely of pyrosulphuric chloride.

The more volatile portion was now treated with ice-cold water, in order to free it as much as possible from pyrosulphuric chloride, quickly separated from the water, dried over calcic chloride, and distilled, when it was found to boil between 100° and 140°. With the small quantity at my disposal (at the most 5 grm.) it was impossible to attempt to purify it by rectification; I therefore, as I suspected it to be trichloracetylic chloride (chloraldehyde), attempted to convert it into the corresponding ether; but unfortunately, through an accident, lost it all in so doing.

The following are the observed properties of the above product:—In contact with water it became gradually decomposed, hydric chloride being formed; it acted very violently on alcohol, also on aqueous ammonia, and the solution here obtained yielded on evaporation long prismatic needles of ammonic trichloracetate (?).

Had the reaction taken a normal course, the formation of chloraldehyde was to be expected, according to the equation,—

$$C_2Cl_6 + 2SO_3 = C_2Cl_4O + S_2O_5Cl_2.$$

The properties of the substance obtained also agree in so far with those given for chloraldehyde, and I have therefore little doubt but that it has really resulted; the yield is, however, very small, in consequence of secondary reactions taking place.

Unfortunately I am not in possession of the necessary material to repeat the experiment and place its formation beyond all doubt.

Thus far have I studied the substitution of Cl_2 by O up to the present moment, but I intend prosecuting my researches in this direction, which seems to me to present a number of interesting points. It is a question whether the compounds C_2Cl_4 and C_2HCl_3, isologous with CCl_4 and $CHCl_3$, will give rise to C_2OCl_2 and C_2O, isologous respectively with $COCl_2$ and CO; the formation of such bodies, and of acids of the general formula $\left\{\begin{array}{l}COHO\\ C_n\\ COHO\end{array}\right.$, seems theoretically neither impossible nor improbable.

Will the action on chlorpicrin be analogous with that on carbonic tetrachloride or on chloroform?

The production of a silicic oxychloride from silicic chloride by the same means is also to be expected.

Action of Sulphuric Anhydride on Carbonic Disulphide.

The extraordinary mobility of the one oxygen atom in sulphuric anhydride, evident in all the preceding experiments, gave rise to the hope that in sulphur compounds a substitution of sulphur by oxygen might be realized, and thus the interesting gas carbonic oxysulphide be directly obtainable from carbonic disulphide.

According to Genther*, carbonic disulphide and sulphuric anhydride are without action on one another, a simple solution of the anhydride taking place. Notwithstanding this I thought it advisable to repeat the experiment, and have by so doing found that a reaction does take place exactly in the sense I had expected.

If equivalents of the two are mixed together, action sets in, even at common temperatures, after a short time, but at once on warming the mixture, and is accompanied by a continuous evolution of gas, which, by alternate heating or cooling, can be regulated at will.

The escaping gas was first passed through a wash-bottle containing water, and then, to free it from carbonic-disulphide vapour, through tubes filled with pieces of unvulcanized caoutchouc placed in a freezing-mixture of ice and salt †, and, finally, to remove the last traces of sulphurous anhydride, over plumbic peroxide. Thus purified, it possessed all the properties described by Than ‡ and others as characteristic of carbonic oxysulphide, and its identity therewith was further proved by a gasometric analysis, which gave the following data :—

	Vol.	Pressure.	Temp. C.	Vol. at 0° C. and 1 min. press.
Gas employed, dry ..,..	92·1	·2551	13·8	22·33
After addition of oxygen	290·7	·4566	13·8	126·35
After explosion	276·9	·4377	13·8	115·40

Gas employed .... 22·33
Contraction observed .... 10·95

A contraction to one-half the original volume is required on the assumption that 1 vol. $COS+3$ vol. $O=1$ vol. CO_2+1 vol. SO_2; and this, as is evident, is nearly the case.

Ammonic sulphocarbamate, formed by the direct union of carbonic oxysulphide with ammonia, and first described by Berthelot, I found to be produced in quantity on passing the gas and ammonia together into absolute alcohol, it separating out from the concentrated solution, on standing, in long prismatic needles.

Than, by decomposing carbonic oxysulphide over mercury in the one limb of a V tube provided with platinum wires, found that a separation into an equal volume of carbonic oxide and sulphur, which appears as a thick cloud on the tube every time on passing the spark, took place. On repeating his experiment with my gas, I found exactly the same to be the case.

The residue in the flask, after the evolution of gas had ceased, was obtained, after washing with water, as a yellow friable mass, consisting entirely of sulphur.

* Jahresbericht; J. Chemie, 1858, p. 85.

† Bender's modification of Than's original plan, Ann. Ch. Pharm.

‡ Ann. Ch. Pharm. Supp. Band v. p. 236.

The following simple equation, therefore, explains the reaction which has taken place :—

$$CS_2 + SO_3 = COS + SO_2 + S.$$

Action of Sulphuric Anhydride on Phosphorous Chloride.

The ease with which the foregoing reaction had taken place led me to try the action of the anhydride on a compound which, possessing latent affinities and a predisposition to combine with oxygen, it was to be expected would cause the separation of the "extraradical" oxygen atom, as I term the atom which is signalized by its great mobility. The compound chosen was phosphorous chloride.

On adding this to the anhydride, which must be in a flask surrounded by ice, a violent reaction takes place, attended by a copious evolution of sulphurous anhydride. No further action is observable after equal equivalents have been employed; and on distilling the resulting liquid and fractioning two or three times, two products are obtained, the one boiling from 110° to 114°, which, from all its properties, is undoubtedly phosphoric oxychloride.

The reaction has therefore partly taken place as was expected, and according to the equation—

$$PCl_3 + SO_3 = POCl_3 + SO_2.$$

The second product, which is obtained in varying quantity, according as more or less anhydride is employed, the more being formed, the greater the proportion of the latter *, boils at the first distillation between 120° and 170°, and cannot be obtained of constant B.P., even by repeated rectification, by each of which it only suffers further decomposition, a thick varnish-like residue remaining every time. This product contains phosphorus, chlorine, and sulphur.

H. Rose, who also studied this reaction, though without observing the formation either of phosphoric oxychloride or of sulphurous anhydride, which latter he only remarked was given off on subsequent distillation, also describes this second product; he ascribes to it, however, an exceedingly complicated formula.

It is very possible that a further substitution of chlorine by oxygen has taken place, as explained by the equation—

$$POCl_3 + 2SO_3 = PO_2Cl + S_2O_5Cl_2;$$

and this compound can be viewed as metaphosphoric chloride, which it is to be expected would be of a very unstable nature. The above product is then, on this supposition, a mixture of two chlorides, to decide which it will be first necessary to institute experiments with pure phosphoric oxy-

* It is probable that only phosphoric oxychloride would be formed were the experiment reversed, and the anhydride allowed to act on an excess of the phosphorous chloride.

and sulphochlorides; it may then be possible to separate the products of the reaction by means of distillation under reduced pressure.

The formation of phosphoric oxychloride in this reaction can be considered as the result of the simple addition of oxygen to phosphorous chloride, the triatomic phosphorus becoming pentatomic*.

On the Properties and several Reactions of Pyrosulphuric Chloride.

Pyrosulphuric chloride was first obtained by H. Rose by the action of sulphuric anhydride on chloride of sulphur S_2Cl_2, and later on by simple distillation of chloride of sulphur saturated with chlorine with Nordhausen sulphuric acid.

Rosenstiehl prepared it by heating sodic chloride with sulphuric anhydride,—

$$3SO_3 + 2NaCl = S_2O_5Cl_2 + Na_2SO_4.$$

According to him, acetylic chloride is formed by heating it with sodic acetate, and chlorochromic acid by its action on potassic chromate, sodic and potassic pyrosulphates being formed at the same time, as he proved by analysis,—

$$K_2CrO_4 + S_2O_5Cl_2 = CrO_2Cl_2 + S_2O_7K_2.$$

On passing its vapour through a tube heated to dull redness, I have found the following decomposition to take place:—During the whole operation chlorine and sulphurous anhydride escaped, and in the receiver, which was kept cold by ice, sulphuric anhydride and a liquid layer were condensed. The latter yielded on distillation two products, the one boiling below 100° and the other consisting of the pyrosulphuric chloride which had escaped decomposition. The first portion was found to be sulphuric anhydride contaminated with traces of pyrosulphuric chloride; for on passing a stream of dry carbonic anhydride through it the whole solidified. The decomposition is therefore expressed by the equation—

$$S_2O_5Cl_2 = SO_3 + SO_2 + Cl_2.$$

The result of a vapour-density determination, according to Dumas's method, also speaks for the above decomposition. The following are the observations recorded:—

	mm.	
Weight of globe+dry air at 14°·7 and 758·9		50·513
Weight of globe+vapour at 202° and 758·6		50·732
Capacity of globe		249 cub. centim.
Residual air		1 „ „

which gave a specific gravity=5·06.

The calculated number for $S_2O_5Cl_2$=2 vol. is 14·89; a splitting up into $SO_3+SO_2Cl_2$ being admitted, it is 7·44; and if the decomposition go further,

* Experiments were also made to oxidize carbonic oxide; but the anhydride was found to be without action on it up to a temperature of 200°. I have no doubt, however, that the employment of a somewhat higher temperature will effect the combination.

and SO_3, SO_2, and Cl_2 are the final products obtained on heating, 4·96 is the theoretical number.

The experimental number, however, lies between the two latter, and it is therefore to be supposed that already, at a temperature of 200°, the dissociation was almost complete.

I intend making a series of determinations at varying temperatures and pressures, especially as H. Rose and Rosenstiehl have obtained numbers very different from mine; the former gives 8·96 as the mean of 5 concordant experiments all made at about 200°, and the latter 7·52, the temperature at which the determination was made not being given in this case.

The action of phosphoric chloride on the chloride is somewhat remarkable. On bringing the two together a violent action takes place, and sulphurous anhydride and chlorine escape; after adding slightly more than 1 equivalent of the chloride and 1 equivalent phosphoric chloride, and warming a short time, the latter had entirely disappeared. On distilling but little passed over below 130°, the greatest portion between 130° and 140°, and from 140° to 150° about one-third of the whole. Under all circumstances the formation of phosphoric oxychloride was to be expected, and it was therefore remarkable that so little had distilled over within the limits of its boiling-point. The analysis of the three fractions, however, has shown that they are all mixtures of $POCl_3$ and $S_2O_5Cl_2$ in varying proportions.

From fraction 1 (130°),—

·1655 grm. gave ·409 grm. $AgCl$ = 61·1 Cl.
·4027 grm. gave ·1395 grm. $BaSO_4$ = 4·75 S.
·165 grm. gave ·0777 grm. $Mg_2P_2O_7$ = 13·15 P.

The amount of phosphorus shown by analysis corresponds to 65·1 $POCl_3$; but the amount of chlorine remaining after deduction of that required by the phosphorus and the sulphur are not in the proportion required either by sulphuric chloride or by pyrosulphuric chloride: it is therefore probable that a mixture of both is present.

The second fraction still contained 20·8 $POCl_3$.
The third „ „ 12·5 $POCl_3$.

The decomposition is accordingly expressed by the equation—

$$S_2O_5Cl_2 + PCl_5 = POCl_3 + 2SO_2 + Cl_2.$$

Sulphuric chloride, which it at first seemed probable would be the sole product beside phosphoric oxychloride,—

$$\left\{\begin{matrix} SO_2Cl \\ O \\ SO_2Cl \end{matrix}\right. + PCl_5 = POCl_3 + 2SO_2Cl_2,$$

has, if at all, only been formed in very small quantity, it seems.

As to the constitution of the body $S_2O_5Cl_2$, all seems to speak for its

being the chloride of pyrosulphuric acid (so-called Nordhausen), to which it bears the same relation as sulphuric chloride to sulphuric acid,—

$$SO_2\left\{\begin{matrix}HO\\HO\end{matrix}\right., \quad SO_2\left\{\begin{matrix}Cl\\Cl\end{matrix}\right. \cdot \quad \left\{\begin{matrix}SO_2HO\\O\\SO_2HO\end{matrix}\right., \quad \left\{\begin{matrix}SO_2Cl\\O\\SO_2Cl\end{matrix}\right..$$

The old view of considering Nordhausen sulphuric acid as merely a solution of sulphuric anhydride in ordinary sulphuric acid has now probably but few partisans among chemists, it being looked upon as a true chemical compound, although of a very unstable nature, for the reason that on one side definite (sodic and potassic) salts of it are known, and on the other it presents analogies with certain chromium and phosphorus compounds, in which groups we are acquainted with the following series:—

$$\left\{\begin{matrix}SO_2HO\\O\\SO_2HO\end{matrix}\right., \quad \left\{\begin{matrix}SO_2Cl\\O\\SO_2Cl\end{matrix}\right., \quad \left\{\begin{matrix}CrO_2HO\\O\\SO_2HO\end{matrix}\right., \quad \left\{\begin{matrix}CrO_2HO\\O\\CrO_2HO\end{matrix}\right., \quad \left\{\begin{matrix}CrO_2Cl\\O\\CrO_2HO\end{matrix}\right., \quad \left\{\begin{matrix}PO(HO)_2\\O\\PO(HO)_2\end{matrix}\right..$$

The peculiar crystalline compound which plays an important part in the sulphuric-acid manufacture is also very possibly a derivative of pyrosulphuric acid, thus:— $\left\{\begin{matrix}SO_2NO_2{}^{*}\\O\\SO_2NO_2\end{matrix}\right.$; and I hope to be able to prove this by the action of sulphuric anhydride on $CH(NO_2)_3$ or $C(NO_2)_4$.

IV. "On some of the more important Physiological Changes induced in the Human Economy by change of Climate, as from Temperate to Tropical, and the reverse." By Alexander Rattray, M.D. (Edinb.), Surgeon R.N., H.M.S. 'Bristol.' Communicated by George Busk, F.R.S. Received May 3, 1870.

Besides its obvious bearing on the long-vexed and still unsettled question of the unity of the human species, and on the closely related one of acclimatization, the present inquiry is of great medical importance. Tropical pathology, whether of native or foreign races, cannot be fairly studied until we thoroughly know its physiology; nor can we recognize and properly estimate disturbed action of organs till we understand their healthy functions. Otherwise natural phenomena may be mistaken for symptoms of sickness. Many so-called tropical diseases are merely exaggerations of the ordinary effects of climate, physiological merged into pathological phenomena; a knowledge of the one is the first step to an accurate acquaintance with and philosophical method of treating or preventing the other.

No inconsiderable part of our present knowledge of the vital phenomena

* Frankland, Journ. Chem. Soc. xix. p. 392.

induced in the human economy in passing from cold to warm regions, or the reverse, is derived from experiments carried out in artificially made or seldom encountered climates. By hot-air chambers we illustrate the effects of augmented temperature on the respiration, pulse, &c., and by the rarefied atmosphere of mountain-tops show how diminished density acts. Neither of these, however, are fair examples of natural climates. Thus the former, dry and warm, is unlike the tropics, with its triple combination of increased heat, rarefied air, and excessive moisture; as the latter, dry and chilly, is dissimilar from the usual surface-climates of extra-tropical latitudes, which conjoin cold, condensation, and moisture. The dry and warm, or dry and cold, climates which occur in nature are usually local and limited. Nor do such abrupt and temporary exposures to heat and cold have any parallel in ordinary life, or are they likely to induce results similar to the comparatively slow transition involved in an ordinary change of climate; and though the rarefied air of heated chambers will decrease, while that of great altitudes will accelerate respiration, the former will do so less, and the latter more, than they otherwise would, from the skin, and especially the more slowly acting liver and kidneys, being unable at once to increase their action so as to aid the lungs in eliminating carbon. The functional changes so induced cannot therefore be taken as a fair criterion of what occurs in nature; and as mere approximates to truth, such observations, though interesting, are evidently wanting in practical importance.

I. *The Influence of Tropical Climates on the Respiration.*

It has been ascertained, by the experiments already alluded to, that the respirations are diminished in frequency in warm and increased in cold air; but we do not yet know what happens in the tropics, where great heat, rarity of air, and moisture are conjoined. Nor has it yet been shown whether the quantity of air respired is greater in the tropics or less. It is obviously necessary to ascertain both before we can decide whether the total quantity of air and oxygen respired, and the amount of carbonic acid and watery vapour exhaled, be different or not.

The following experiment will show that the *capacity of the chest* for air is materially affected by tropical climates:—

Table I.—To show the effect of tropical weather on the capacity of the chest, as indicated by the spirometer.

		Age.	Height.	1. Temp. zone near England, at sea, June 23, 1869. Therm. 65° F. Hygr. 2½° F.	2. Tropics, lat. 13° N., at sea in equatorial doldrums, July 12, 1869. Therm. 78° F. Hygr. 4° F.	3. Tropics, lat. 70° S., at sea in equatorial doldrums, Aug. 20, 1869. Therm. 83° F. Hygr. 4° F.	4. Temp. zone, near England, at sea, Sept. 14, 1869, Therm. 65° F. Hygr. 1½° F.	5. Temp. zone, England, Plymouth, Feb. 10, 1870. Therm. 42° F. Hygr. 3° F.
			ft. in.	cub. ins.	cub. ins.	cub. ins.	cub. ins.	cub. ins.
(adult).	Wright *	25	5 11¼	288	315	324	300	288
	Scaulan *	26	5 5½	222	255	254	240	236
	Rodd *	22	5 10¼	303	327	327	300	288
	Bushell *	29	5 7	240	261	270	234	...
(adult).	Maclean *	28	5 11	321	350	360	330	318
	Knott *	28	5 7¼	219	243	246	234	...
	Rattray *	38	5 7¼	219	246	258	234	224
	Norcott *	26	5 7½	216	240	246	231	215
	Carr *	25	6 0¼	256	294	303	258	255
	Browne *	26	5 11	285	316	318	276	274
	Walter *	27½	5 8¾	267	285	300	270	252
	Symons *	34	5 9	237	237	243	216	213
	Turner	30	5 11	222	230	...	...	...
	Silver	31	5 6½	...	240	255	238	228
	Yesting	22	5 9¼	...	243	255	247	...
	Maude	22	6 3	...	...	288	252	...
	Haynes	23	5 8¼	...	...	270	231	...
	Huyghue	50	5 4	...	198	198	180	...
(cadets).	Fisher	16⅓	...	...	...	152	150	...
	Spencer	16½	...	...	...	162	140	...
	Malan	16¾	...	...	...	153	149	...
	Collins	16¾	...	...	...	180	165	...
	Simeon	16½	...	...	...	171	156	...
	Lees	16½	...	...	...	153	180	...
	verage capacity of the 12 marked *	...	...	256·083	280·75	287·416	260·25	253·727
	" gain (by heat)	...	...	...	24·833	6·5833	...	...
					31·4163			
	" loss (by cold)	...	...	...	...	...	26·333	6·523
	rcentage of gain in the tropics	...	...		12·24		...	...

This gives the results of observations with the spirometer on 24 healthy individuals, made during the voyage from England (lat. 51° N.) to Bahia (lat. 11° S.) and back. Four of these were strong, full-chested, adult seamen, fourteen healthy adult officers, and six young growing lads. In the twelve marked by an * the experiments were carried on throughout. Column 1 shows the capacity in the temperate climate of England during summer (June, average temperature 65° F., shade). The average of these twelve cases gives 256 cubic inches. Column 2 gives the capacity, nineteen days afterwards, in the equatorial doldrums and greatest heat (78° F., shade) of the outward voyage, and shows that this had increased to 280 cubic inches,

equal to an average gain of 24 cubic inches per man. Column 3 gives the capacity, thirty-eight days later, in the equatorial doldrums and highest temperature of the return voyage (83° F., shade), and shows that in ten of the twelve cases this increase was still further augmented by an average of $6\frac{1}{2}$ cubic inches from prolonged tropical exposure. It would be interesting to know the limits of this increase, and whether it is, as likely, permanent. The other two cases remained stationary. The total average in the twelve cases, during this fifty days' residence in the tropics, was 31 cubic inches (12·24 per cent.). In order to test whether this was due to climatic causes, or resulted from custom in using the instrument, the same cases were again tested about three weeks afterwards, on return to England in September, when it was found (column 4) that the capacity of the chest for air had again decreased in every case by an average of 26 cubic inches. Although the temperatures were identical (65° F., shade) on quitting and returning to England, the time was apparently too short to allow the capacity of the lungs to resume their first standard average of 256 cubic inches, being still at 260 cubic inches. But this result followed a subsequent reduction of temperature to 42° F. (shade) in February 1870, when the average capacity of eleven of the same cases was found to be 253 cubic inches, that is, 3 cubic inches below the first trial. This fact, however, goes far to prove that there is a limit to this reduction in the pneumatic capacity of the chest in health, which was probably nearly reached. The results among the other adults were identical, and showed that the capacity of the chest for air is considerably greater in the tropics than in temperate climates. This was noticeable in five of the six cadets. During the three weeks of the return voyage between the tropics and England, when from faulty diet their growth was nearly or altogether stopped, the capacity of the chest decreased considerably, when it might have been reasonably expected that the diminished capacity from climatic causes would have been more than counterbalanced by expansion from growth, usually very rapid at that age. This actually occurred in the sixth (Lees), a tall growing youth, in whom an increase of 27 cubic inches showed that his chest had enlarged considerably. The greatest increase in the pneumatic capacity of the chest from this sojourn in the tropics, of the twenty-four cases here recorded, amounted to 39 cubic inches, and the lowest to 21.

This and the subsequent experiments were made at sea, and in equable super-oceanic temperatures *. It will be interesting to know whether the same laws prevail in insular, littoral, and especially continental climates, possessing a higher day, lower night, and greater diurnal and annual range of the thermometer.

* The daily mean range of temperature, Fahrenheit, was:—

	Highest.	Lowest.	Range.
Extra-tropical	9°	1°	3°·63
Tropical..............................	$6\frac{1}{4}$°	1°	2°·9

the average for the entire voyage being 3°·2 F.

The following Table will show that the same law extends to the negro :—

TABLE II.—To show the effect of climate on the capacity of the chest in the black races.

Age &c.	Race.	Aug. 13, 1869. Lat. 8° S. Temp. 79° F.	Aug. 25, 1869. Lat. 16½° N. Temp. 78° F.	Feb. 12, 1870. England. Temp. 32° F.
		cub. ins.	cub. ins.	cub. ins.
Benjamin Campbell, æt. 21 Height 5 ft. 5¼ ins.	Negro of Sierra Leone	210	207	185
John Campbell, æt. 20 ... Height 5 ft. 4 ins.	Negro of Sierra Leone	174	166	156
John Williams, æt. 31 ... Height 5 ft. 4 ins.	Half caste	176	162	...

In the first case, a pure black, the capacity of whose chest amounted in the tropics to 210 cubic inches, it became reduced in the winter of England to 185 cubic inches. In the second, also a pure black, it fell from 174 in the tropics to 156 during the English winter. The 2nd column records the results of the exit from the doldrums into the trades,—the lungs of all races, and particularly those of the black tribes, being then supersensitive to even slight reductions of temperature.

That a similar decrease occurs in disease was shown in several invalids from Bahia with chest affections :—

TABLE III.—To show the influence of climate on the capacity of the chest for air in pulmonary disease.

Name &c.	Aug. 20, 1869, 3 P.M. Lat. 7° N.; calms of equator under sun; very sultry. Temp. 83° F.	Aug. 25, 1869, 3 P.M. Lat. 16½° N.; cool N.E. trades. Temp. 78° F.
	cub. ins.	cub. ins.
Abbott, æt. 22; phthisis, early 2nd stage ...	135	120
Cribbes, æt. 25; phthisis, 1st stage	148	140
Hughes, æt. 17; phthisis, 2nd stage	147	135
Ratford, æt. 25; phthisis, 2nd stage.........	96	84

Here, in all four cases, there was a decrease from 8 to 15 cubic inches, even in so brief an interval as five days, caused by the cool dry north-east trade-winds, suddenly met with after calm, moist, sultry weather near the equator. The period was evidently too brief, and the disease not sufficiently active in any of these cases, for this to have resulted from the formation or increase of cavities in the lungs; and it can only be ascribed to the law that the pneumatic capacity of the chest varies with temperature, increasing in tropical, and diminishing in temperate and cold climates.

A knowledge of this law is evidently of practical application in preventing mistakes in the spirometric diagnosis of certain lung-diseases. Thus the capacity of the chest of an individual debilitated by residence in the tropics, and weak-chested, but with no active lung-disease, being, say, 250

or 270 cubic inches, he might be supposed to have contracted incipient phthisis on reaching England in winter labouring under catarrh, with the pneumatic capacity of his chest reduced by from 25 to 35 cubic inches. On the other hand, a patient actually in incipient phthisis might be erroneously considered to have permanently recovered by a trip to the tropics having raised the capacity of his lungs for air by a similar amount, *i. e.* almost or actually up to the normal standard for his age and height in a temperate climate. A similar mistake might be made, especially if the instrument is carelessly used, in the same climate, *e. g.* that of England, at different seasons of the year, such as the height of summer and depth of winter, when a considerable difference in the capacity of the lungs for air must not be taken as an index of disease *. The greater the range between the summer and winter temperatures, the more marked will be the difference in the spirometric indications.

But it is not by deep inspirations like these that ordinary respiration is carried on; and it is important to ascertain whether the air inspired in each ordinary breath undergoes a similar increase and decrease according to climate. The difficulty in limiting and measuring the small quantity of air expired during our usual faint breathings makes this a far more delicate and difficult experiment than the preceding. From analogy, however, we may infer that it does vary; and the following will go far to prove it. My ordinary respirations ranged from 4 to 8, and averaged 6 cubic inches in a temperature of 44° F. (shade) during the winter of England. At Lisbon, during an average temperature of 65° F. (shade), they ranged from 5 to 13, and averaged 9 cubic inches†. Unfortunately while in the tropics I had not the proper apparatus to ascertain how much they increase during the far greater temperature of equatorial regions. As these results therefore only prove the existence of an increase, and do not show its extent, it will be necessary, as it is doubtless correct, to calculate the minor from the major increase.

The following Table will show the negative effect of period of the day on the capacity of the chest for air in the tropics.

TABLE IV.—The capacity of the chest for air, as influenced by period of the day in the tropics.

	In the Tropics, as a whole, 51 days.	Doldrums, or warmest part of the Tropics; average temperature 78¼° F.
	cubic inches.	cubic inches.
9 A.M.	244·3	243
3 P.M.	244·42	242·57
9 P.M.	245·06	243

* Table I. cols. 4 & 5.

† These amounts are all small, because those taken were the short ones which immediately follow the deep inspiration in which ordinary breathing usually culminates every twentieth or thirtieth inspiration.

Column 1 gives the average of the entire voyage, during which the equator was crossed and again recrossed. Here the results, morning, afternoon, and evening, both in the tropics generally and in its warmest part, the equatorial doldrums, are so very nearly identical that we may conclude that period of the day has very little influence on the capacity of the chest for air, or the power of taking deep inspirations.

Closely allied to the foregoing is the influence of tropical climates on the *frequency* of the respirations. In heated air-chambers respiration becomes less in man*. Vierordt and Ludwig also found that the respirations are lessened in number in animals subjected to heat†. Does this occur in natural tropical climates? We might infer that as the volume inspired increases, so does the number of respirations. But fact (Table V.) proves the reverse, and shows that, as in hot artificial climates, they are diminished in number.

Table V.—To show how the frequency of the respiration is affected by tropical climate.

		Average temperature (shade).	Highest number of respirations.	Lowest number of respirations.	Average number of respirations.
		Fahr.			
Temperate Zone.	England, in summer (June)	62°	18	13·5	15·68
	„ in winter (8 Feb.)	42·25	17·5	15	16·5
Tropics ...	Equatorial doldrums (outward voyage)	78·74	14·5	11	12·74
	Equatorial doldrums (return voyage)	78·6	15	12	13·74

This Table is compiled chiefly from the daily results of a three months' voyage to Bahia and back, the observations being taken in the standing posture at 9 A.M., 3 P.M., and 9 P.M.; the averages are for a week. Thus in the summer of England, with an average temperature of 62° F., the average number of respirations per minute was $15\frac{2}{3}$; whereas in the doldrums, or warmest part of the tropics, during the outward voyage, with a temperature of $78\frac{3}{4}$° F., the average was only $12\frac{3}{4}$, and on the return voyage $13\frac{2}{3}$,—a decided decrease. In the winter of England (February), with a temperature of 42° F., the average had increased to $16\frac{1}{2}$, and at freezing-point (32° F.) to $17\frac{1}{2}$. With those results similar experiments carried out at my request by a colleague‡ coincided, and showed that though, as in temperate latitudes, the activity of the respiration differs in different individuals, they are diminished in number in the tropics. Thus, while his respirations averaged 16·077 per minute in the temperate climate of England (January, average temperature 32° to 44° F.), they average

* Parkes, 'Practical Hygiene'; Hooper, 'Physicians' Vade Mecum,' &c.

† Parkes, *ibid.*

‡ Mr. T H. Knott, Assistant-Surgeon.

15·4 in the West Indies winter season (temperature 79° to 83°, shade); while, again, the sick-bay man's were 17·3 in England during winter, and 16 in the West Indies, my own were 17·5 in England, and 16·2 in the West Indies. The same may be noticed in an artificial climate of a heated room in England. Thus, while my respirations out of doors in a temperature of 30° F. were 17·5 per minute, they rose to 15·8 shortly afterwards by simply standing before a fire in a temperature of 57° F.

From these data we find that the increased quantity of air and oxygen inspired in the tropics does not make up for the diminished number of respirations in supplying the same amount of air and oxygen for blood-purification as in cold climates, though doubtless a requisite quantity is inspired, less probably being needed there, as will presently appear, to carry on the vital processes. Thus, taking the average number of respirations in the temperate climate of England at 17 (Table V.), and the quantity of air inspired each breath at 15 cubic inches, this would give 255 cubic inches per minute. Now if the chest (Table I.) gains in capacity by an average of 31 cubic inches (=12·24 per cent.) in the tropics, the gain on each ordinary respiration would be 1·836 cubic inch, thus raising the quantity inspired each breath to 16·836 cubic inches. The average number of respirations being taken at 14, this would give 235·704 cubic inches per minute, *i. e.* less by 19 cubic inches (8 per cent.) than in temperate climates; equal to 1157·760 cubic inches per hour, or 27786·24 cubic inches per day. Thus—

	Cubic inches in each inspiration.		Number of respirations.		Cubic inches respired per minute.
England	15	×	17	=	255
Tropics	16·836	×	14	=	235·704

Difference in favour of a temperate climate 19·296 cub.in. (=7·567 p.c.)

This decrease of 7·567 per cent. in the quantity of air respired daily diminishes the quantity of carbon which the lungs in ordinary circumstances can throw off in the tropics by 0·7567 oz., or rather more than $\frac{3}{4}$ oz.; 10 ozs. being taken as the average amount thrown off in temperate climates*, will give 9·243 oz. as the amount for the tropics. But as tropic air contains less oxygen for a given bulk than air of colder latitudes, according to Dalton and Gay Lussac's law of expansion of gases by heat, the decarbonizing capabilites of the lungs in tropical latitudes will evidently be still further curtailed, and the amount of carbon they can throw off considerably decreased. Air increases by $\frac{1}{480}$ its volume for every Fahrenheit degree of heat; and the difference between the temperatures in which these experiments were carried on being 18° F. (65° and 83° F.), if we reduce the amount inspired in the tropics by a $\frac{18}{480}$ part, this will give its equivalent bulk in the temperate zone. Thus,—

* Hooper, 'Physicians' Vade Mecum.' Mean of three estimates by Lavoisier and Seguin, Davy, and Allen and Pepys.

$$\frac{235 \cdot 704}{(1+\frac{1}{480}\times 18)}=\frac{235 \cdot 704}{1 \cdot 0375}=227 \cdot 1846 \text{ cubic inches,}$$

which is equal to a decrease of 8·5194 cubic inches or 3·614 per cent.; then 225—227·1846 cubic inches gives 27·8154 cubic inches per minute, or 1668·924 cubic inches per hour, or 40054·176 cubic inches per day= 10·907 per cent. as the grand total difference in favour of a temperate climate, after deducting the real decrease in volume, and correcting for expansion by heat. By again reducing the 9·243 oz. of carbon by 3·614 per cent., or 0·33409 oz., we get 8·909 oz. as the total amount which the lungs throw off in the tropics, the difference between the tropical and extra-tropical quantities being 1·1028 oz. This result in the human species accords with Vierordt's observation on the lower animals, viz. that less carbonic acid and presumably less water are eliminated when they are subjected to heat*. This, then, is probably the rule, but in exceptional cases, from idiosyncrasy, accelerated or forced respiration may make the quantity of carbon which the lungs can exhale in temperate and tropical regions more nearly alike.

Thus the relative proportions of carbon thrown off by the several depurating organs in the tropics differs from those of temperate latitudes. Hooper† gives the latter as:—lungs 10½ oz., skin ¼ oz., fæces ½ oz., urine ½ oz., total 11¾ oz.; we have found that in the tropics the amount eliminated by the lungs is reduced by 1¼ oz. Under judicious hygienic, and especially dietetic management, that for the skin, liver, and urine may not be materially altered from these figures; but otherwise it is probably on the skin that the greatest share of its burden is thrown. We may presume that the liver-work and bile are increased, though perhaps only slightly, in the tropics, although this has not yet been actually proved. The kidneys may assist to a greater extent; for though the urine is diminished in quantity in the tropics, the relative amount of solids is not correspondingly altered. Much of the latter may be carbon; for though the urea eliminated by the kidneys in the tropics is diminished‡, the quantity of uric acid, which contains thrice the amount of carbon, has not yet been ascertained.

The mutual bearing of these two closely related but very opposite results, viz. an increase in the capacity of the chest for air in the tropics, with a decrease in the number of the respirations, is an interesting if not important study. Hasty inference might lead us to attempt to explain the former by a greater volume of the rarefied and moisture-laden air of the tropics being required to supply the system with the requisite quantity of oxygen. But there are several objections to this theory, *e. g.*:—

1. Nature might do this in health as she often does in disease, such as phthisis, pneumonia, &c., by increasing the frequency of the respirations,

* Parkes, 'Practical Hygiene,' 2nd edit.

† Physicians' Vade Mecum.

‡ Parkes, 'Practical Hygiene,' 2nd edit. p. 448.

whereas here they are diminished in number. To increase the one and diminish the other, the first implying augmented, and the second decreased energy in the respiratory muscles, implies an anomaly which it is unnecessary to accept when the phenomena can be otherwise and better explained.

2. External measurement, moreover, shows that there is actual increase in the capacity of the chest from its expansion by increased action by its inspiratory muscles. Thus in three adults in which the chest was measured by the spirometer and tape in the winter of England (average temperature 30°–32° F.), and again at Lisbon (average temperature 60° F.), though in all three there was a decided increase in the capacity of the lungs for air to an average of 11 cubic inches, there was no perceptible increase in the circumference of the chest. In one case the latter had apparently decreased by one inch, as if from loss of muscular tone.

3. It is more than doubtful if the system actually requires an increased quantity of air or as much oxygen to carry on the vital processes in the tropics as in cold climates, and the above-mentioned data prove that it actually gets less of both. Less oxygen is required in the high temperatures of low latitudes, because the tissues generally decay less rapidly*. Owing to diminished mental and bodily exertion of the two which make up the bulk of the body, viz. the muscular and nervous, less is required for the metamorphosis of waste particles. Where a diminished necessity and desire for food lessen the ingesta, and judicious selection reduces the amount of carbon this contains, less oxygen is required for direct combination with the elements of the food to generate heat. Moreover it is certain that the increased absorption of oxygen by the functionally excited skin in the tropics, where it acts vicariously for the lungs as a respiratory organ, lessens materially the amount required by the lungs. Furthermore, the skin is aided in relieving the lungs in the decarbonizing process by the functionally excited liver and perhaps kidneys, which throw off the carbon in forms which do not require much oxygen for their formation, viz. as bile and uric acid.

The true explanation appears to be that there is really no actual increase in the capacity or size of the chest and enclosed lungs, but only an alteration in the relative proportion of blood and air contained in the latter†.

* Carpenter and others doubt this, and believe that they decay more rapidly; in other words, that when used there is a greater waste for the same amount of work. The sum total of this, however, for all the tissues will probably not be greater than the reduction due to the diminished amount of work which the principal tissues have to perform.

† With regard to the observation that the increased capacity for air in the chest is "only an alteration in the relative proportion of blood and air in the lungs," there must be a reservation made in favour of a statement (though it still, perhaps, has to be proved) that the bases of the lungs have a reserved capacity for air, any enlargement of which would, of course, have to be measured, not by expansion of the costal walls, but by protuberance of the abdomen, through encroachment on its cavity by

The bulk of the lungs remaining the same in the tropics as in colder latitudes, or being even somewhat diminished from their comparative abeyance as excreting and heat-generating organs, the blood, diverted to the functionally excited and congested skin and liver, permits the ingress of a larger quantity of air into the pulmonary air-cells and tubes; whereas in colder latitudes this is reversed, the lungs being more and the skin and liver less active; the blood drawn from the latter to the former diminishes the calibre of the air-vesicles and bronchi, lessening the quantity of contained air, as happens to a still greater extent in some lung-diseases, *e. g.* bronchitis, pneumonia, pulmonary congestion *, and phthisis, tubercle and not blood being in the latter the displacing agent. Briefly, the lungs (identical in size in both) contain less blood and more air in tropical than in temperate climates. The truth of the above explanation appears to be confirmed by facts noticeable in Table I., viz. that it is in thoroughly warm tropical regions, where the skin acts most freely and the sensible perspiration is most abundant, that the greatest difference is observable in the capacity of the lungs for air. Thus the difference between the capacity near the equator (temp. 83° F.) and September in England (temp. 65° F.) was 26 cubic inches; while between the latter and the winter of England (February, temp. 42° F.), when the functions of the skin were much more in abeyance, was no more than six cubic inches. And it corresponds with what Dr. Francis (Bengal Army) † has observed, viz. that the lungs are lighter after death in Europeans in India than the European standard. Parkes has made the same observation, and remarks that it shows apparently *a diminished respiratory function.*

A knowledge of this law, in addition to its diagnostic value, is evidently of considerable therapeutic importance as furnishing a guide to the rational treatment of many, and especially congestive or inflammatory diseases of the lungs. By its facts the true *rationale* of the benefit derived in the earlier stages of phthisis, or where it is merely imminent from a sojourn in a subtropical climate, is of easy explanation. Residence in a warm atmosphere is followed by a decrease in the quantity of blood in the affected lungs, by diminished activity in the vital processes carried on therein, by facilitated respiration, and, above all, by diminished lung-work from vicarious action of the physiologically excited skin and liver; while the inhalation of milder, more equable, and less irritant air diminishes the chances of excitement or increase of distressing local inflammation and

---

descent of the diaphragm; and the exceptional case above related of diminution of the costal circumference may possibly be explained by the diaphragm counteracting the other respiratory muscles, whilst unusually contracting in answer to the room required by this reserved power of the bases of the lungs being called into action.

* See an admirable letter on "Swimmer's Cramp" in the 'Lancet' for October 9, 1869, p. 531, by my friend Mr. Henry Hales of Reigate, who first suggested to me a satisfactory explanation of the above phenomena.

† Parkes, 'Practical Hygiene,' 2nd edit. p. 463.

those bronchial attacks so apt to break up old and cause the deposition of new tubercle. Might we not wisely imitate this oftener than we now do in practice? The increased pneumatic capacity of the chest indicates a decrease in the quantity or bulk of blood in the lungs equivalent to the increase in the quantity of air. If, for example, the latter is increased by 30 cubic inches, this implies a permanent withdrawal of 16·62 ounces of blood from the lungs to the skin and liver. Now if we can imitate Nature's operations, and, by increasing the temperature of a sick-room or ward in this the temperate climate of England, can convert it into a local subtropical or tropical climate, we withdraw no inconsiderable amount of blood from the lungs to the skin and liver, thus relieving its overburdened capillaries, permitting freer access of air, and so aiding the respiratory process—a safe and sure mode both of relieving dyspnœa and cough, and aiding the *vis medicatrix*.

In tropical hygiene the law appears equally suggestive. Is not the decrease in the quantity of carbon which the lungs can throw off, by $1\frac{1}{10}$ oz., an indication of the necessity for regulating the diet both as to quantity and kind, and especially of making it less carbonaceous? When this is attended to, and for other reasons already alluded to, the quantity of oxygen taken in by the lungs is sufficient to enable them to throw off their allotted portion of carbon. Even when the diet is unaltered, the functionally excited and vicariously acting skin, liver, and perhaps kidneys may be able to eliminate surplus carbon up to the above-mentioned amount, and perhaps a little beyond it. But this doubtless has a certain and probably individually varying limit; and from a prolonged and excessive ingestion of highly carbonaceous food in the tropics, all three organs are apt to suffer from overwork, as may also the lungs in endeavouring to aid them by accelerated and perhaps forced respirations. The importance of keeping the lungs, the great carbon-eliminator in all climates, and the skin, one of the chief in the tropics, in a state of the most perfect functional activity possible, especially in the tropics, will be equally apparent, as will also, in disease of any one of them, the indication with regard to the rest; and, considering the great importance of the skin in acting vicariously for the lungs as an eliminator both of carbonic acid and water, the necessity for constantly keeping it in healthy action during disease of the latter organs, especially phthisis, will appear imperative.

The following Table will show the relation of the frequency of the respirations to the period of the day in the tropics.

TABLE VI.—To contrast the number of respirations per minute (morning, afternoon, and evening) in tropical and extra-tropical latitudes.

	Extra-tropical, between lat. 50° and 26° N.; average of 26 days.		Tropical, between lat. 32° N. and 13° S.; average of 53 days.
	Number per minute.		Number per minute.
Morning (9 A.M.)	North ... 14·19 South ... 13·8	... 13·99	13·15
Afternoon (3 P.M.)	North ... 16·34 South ... 14·75	... 15·74	13·65
Evening (9 P.M.)	North ... 16·30 South ... 15·06	... 15·68	14·18
Averages		15·07	13·66

Here two facts are apparent,—first, that the number of respirations (morning, afternoon, and evening) are all less than in temperate regions; and, second, what is more specially designed to prove, viz. that in tropical as in temperate latitudes the respiration is least frequent in the morning, and gradually increases as the day advances. Thus at 9 A.M. it was 13·15, at 3 P.M. 13·65, and at 9 P.M. 14·18. The Table further appears to indicate that the difference between the number of morning and evening respirations is not so great in the tropics as in colder latitudes. In the former the lungs play a less active part as a heat-generator and eliminator of carbonic acid and water. Hence the respiration is calmer and more equable. Thus in the tropics there is only one respiration and a fraction more in the evening; while in the temperate zone there is $1\frac{3}{4}$ nearly. Had the season been winter instead of summer, and the weather colder, the difference would have been greater.

II. *The Influence of Tropical Climates on the Pulse.*

Like the previously detailed experiments on the respiration, the present were made during a voyage from England (lat. 51° N.) to Bahia (lat. 11° S.) and back, *i. e.* across the equator, and extend over 60 days (53 in the tropics, and 7 in the latitude of England), during which the thermometer ranged from 57° to 84° F. (27° F.). The observations were taken thrice a day in the standing posture. Table VII. shows that, as in temperate latitudes*, the highest pulse of the day in the tropics occurs (though by no means invariably) in the morning.

TABLE VII.

Highest pulse of the day in the tropics during 53 days	9 A.M.	20 days
	3 P.M.	19 "
	9 P.M.	14 "

* Dr. Guy, Guy's Hospital Reports, vol. iii.

The following Table shows, first, that the average pulse for the tropics ($87\frac{1}{2}$) is lower by $2\frac{1}{2}$ beats than that for the temperate zone (90), indicating a more languid circulation; second, that the same holds good for the average morning and evening pulse; third, that the average afternoon pulse is higher in the tropics than in temperate latitudes, probably from solar heat, greatest at that period of the day; fourth, that the highest and lowest pulse of the period occurs in the morning; fifth, that the morning pulse has the greatest, and the evening the lowest range.

TABLE VIII.—To contrast the tropical and temperate pulse &c.

	Number of observations.	Tropics.				Temperate zone average.
		Lowest.	Highest.	Range.	Average.	
9 A.M.	53	66	112	46	86·4	91·7
3 P.M.	53	66	108	42	88·8	88·1
9 P.M.	49	73	110	37	87·3	90·5
Averages	......	......	......	......	87·5	90·1

The reduction of the pulse in the tropics is doubtless related to the diminished respiratory function; and further observation may prove what the latter fact suggests, viz. that the pulse is diminished not only in frequency but in force*. The rise in the pulse towards afternoon and its subsequent fall are doubtless physiologically connected with a corresponding rise and fall in the temperature of the body at the same periods (Table IX.). The relation, however, does not extend to the volume or number of the respirations, the former being nearly alike during these three periods of the day, and the latter greatest towards night. As these observations, however, on the most excitable of all organs, the pulse, were made on one individual only, and at sea, where motion of the ship, weather, &c. render it impossible to ensure day by day the identical conditions necessary to give completely satisfactory results, their confirmation is necessary.

III. *The Influence of Tropical Climates on the Temperature of the Body.*

In this inquiry, so intimately connected with the two previous ones, viz. the respiration and circulation, the facts were ascertained by placing an ordinary Fahrenheit thermometer under the tongue thrice a day, during the same voyage as last, the temperature of the air in the shade on the verge of the tropics being 72° F., at the equator 84° F., and the average of the tropics generally 76·9; and the atmospheric humidity represented by a difference between the wet and dry bulbs of a Mason's hygrometer of 0° to $7\frac{1}{2}$°, the average being 3°·8 F.

* Parkes ('Practical Hygiene') avers that the pulse is quicker in the tropics, though perhaps not so full. In animals moderate heat does not, but great heat does quicken the pulse. The latter, however, is evidently an unnatural temperature.

The following Table contrasts the average heat of the body in the tropics and in temperate climates. Thus, while in England during a summer and almost subtropical temperature ranging from 60° to 70° F., the average temperature of the body was 98°·3 F. it rose in the tropics to 98°·6, and in the equatorial doldrums to 99° F. The difference would have been greater had the season in England been winter, or the latitude higher. From column 3 it appears that the temperature of the body in the tropics is greatest during the afternoon, when the sun is high and the body most active, and least in the morning—an interesting fact in connexion with the pulse, likewise highest and lowest then.

Table IX.—To contrast the bodily temperature in tropical and extra-tropical latitudes.

	Temperate climate (near England), June, temp. 65° F., average of 10 days.	Tropics generally, average of 51 days,	Equator, temp. 84° F., average of 7 days.
9 A.M.	98·1	98·51	98·5
3 P.M.	98·3	99	99·5
9 P.M.	98·5	98·47	99·1
Average	98·3	98·66	99·02

While observation thus showed that the average temperature of the body about the latitude of England is 98°·3 F., the following Table shows that it rises in the tropics to 98½–99–99½, and occasionally even to 100° F. This fact is interesting, if not important, in connexion with temperatures in disease; and the mutual relation of the two is worth study.

Table X.—To show the temperature of the body in the tropics, and its relation to period of the day.

	Average temp. of air (shade).	Number of observations.	Temp. 98° F.	Temp. 98½° F.	Temp. 99° F.	Temp. 99½° F.	Temp. 100° F.
9 A.M.	98·51	51	22	11	11	7	0
3 P.M.	99	51	6	5	18	17	5
9 P.M.	98·47	51	8	23	15	5	0
Averages and totals	98	153	36	39	44	29	5

Table X. further shows the great preponderance of the lower temperatures, especially the lowest (98° F.), during the mornings, of the higher during afternoon, and of the medium ones (98½° and 99° F.) during the evening. The heat of the body (and the blood?) thus rises in the

tropics with the temperature of the air, and probably the activity of the body and brain, both greatest in the afternoon, and again decrease with these towards evening. The highest temperature (100°) occurred at 3 P.M., the most oppressive part of the day in the equatorial calms, where the air was most stagnant, humid, and hot (81° to 84° F.), with no breeze to cool, by carrying off the surface heat, and facilitating evaporation. Their source could not be dietetic, as little food was taken between 8 A.M. and 6 P.M., and there was no rise after the latter hour. For the same reason it could not be the result of local muscular activity. The totals show that 99° was the most frequent temperature, and next to it 98½°, while 99½° and 100° form no less than 22 per cent. of the whole. Is not the blood itself likewise somewhat warmer in tropical than in extra-tropical latitudes? The range of the temperature of the body in health is thus about 2° F. John Davy gives it as from ·5 to 1° F.*; Eydaux and Brown Sequard at from 1° to to 2½° and 3° F. The super-oceanic atmosphere, in which the present observations were made, is never so highly saturated as to completely stop evaporation from the cutaneous surface, otherwise the temperature of the latter would rise much higher. Blagden and Fordyce bore a temperature of 260° in the dry air of a heated oven, the temperature of their skin rising 2½° F. only; but when the air of the heated oven is so moist as to hinder evaporation, the temperature of the body rises rapidly; Ludwig says as much as 7° or 8° F., and Obernier confirms this †. Observations on this point are still wanted with regard to continental, littoral, and insular equatorial climates, both dry and humid.

The following Table, which shows the relation of the temperature of the body to that of the external air in the shade, indicates a gradual though trivial increase in the temperature of the former with that of the temperature of the latter, proving that the one is slightly influenced by the other. Thus at first it was 98° F.; then 98·3, 98·6, 98·63, 98·8, 99·08 successively, as the temperature of the air rose from 57° F. to 84° F., the average for the warmest part of the temperate zone being 98·3, and for the tropics somewhat more, viz. 98·836. The Table further shows that 98° F. is the prevalent temperature of the body in the temperate zone, as it occurred in 24 out of 37 markings; while in the tropics 99° F. is the most frequent, and 98½° and 99½° nearly as frequent. These results correspond closely with those of Dr. Davy ‡.

* Parkes, 'Practical Hygiene.' † Ibid.

‡ As quoted by Carpenter ('Physiology').

Table XI.—To show the relation of temperature of the body to that of the air in the shade, especially in the tropics.

		Temperate Zone. Temperature of air between				Tropics. Temperature of air between			
		57° & 60°.	60° & 65°.	65° & 70°.		70° & 75°.	75° & 80°.	80° & 84°.	
Highest		98°	99°	99½°		100°	100°	100°	
Lowest		98	98	98		98	98	98	
Range		0	1	1½		2	2	2	
Averages		98	98·3	98·6		98·63	98·8	99·08	
Total averages			98·3				98·836		
					Totals.				Totals.
Number of markings	98°	7	6	9	22	20	18	6	44
of temperature of	98½°	...	1	1	2	4	37	3	44
body, taken thrice	99°	...	2	6	8	18	32	6	56
a day, viz. at 9 A.M.,	99½°	...	...	3	3	4	23	12	39
3 P.M., and 9 P.M.	100°	...	...	...	...	2	1	4	7
			Total	.....	35		Total	.....	190

Unfortunately the statistics were not sufficiently numerous to draw satisfactory deductions as to the effect of humidity on the temperature of the body in mid-ocean in the tropics, where the amount of moisture is usually considerable. This would be interesting, as would others regarding tropical terrestrial climates, in some of which the hygrometric range is sometimes smaller, though in the majority greater.

V. "Observations on the Mode of Growth of Discoid and Turbinated Shells." By Alexander Macalister, Professor of Zoology, University of Dublin. Communicated by the Rev. S. Haughton, F.R.S. Received May 4, 1870.

A most interesting paper on the geometrical forms of turbinated and discoid shells was published by the Rev. Canon Moseley in the Philosophical Transactions for 1838, p. 351, in which some important points were noticed regarding the geometrical construction of shell-forms. The author of that paper describes discoid shells as generated by the revolution around a central point of the perimeter of a geometrical figure, which latter, although regularly increasing in size, yet remains always geometrically similar in form. The producing figure in many Gasteropodous Mollusks is represented by the operculum, and in all it may be recognized by making a vertical section in the plane of the radius vector. A turbinated shell is similarly generated, but the generating figure in the production of the helix slips down along the axis instead of revolving in a constant plane. The Rev. Mr. Moseley gives, as illustrations of these points, measurements of *Nautilus pompilius*, *Turbo phasianus*, *Turbo duplicatus*, and

Buccinum subulatum, and describes many interesting particulars regarding the formation and growth of the operculum in different shells.

This subject does not seem to have attracted much attention from naturalists, as, with the exception of a notice in Professor Goodsir's lecture "On the Use of Mathematical Modes of Investigating Organic Forms" *, it is not, to my knowledge, referred to by any writer on zoology.

While engaged in arranging the large collection of shells in the Museum of the University of Dublin, I was led to make measurements of univalve shells in order to see whether any deduction of zoological importance might be drawn from these valuable geometrical observations, and more especially to determine whether it might be possible to arrive at constant specific numerical parameters in these cases; and in all instances I have been surprised by finding that, in well-formed shells, the ratios of the successive whorls have been specifically constant. In making these measurements, the points to be determined are three, viz.:—1st, the ratio of elongation of the radius vector of the spiral (k); 2nd, the degree of linear expansion of the generating figure in the successive whorls (m); and 3rd, the degree of translation or slipping of the spiral on the central axis (n). The second of these we may call the discoidal coefficient, and the third the helicoidal coefficient.

On applying these measurements to univalve shells, we find that the possible combinations are five in number:—

1st, those in which $n=0$ and $m<k$,
2nd, those in which $n=0$ and $m=k$,
3rd, those in which $n=m$,
4th, those in which $n>m$,
5th, those in which $n<m$.

The cases of discoid shells in which $n=0$ are two, the first and second on the list. The first and most uncommon is that in which the amount of elongation of the radius vector in the formation of the successive whorls exceeds the transverse linear increase of the producing figure. The resulting form of this case (which may be formulated thus, $k>m$) is an open spiral, as in the fossil Gasteropodous genus *Eccyliomphalus*, or the Cephalopodous genera *Gyroceras*, *Nautiloceras*, and *Spirula*. The common species of this last genus gives the following measurements:—

Spirula prototypus, $m=2{\cdot}6$, $k=3{\cdot}3$, $n=0$. Generating figure, a circle. Average width of whorls 0·075 in., 0·2 in.†

It will be noted that all these spirals are true logarithmic curves; and

* Goodsir's 'Anatomical Memoirs,' vol. ii. p. 209.

† In all the specimens measured and referred to in this paper I have made at least three measurements of each individual, and in the majority of cases I have measured at least six specimens of each species. These measurements are in decimal parts of an English inch, and were made with a finely pointed pair of compasses and a diagonal scale, the eye being in some cases aided by a magnifying-glass. Some specimens were measured by means of sections made in a plane perpendicular to the axis.

hence the widths of the whorls measured on the radius vector will form a series of numbers in geometrical progression, the common ratio of the progression being, in discoid shells of the second group where $m=k$, equal to the coefficient of linear increase of the generating figure. To verify the coefficients deduced from the numbers obtained by measurement, I have used the method given by the Rev. Canon Moseley, which depends upon a well-ascertained property of the logarithmic spiral, that if μ be taken to represent the ratio of the sum of the lengths of an even number (m) of the whorls to the lengths of half that number, then $k=(\mu-1)^{\frac{2}{m}}$. Applying this formula to the cases given below, I have in the majority of cases obtained results which confirm the ratios of the series of measurements otherwise obtained.

The second case of discoid shells, in which $m=k$ and $n=0$, is by far the commoner, as to it belong all genera of discoidal mollusks, with the few exceptions noticed above. The case $m > k$ is one which cannot occur, as then the outer whorl must necessarily crush the inner, and then the generating figure could not retain its geometrical identity while enlarging; hence we find no examples of it in discoid shells.

I have placed in this second case some instances in which the ratio of slipping or translation on the axis is not easily measured, and virtually amounted to nothing.

The following Table of examples illustrate case No. 2:—

Species.	$n=0$, $k=m$.	Generating figure.	Width of whorls in decimals of an inch.							
Haliotis viridis	10	Ellipse	0·075 0·05 0·15	0·75 0·5 1·5						
Haliotis rugoso-plicata	9·3	,,	0·02 0·03	0·18 0·28	1·6 1·7					
Sulculus (Haliotis) parvus	6	Oval	0·03	0·17	1					
Padollus (Haliotis) excavatus	4·2	Ellipse	0·06	0·25	1·1					
Natica canrena	3	Segment of circle	0·025	0·075	0·25	0·76				
Nautilus pompilius	3	Segment of ellipsoid	0·2265	0·68	2·04					
Dolium zonatum	2·1	......	0·119	0·25	0·525					
Solaropsis pellis-serpentis	2	......	0·023	0·047	0·086	0·17	0·34			
Planorbis corneus	2	Segment of circle	0·02	0·04	0·08	0·172				
Euomphalus pentangulatus	2	......	0·124	0·25	0·48					
Architectonica magnifica	1·75	Rhomboid	0·07	0·12	0·2	0·35	0·65			
Architectonica trochleare	1·62	,,	0·046	0·075	0·175	0·2	0·325	0·55		
Conus betulinus	1·43	Triangle	0·02	0·03	0·05	0·072	0·09	0·12	0·17	0·25
Conus literatus	1·4	,,	0·03	0·04	0·05	0·86	0·125	0·176	0·25	
Conus virgo	1·25	,,	0·08	0·1	0·105	0·16				
Planorbis, sp.	1·38	......	0·03	0·042	0·053	0·078	0·1	0·15	0·18	

Hitherto we have been examining the formulæ for discoid shells; but by far the greater number of shell-forms are those in which the whorls, instead of remaining in the same plane, slide down on the central axis, thus making a turbinated shell-form. A new principle enters into our calculation here; for the shape of a turbinated shell depends on the mutual relation of three, and not two constants. These are, first, the form of the

generating figure; secondly, the discoidal coefficient m; thirdly, the helicoidal coefficient n. Upon the relations of these parameters to each other depends the shape of the shell. Thus in some n is nearly equal to m, and in such cases the whorls scarcely embrace each other, and the figure produced is that of an elongated cone, as in the genera *Turritella*, *Cerithium*, *Acus*, &c. Sometimes n exceeds m; and in this case the resulting form is an open spiral as in *Vermetus*, or a rapidly descending series of whorls. A third possible case is that in which n is less than m, and the resulting figure is globular; but of this case, though a possible one, I have not as yet succeeded in obtaining an example.

The following cases illustrate the formula $n > m$:—

	n.	m.	Width of whorls in decimals of an inch.				Amount of translation.			
Vermetus lumbricalis ..	1·42	1·3	0·075	0·1	0·13	0·175	0·15	0·22	0·3	0·45
Delphinula atrata	6·00	2·85	0·018	0·5	0·148	0·41	0·01	0·05	0·3	

The following instances exemplify the case $n = m$:—

Species.	$n=m$.	Length of whorls in decimals of an inch.											
Helicostyla polychroa....	2	0·41	0·081	0·158	0·32	0·7							
Fusus colosseus	1·71	0·09	0·14	0·26	0·43	0·76							
Phasianella bulimoides ..	1·8	0·07	0·125	0·23	0·45								
Scalaria preciosa........	1·56	0·05	0·078	0·13	0·2	0·32	0·52						
Fusus antiquus	1·5	0·15	0·225	0·343	0·54	0·84							
Mitra episcopalis	1·434	0·245	0·4	0·575	0·82								
Trochus niloticus	1·41	0·2	0·3	0·425	0·63	0·9	1·2						
Fusus longissimus	1·341	0·25	0·3	0·44	0·6	0·81							
Fusus colus	1·33	0·15	0·2	0·26	0·35	0·42	0·54	0·83					
Pyrazus sulcatus........	1·33	0·13	0·17	0·29	0·38	0·51							
Acus dimidiata	1·277	0·2	0·267	0·31	0·4	0·52	0·62	0·88					
Acus maculata..........	1·25	0·15	0·176	0·23	0·29	0·37	0·45	0·53	0·7	0·9			
Acus crenulatus	1·25	0·2	0·25	0·32	0·38	0·496	0·6						
Cerithium nodulosum ..	1·24	0·23	0·3	0·37									
Pirena terebralis........	1·23	0·08	0·12	0·15	0·178	0·22	0·28	0·35					
Pyrazus palustris	1·22	0·15	0·182	0·22	0·27	0·34	0·42	0·5					
Zaria duplicata	1·23	0·078	0·1	0·125	0·16	0·2	0·24	0·3	0·26	0·44	0·53	0·625	0·76
Acus subulata	1·163	0·175	0·2	0·23	0·265	0·32	0·367	0·432	0·47	0·641			
Telescopium fuscum	1·14	0·1	0·112	0·125	0·15	0·18	0·2	0·24	0·28	0·325	0·365		

VI. "Contributions to Terrestrial Magnetism.—No. XII. The Magnetic Survey of the British Islands, reduced to the epoch of 1842–5." By General Sir Edward Sabine, K.C.B., President of the Royal Society. Received June 15, 1870.

(Abstract.)

This paper contains a statement of the origin, progress, and completion of this survey. It is accompanied by maps of the declination, inclination, and magnetic force, which have been drawn at the Hydrographic Office of the Admiralty under the superintendence of Captain Frederick John Evans, R.N., F.R.S. The paper consists in great measure of Tables, giving the observation of each of the three magnetic elements, with reductions in every case for the secular change between the date of the observation and that of the epoch (1842–5) for which the maps are constructed.

VII. "On Supersaturated Saline Solutions."—Part II. By CHARLES TOMLINSON, F.R.S. Received May 17, 1870.

(Abstract.)

The object of this paper is to develop more fully the principles attempted to be established in Part I.*, not only by clearer definitions of terms, but also by new facts and conclusions. The paper is divided into two sections; in the first of which are stated the conditions under which nuclei act in separating salt or gas or vapour from their supersaturated solutions, while in the second section is shown the action of low temperatures on supersaturated saline solutions.

The first section opens with definitions of the terms used.

A *nucleus* is a body that has a stronger attraction for the gas or vapour or salt of a solution than for the liquid that holds it in solution.

A body is *chemically clean* the surface of which is entirely free from any substance foreign to its own composition. Oils and other liquids are chemically clean if chemically pure, and contain no substance, mixed or dissolved, that is foreign to their composition. But with respect to the nuclear action of oils &c., the behaviour is different when such bodies exist in the mass, such as a lens or globule, as compared with the same bodies in the form of films.

Catharization is the act of clearing the surface of bodies from all alien matter, and the substance is said to be *catharized* when its surface is so cleared.

As everything exposed to the air or to the touch takes more or less a deposit or film of foreign matter, substances are classed as *catharized* or *uncatharized* according as they have or have not been so freed from foreign matter.

Referring to the definition of a nucleus, substances are divided into *nuclear* or *non-nuclear*.

The nuclear are those that may, *per se*, become nuclei. The non-nuclear are those that have not that quality.

The nuclear substances would seem to be comparatively few, the larger number of natural substances ranking under the other division.

Under nuclear substances are those vapours and oily and other liquids that form thin films on the surfaces of liquids and solids; and generally all substances in the form of films, and only in that form. Thus a stick of tallow, chemically clean, will not act, but a film of it will act powerfully; and again, a globule of castor-oil will not act, if chemically clean; but in the form of a film, whether chemically clean or not, it will act powerfully.

If a drop of a liquid be placed on the surface of another liquid it will do one of three things (apart from chemical action),—(1) it will diffuse through the liquid, and in general, under such circumstances, not act as a

* Proc. Roy. Soc. vol. xvi. p. 403; Phil. Trans. 1868, p. 659.

nucleus; or (2) it will spread out into a film, or (3) remain in a lenticular shape. It becomes a film or a lens according to the general proposition, that if on the surface of the liquid A, whose surface-tension is a, we deposit a drop of the liquid B, whose surface-tension, b, is less than a, the drop will spread into a film; but if, on the contrary, b be greater than a, or only a little less, the drop will remain in the form of a lens. Hence if B spread on A, A will not spread on the surface of B.

This general proposition may not always apply in the case of supersaturated saline solutions, on account of the *superficial viscosity*, or the greater or less difficulty of the superficial molecules to be displaced.

A glass rod drawn through the hand becomes covered with a thin film, or the same rod by exposure to the air contracts a film by the condensation of floating vapours, dust, &c., and in either case is brought into the nuclear condition.

A second class of nuclear bodies are permanently *porous* substances, such as charcoal, coke, pumice, &c. The action of these is chiefly confined to vaporous solutions, and if catharized having no power of separating salts from their supersaturated solutions.

Under the non-nuclear, forming by far the larger class of substances, are glass, the metals, &c., while their surfaces are chemically clean.

Among the non-nuclear substances will be found air; for its ascribed nuclear character is due, not to itself, but to the nuclear particles of which it is the vehicle. Thus, as stated in Part I., if air be filtered through cotton-wool it loses its apparent nuclear character; so also if heated.

When a catharized body is placed in a supersaturated solution, such solution, as explained in Part I., adheres to it as a whole; but if such body be non-catharized, the gas or vapour or salt of the solution adheres to it more strongly than the liquid portion, and hence there is a separation. In the present paper it is shown that an active or non-catharized surface is one contaminated with a film of foreign matter, which filmy condition is necessary to that close adhesion which brings about the nuclear action; for it can be shown that an oil, for example, is non-nuclear in the form of a lens or globule, but powerfully nuclear in the form of a film.

Some liquids (absolute alcohol for example) form films, and act as nuclei by separating water instead of salt from supersaturated solutions.

Other liquids (glycerin for example) diffuse through the solutions without acting as nuclei.

Fatty oils may slowly saponify, or oil of bitter almonds form benzoic acid in contact with supersaturated solutions of Glauber's salt without acting as nuclei.

The solutions (say of Glauber's salt) are prepared with 1, 2, or 3 parts of the salt to 1 part of water; they are boiled, filtered into clean flasks, and covered with watch-glasses. When cold, the watch-glass being lifted off, a drop of oil is deposited on the surface of the supersatu-

rated solution. In an experiment described, a drop of pale seal-oil formed a well-shaped film, with a display of iridescent rings, and immediately from the lower surface of the film there fell large flat prisms with dihedral summits of the 10-atom sodic sulphate. The prisms were an inch or an inch and a half in length, and three-eighths of an inch across. The crystallization proceeded from every part of the lower surface of the film, and as one set of crystals fell off, another set was formed, until the whole solution became a mass of fine crystals in a small quantity of liquid, an effect quite different from the usual crystallization which takes place when a supersaturated solution of Glauber's salt is subjected to the action of a nucleus at one or two points in its surface, as when motes of dust enter from the air, or the surface is touched with a nuclear point. In such case small crystalline needles diverge from the point and proceed rapidly in well-packed lines to the bottom, the whole being too crowded and too rapid to allow of the formation of regular crystals.

Similar experiments were made on solutions of Glauber's salt of different strengths, with drops of ether, absolute alcohol, naphtha, benzole, the oils of turpentine, cajuput, and other volatile oils, sperm, herring, olive, linseed, castor, and other fixed oils of animal and vegetable origin, with this general result, that whenever the liquid drop spread out into a film, it acted as a powerful nucleus; but when the oil formed a lens there was no separation of salt, even when the flasks were shaken so as to break up the lens into small globules. If, however, a sudden jerk were given to the flask so as to flatten some of the globules against its sides into films, the whole solution instantly became solid. A similar effect was produced by introducing a clean inactive solid for the purpose of flattening a portion of oil against the side of the flask.

Stearine from sheep's tallow that had been exposed to the air produced immediate crystallization, but by boiling the solution and covering the flasks, the stearine, now catharized, had lost its nuclear character on the cold solution. Similar observations were made with the fixed oils that form lenses or globules in the solution. So also volatile oils containing products of oxidation, dust, &c. are nuclear, but when catharized by being redistilled they are inactive in the globular state, active in the form of films.

Supersaturated solutions of potash alum, ammonia alum, sodic acetate, and magnesia sulphate were also operated on with results similar to those obtained with solutions of Glauber's salt.

When a liquid forms a film on the surface of a supersaturated solution, the surface-tension of the solution is so far diminished as to bring the film into contact with the solution, when that differential kind of action takes place whereby the salt of the solution adhering more strongly to the film than the water of the solution, the action of separation and crystallization, thus once begun, is propagated throughout. A similar action takes place with solid bodies that have contracted filmy nuclei by being touched or drawn through the hand, or merely exposed to the air;

they are active or nuclear by virtue of the films of matter which more or less cover them.

On the other hand, when a drop of oil (or many drops) is placed on the surface of a supersaturated saline solution, and it assumes the lenticular form, or even flattens into a disk, such lens or disk is separated from actual contact with the solution by surface-tension. That the adhesion is very different from that of a film may be shown by pouring a quantity of recently distilled turpentine, for example, on the surface of chemically clean water, and scraping upon it some fragments of camphor; these will be immediately covered with a solution of camphor in the oil, which solution will form iridescent films, and sail about with the camphor, vigorously displacing the turpentine, and cutting it up into smaller disks and lenses. So in the case of supersaturated saline solutions, the oil-lens is not sufficiently in contact with the surface of the solution to allow of the exertion of that differential kind of action whereby salt is separated. Even when, by shaking, the oil is broken up into globules, and these are submerged, they are still so far separated from the solution by surface-tension as to prevent actual contact.

In the second section it is shown that solutions of certain salts which remain liquid and supersaturated at and about the freezing-point of water, by a further reduction in temperature, to from 0° Fahr. to —10° and in the absence of a nucleus, rather solidify than crystallize, but on being restored to 32° recover their liquid state without any separation of salt.

A solution of ferrous sulphate, for example, at 0° Fahr. formed tetrahedral crystals at the surface, which spread downwards until the contents of the tube became solid. In snow-water at 32° the frozen mass shrank from the sides of the tube, formed into a smooth rounded mass, and gradually melted, leaving the solution clear and bright without any deposit. On removing the cotton-wool from the mouth of the tube, small but well-shaped rhomboidal crystals soon filled the solution.

A similar experiment was tried with the double salt formed by mixing in atomic proportions solutions of the zincic and magnesic sulphates. A supersaturated solution of this salt at about —8° Fahr. became solid, and it melted quickly at 32°. Such a solution may be solidified and melted a number of times, provided it be protected from the action of nuclei; but if the cotton-wool be removed from the tube, even when the contents are solid, and be immediately reinserted, there will be a separation of the salt during the melting, in consequence of the entrance of nuclear particles from the air.

Solutions of such a strength as to be only saturated at ordinary temperatures, and therefore not sensitive to the action of nuclei, become very much so by reduction of temperature below 32° Fahr. Salts that contain a large amount of water of crystallization, such as the zincic and magnesic sulphates, require only a small portion of added water in order

to form supersaturated solutions, and they become more sensitive to the action of nuclei as the temperature falls, or, in other words, as they become more highly supersaturated. Thus a very strong solution of calcic chloride, which is not sensitive to nuclei at 40° or 45°, becomes very much so at 24° to 34°.

The sodio-zincic sulphate contains only 4 proportionals of water of crystallization, and hence its supersaturated solutions are not stable at low temperatures. When freshly made, they may be reduced to 10° Fahr. withouts eparation of the salt; but by repose, even in clean tubes and in the absence of nuclei, long crystals of the separated salts occupy the length of the tube, but they are invisible on account of having the same refractive index as that of the solution in which they are immersed. In the course of time, probably from the escape of vapour of water through the porous plug, they become visible.

A solution of the ammonia zincic sulphate at 4° Fahr. formed beautiful large feathery crystals of an opaque white, which gradually filled the tube. They melted rapidly at 32°.

A supersaturated solution of nickel sulphate resisted a temperature of 6° Fahr. Mixed with an equivalent weight of cupric sulphate, the two salts separate if the solution be exposed to the air, but in closed tubes the solution at 0° Fahr. forms beautiful feathery crystals, which melt rapidly at 32°, without any separation of salt.

Similar phenomena are produced by a supersaturated solution of zinc sulphate and potash alum in equivalent proportions exposed to a temperature of 4° Fahr. A similar solution of the cupric and magnesic sulphates at −4° also became solid, and melted rapidly at 32°.

Experiments were also made with the sodic and magnesic sulphates, cadmic, and some other sulphates. The addition of potassic sulphate to other sulphates, in atomic proportions, forms double salts, which, so far as they were examined, do not form supersaturated solutions.

The effect of low temperatures was in some cases to throw down a portion of the salts in the anhydrous form, upon which were formed by repose crystals of a lower degree of hydration than the normal salt. Some cases of this kind are described in the paper.

VIII. "On Furfuraniline and Furfurtoluidine." By JOHN STENHOUSE, LL.D., F.R.S. Received May 19, 1870.

In an epistolary communication to Mr. H. Watts * I stated that "The most abundant and economical source of furfurol is in the preparation of garancin by boiling madder with sulphuric acid. If the wooden boilers, in which garancin is usually manufactured were fitted with condensers, furfurol might be obtained in any quantity without expense. Furfurol is

* Watts's Dictionary, ii. 751.

also produced by boiling any kind of madder with solution of sulphate of aluminium.

Crude furfurol, whether obtained from madder, bran, sawdust, or any other of its usual sources, is always contaminated with another aromatic oil, which I called metafurfurol *. This has a higher boiling-point, and oxidizes more readily than furfurol, so that by repeated rectification almost the whole of it is converted into a brown resinous matter, which remains in the retort. It is owing to the presence of this impurity that crude furfurol so rapidly changes its colour when kept, the metafurfurol not only being itself decomposed, but superinducing the oxidation of the furfurol.

A much simpler and more effective method, however, of purifying furfurol from this substance consists in digesting it for some hours with very dilute sulphuric acid, to which small quantities of acid potassium chromate are added from time to time; this effectually destroys the metafurfurol and other impurities, so that the furfurol which distils over after being separated from the water and dried over fused chloride of calcium has a constant boiling-point of 161° C. It has a much more agreeable odour than before, is nearly colourless, and may be exposed to the air under a layer of water for months, without any considerable increase of colour. Its refractive index for the D line at 20° C. is 1·520 †.

Action of Furfurol on Aniline Furfuraniline.

Twenty years ago ‡ I stated that when aniline was added to furfurol, the mixture acquired a rose-red colour, which it communicated to the skin, and likewise to paper, linen, and silk, but that these rapidly lost their colour, becoming of a brownish yellow, even when light was excluded. I was unable, however, to obtain this red substance in a crystalline state.

In 1860 the subject was again examined by M. Jules Persor §, who dissolved aniline in acetic acid, and then added an excess of aqueous solution of furfurol; after some time a deep red viscid mass was deposited on the sides of the vessel, which communicated to silk and wool a fine but very fugitive red colour. He was not more fortunate than I had been in obtaining this substance in a crystalline state.

A few months ago I resumed the investigation of this subject, and although I was unable to obtain definite compounds either by the action of furfurol on aniline itself or on its salts when in a pure state, yet when furfurol was added to a strong alcoholic solution of aniline hydrochlorate containing an excess of aniline a deep red colour was produced, and in the course of a few minutes the mixture solidified to a crystalline mass of a fine iridescent purple colour.

* Ann. Chem. Phar. lxxiv. 282.

† I am indebted to the kindness of Messrs. T. and H. Smith, of Edinburgh, for the greater portion of the furfurol employed in this investigation. This firm has long been in the habit of manufacturing it for preparing furfurine for medical purposes.

‡ Ann. Chem. Pharm. lxxiv. 282. § Rep. Chem. App. 1860, p. 220.

Furfuraniline hydrochlorate.—The best method of preparing this salt was to dissolve 46 parts aniline and 65 parts aniline hydrochlorate in 400 parts of warm alcohol, and then add 48 parts furfurol, likewise dissolved in 400 parts spirit; after the solutions were thoroughly mixed, they solidified in the course of a few minutes to a mass of crystals of the salt. When cold, these were thrown on to a filter, freed from the mother liquor by means of a vacuum filter, and washed with a small quantity of coloured spirit. They were then readily obtained in a pure state by recrystallization from boiling spirit. The substance analyzed was dried *in vacuo*.

I. ·207 grm. substance gave ·488 grm. carbonic anhydride and ·120 grm. water.

II. ·158 grm. substance gave ·372 grm. carbonic anhydride and ·082 grm. water.

III. ·228 grm. substance gave ·103 grm. argentic chloride.

IV. ·282 grm. substance gave ·128 grm. argentic chloride.

V. ·0448 grm. substance gave ·003773 grm. nitrogen.

		Theory.	I.	II.	III.	IV.	V.	Mean.
C_{17} =	204	64·05	64·30	64·21	......	......	......	64·25
H_{19} =	19	5·97	6·44	5·77	......	......	......	6·10
O_2 =	32	10·05	......	......	......	......	......	......
N_2 =	28	8·79	......	......	......	......	8·42	8·42
Cl =	35·5	11·14	......	......	11·18	11·23	......	11·20
	318·5							

This corresponds nearly to the formula $C_{17} H_{18} O_2 N_2$, Cl H.

It is insoluble in benzol, bisulphide of carbon, and water, but is slowly decomposed when boiled with the latter. It is soluble in boiling spirit, and crystallizes out on cooling in small needles of a fine purple colour, which acquire a metallic lustre on drying. The crystals are permanent in dry air when light is excluded, but are readily decomposed when boiled with dilute acids or alkalies.

Furfuraniline nitrate.—This is prepared in a manner similar to that employed for the hydrochlorate: 23 parts aniline and 39 of nitrate were dissolved in 200 parts warm spirit, and 24 parts furfurol in 200 of spirit added. The mixture, on being allowed to stand some time, became a semisolid crystalline mass, which was purified in the same manner as the corresponding hydrochlorate.

I. ·289 grm. substance gave ·628 grm. carbonic anhydride and ·157 grm. water.

		Theory.	I.
C_{17} =	204	59·13	59·28
H_{19} =	19	5·51	6·04
N_3 =	42	12·17	....
O_5 =	80	23·19	....
	345	100·00	

This nitrate is therefore $C_{17} H_{18} N_2 O_2, NO_3 H$.

It resembles the hydrochlorate in its properties, but is much more soluble in boiling spirit, and forms larger crystals.

Furfuraniline sulphate.—When 23 parts aniline and 35 of its sulphate were dissolved in 3000 of boiling alcohol, and 24 parts of furfurol in 200 of boiling spirit added, the mixture became deep red, and on cooling deposited minute purple needles of the furfuraniline sulphate, contaminated, however, with colourless crystals of aniline sulphate. When an attempt was made to separate these by crystallization from alcohol, the furfuraniline salt was mostly decomposed, with formation of aniline sulphate, which crystallized out.

Furfuraniline oxalate.—When aniline oxalate of aniline and furfurol were dissolved in spirit, as in the preparation of the salts above described, the solution became of a deep red colour, but did not yield crystals of oxalate of furfuraniline, only oxalate of aniline and a dark red tarry matter being produced.

Furfuraniline.—In order to obtain this base, a salt of furfuraniline (the hydrochlorate or nitrate) was ground up to a paste with water and strong aqueous ammonia added, the whole being intimately mixed until the purple colour disappeared, giving place to a pale brown. Warm water was then added until the liberated base became soft and plastic, so that it could be needed in successive quantities of warm water, in order to remove the ammoniacal salts and free ammonia. The base, as thus prepared, has much the pale brown glossy appearance of stick lac, and, like it, can be drawn out into strings when soft. It is insoluble in water, but very soluble in ether and alcohol, and when hydrochloric acid is added to a strong spirituous solution it becomes deep red, and solidifies in a few moments to a mass of the purple crystals of the hydrochlorate. The base decomposes, however, very rapidly when exposed to the air, or when boiled with alcohol, and will then no longer yield crystalline salts with acids. The same effect takes place, but more slowly, in a vacuum.

Action of Furfurol on Toluidine Furfurtoluidine.

When alcoholic solutions of toluidine and furfurol were mixed, there was no immediate change; but after standing some time they acquired a red colour: as in the corresponding reaction with aniline no crystalline sabstance was produced.

Furfurtoluidine hydrochlorate.—The method employed to obtain this salt was similar to that used in preparing furfuraniline hydrochlorate: 12 parts toluidine hydrochlorate and 9 parts crystalline toluidine were dissolved in 150 parts hot spirit, and 8 parts of furfurol dissolved in 150 parts spirit added; the mixture acquired a deep-red colour, and on cooling became a mass of minute acicular crystals closely resembling in appearance the furfuralinine salt. It was purified by recrystallization from boiling alcohol, dried in vacuo, and analyzed.

I. ·232 grm. substance gave ·562 grm. carbonic anhydride and ·135 grm. water.

		Theory.	I.
C_{19}	= 228	65·80	66·07
H_{23}	= 23	6·64	6·47
O_2	= 32	9·24	....
N_2	= 28	8·08	....
Cl	= 35·5	10·24	....
	346·5	100·00	

This corresponds to $C_{19}\ H_{22}\ O_2\ N_2$, Cl H.

Furfurtoluidine nitrate.—This was prepared in a manner similar to the hydrochlorate, substituting the equivalent proportion of toluidine nitrate: 14 parts toluidine nitrate and 9 parts toluidine were dissolved in 100 parts hot alcohol, and 8 parts furfurol in an equal quantity (100 parts) of spirit added; after standing some time the nitrate crystallized out in deep purple needles. When purified and analyzed it gave the following numbers:—

I. ·160 grm. substance gave ·355 grm. carbonic anhydride and ·098 grm. water.

		Theory.	I.
C_{19}	=228	61·12	60·52
H_{23}	= 23	6·17	6·81
O_5	= 80	21·45	....
N_3	= 42	11·26	....
	373	100·00	

It has therefore the composition $C_{16}\ H_{22}\ N_2$, NO_3, H.

Furfurtoluidine.—The salts of furfurtoluidine, when treated with ammonia solution, were decomposed in a manner similar to that already described under the head *furfuraniline,* but not quite so readily. The crude free furfurtoluidine, when digested with ether, dissolved, and on filtering the solution, distilling off the ether, and drying the residue in a vacuum over sulphuric acid, the base was obtained as a brown amorphous mass, which is brittle and easily reduced to powder. It is not as fusible as furfuraniline, and is far less readily decomposed. A carbon and hydrogen determination of the freshly prepared base, purified by ether and dried *in vacuo,* gave the following results:—

I. ·243 grm. substance gave ·660 grm. carbonic anhydride and ·159 grm. water.

		Theory.	I.
C_{19}	=228	73·54	74·08
H_{22}	= 22	7·11	7·27
O_2	= 32	10·32	....
N_2	= 28	9·03	....
	310	100·00	

This corresponds pretty nearly with the formula C_{19}, H_{22}, O_2, N_2.

Both furfuraniline and furfurtoluidine resemble rosaniline in giving beautifully coloured salts, whilst the bases are nearly colourless, or of a pale brown colour.

Furfurnaphthylamine.—When an alcoholic solution of furfurol was added to a similar solution of naphthylamine it immediately became of a red colour, which is as fugitive as the one obtained from aniline, but much duller. Several attempts were made to prepare crystalline salts of this compound, but without success, only dark-coloured resinous substances being obtained.

Several other typical bases were also tried, but without any results. These were quinidine, coniine, sparteine, and theine. It appears, therefore, from these experiments, that it is only the bases of the aromatic series which combine with furfurol to yield these peculiar-coloured salts in a crystalline state.

I cannot conclude this paper without acknowledging the very efficient aid I have received from my assistant, Mr. Charles Edward Groves, in the preceding investigation.

IX. "On Parasulphide of Phenyl and Parasulphobenzine." By JOHN STENHOUSE, LL.D., F.R.S., &c. Received May 27, 1870.

When sulphide of phenyl, $\left.\begin{matrix} C_6H_5 \\ C_6H_5 \end{matrix}\right\} S$, was passed several times in succession through an iron tube filled with nails and heated to low redness, a considerable amount of carbonaceous matter was deposited, and a portion of the sulphide was converted into an isomeric compound, which I propose to call Parasulphide of Phenyl.

In order to obtain this substance from the dark-coloured distillate which collected in the receiver when sulphide of phenyl was submitted to the action of heat in the manner above described, it was transferred to a copper retort and distilled. The clear dark-yellow oil was then cooled for several hours in a freezing-mixture, when a considerable quantity of a white crystalline substance separated in nodules; this is freed from undecomposed sulphide of phenyl by thoroughly draining it on a vacuum filter. It can readily be purified by repeated crystallization from boiling alcohol, in which it is rather soluble.

I. ·197 grm. substance gave ·557 grm. carbonic anhydride and ·092 grm. water.

II. ·166 grm. substance gave ·473 grm. carbonic anhydride and ·077 grm. water.

III. ·200 grm. substance gave ·254 grm. barium sulphate.

IV. ·218 grm. substance gave ·276 grm. barium sulphate.

	Theory.	I.	II.	III.	IV.	Mean.
$C_{12} = 144$	77·41	77·13	77·73	..	..	77·43
$H_{10} = 10$	5·38	5·19	5·15	..	..	5·17
$S = 32$	17·21	..	..	17·43	17·37	17·40
186	100·00					

This corresponds to the empirical formula $C_{12}H_{10}S$.

Parasulphide of phenyl crystallizes from alcohol in small white needles, which melt at 94° C., and can be distilled at a very high temperature. It is insoluble in water, but rather soluble in bisulphide of carbon, ether, and benzol.

Parasulphobenzine.—When parasulphide of phenyl was digested for several hours with dilute sulphuric acid and acid chromate of potassium, it was gradually converted into a new substance, having a much higher melting-point, so that the completion of the oxidation was readily observed by the entire disappearance of the fused parasulphide. The crude parasulphobenzine was then collected, well washed with water, and purified by two or three crystallizations out of boiling alcohol.

I. ·194 grm. substance gave ·470 grm. carbonic anhydride and ·075 grm. water.

II. ·347 grm. substance gave ·843 grm. carbonic anhydride and ·138 grm. water.

	Theory.	I.	II.	Mean.
$C_{12} = 144$	66·06	66·09	66·27	66·18
$H_{10} = 10$	4·58	4·30	4·42	4·36
$S = 32$	14·68	..	..	..
$O_2 = 32$	14·68	..	..	..
	100·00			

These carbon determinations correspond with the formula $C_{12}H_{10}SO_2$; it has therefore the same percentage composition as the sulphobenzine obtained by the oxidation of sulphide of phenyl *.

Parasulphobenzolene melts at 230° C., is soluble in boiling alcohol, from which it crystallizes on cooling in the form of long white shining needles. It is insoluble in water, soluble in benzol, ether, and carbon disulphide. It dissolves readily in warm sulphuric acid, forming a colourless solution, and does not blacken even when heated to the boiling-point of the acid. Water precipitates it unchanged. It is also soluble in hot strong nitric acid without change, and crystallizes out on cooling.

* Proc. Roy. Soc. vol. xiv. p. 351.

X. "On a Method of graphically representing the Dimensions and Proportions of the Teeth of Mammals." By George Busk, F.R.S. Received May 20, 1870.

Of all the hard parts of animals, the teeth, more especially for palæontological purposes, undoubtedly afford the most constant and the most generally available characters. Any plan, therefore, by which the study and ready comparison of these organs may be facilitated and simplified cannot fail to be of some use to the zoologist and palæontologist.

Having myself found the method I am about to describe convenient in many instances, more particularly in the case of fossil mammals, I have been led to believe, by the representations of several to whom it has been communicated, that it might be found useful by others, and consequently, though at first sight but a trifling matter, worthy of a place in the 'Proceedings' of the Society.

The characters afforded by the teeth are derived from their number, proportions (absolute and relative), and pattern.

In many cases the pattern of the teeth must undoubtedly be taken into account; but in a very great number it will be found that the number and proportions, more particularly of the premolars and molars, are sufficient for the purpose of diagnosis, or, at any rate, that a knowledge of these particulars alone will reduce the necessity for further comparison within a small compass. A good illustration of this is afforded in the smaller Felidæ, in which, owing to their high specialization, the pattern of the teeth is in the main so very closely alike as to render it of very little assistance in diagnosis, though not altogether.

The statement of the particulars above mentioned, in words or figures when numerous comparisons are needed, is tedious and laborious to both writer and reader; and even in the most carefully arranged tables it is difficult without close attention to perceive at once differences which though minute are, from their constancy, important and in fact necessary for the diagnosis of nearly allied forms.

My plan may be termed one for the graphic or diagrammatic representation of the absolute and relative or proportional dimensions and number of the premolar and molar teeth, or of those constituting the molar series, and which have appeared to me in most cases sufficient for the purpose in view. But of course the incisors and canines might be included in the scheme if thought requisite.

The method in which these "odontograms" are prepared will be at once obvious on inspection of the accompanying examples. Each horizontal line in the figures, which represent the maxillary and mandibular molar series of a species, corresponds to a single tooth, whose extreme length or antero-posterior diameter is indicated by the extent of the lighter shade, and its extreme breadth or transverse diameter by the darker shade. Both dimensions are, of course, measured from the same base-line.

The respective measurements, which may be taken with a pair of sharp-pointed caliper-compasses, having been pricked out upon the equidistant horizontal lines, the points showing the length and breadth of each tooth are connected by straight lines, and a sort of figure is thus obtained which, in nearly all cases, will be characteristic of the *genus* or *family*, and in many instances sufficient to determine the species also. In some cases, as for instance in *Canis* and *Viverra*, the odontograms are at first sight so nearly alike that recourse must be had to the pattern of the teeth in addition, as before alluded to.

In order to render figures of this kind easily comparable *inter se*, it is necessary that they should be drawn upon some common scale for the distance between the horizonal lines. This is, of course, entirely arbitrary, all that is requisite being that it should not be too great nor too small.

The accompanying odontograms are drawn upon a scale of ·25 inch = 6·35 mm., which appears convenient for the purpose; and is suitable for all teeth of the dimensions that readily admit of this mode of definition, that is to say, which are neither too large, as those of the Elephant, nor too small, as in the smallest mammals.

Moreover, if the figures are drawn upon ruled paper, the actual measurement of the size of the teeth can be read off at sight; and with this object I have employed paper ruled to a scale of ·05 inch.

The examples selected to show the application of the method above described have necessarily been limited to a very few. They include figures of the dentition of the Lion and Tiger, taken from the largest specimens of each species I have as yet met with; but they afford a fair illustration of the way in which even a slight specific difference is brought out, and which, in the case of these animals, is almost confined to the lower teeth.

The three odontograms of the genus *Ursus* represent the mean dimensions and proportions taken from numerous instances of each species, and they show at a glance the differences between them. In these the small anterior premolars have been purposely omitted to save space.

The odontograms of *Hyæna* are of the same kind.

The dentition of the genus *Canis* is exemplified by instances taken from the Wolf to the Fennec Fox, or from the largest to the smallest species, in order to illustrate the uniformity of the generic type throughout; and amongst these forms, two will serve to show how the method may be used in palæontological research. Plate IX. fig. 13 represents the dentition of the fossil Fox described by Messrs. Durand and Baker from the Siwalik Hills, and fig. 14 that of the existing *Canis bengalensis*, which would thus appear to be the close representative of its supposed miocene progenitor, a resemblance which further comparison of the skulls only serves to render still more obvious. The other figures are introduced merely to indicate the variety of forms produced in this way from the measurements of the teeth of different *genera*.

DESCRIPTION OF THE PLATES.

Plate VIII.

Fig. 1. *Felis leo* (max.).
2. —— *tigris* (max.).
3. —— *jubata* (mean).
4. *Ursus ferox* (mean).
5. —— *arctos* (mean).
6. —— *maritimus* (mean).
7. *Hyæna crocuta* (mean).
8. —— *brunnea* (mean).
9. —— *striata* (mean).

Plate IX.

Fig. 10. *Canis lupus.*
11. —— *aureus.*
12. —— *vulpes.*
13. —— *bengalensis* (fossilis).
14. —— —— (hodie).
15. —— *sarda.*
16. *Sus scrofa* (ferus).
17. —— *domesticus.*
18. *Equus caballus.*

XI. "Note on the Spectra of Erbia and some other Earths." By William Huggins, LL.D., F.R.S. Received May 26, 1870.

Bahr and Bunsen have shown* that erbia, rendered incandescent in a Bunsen's gas-flame, gives a spectrum of bright lines in addition to a brilliant continuous spectrum. As they were unable to discover the bright lines in the flame beyond the limits of the solid erbia, they suggest that the light which is dispersed by the prism into bright lines is emitted by the solid erbia, which substance therefore appears to stand alone, as a remarkable exception, among solid bodies. Bahr and Bunsen found the spectrum of bright lines to coincide very nearly with the absorption spectrum of some compounds of erbium.

A few weeks since, when in Ireland, I made the observation that the spectrum of the ordinary lime-light contains bright lines†. Dr. Emerson Reynolds, Director of the Laboratory of the Royal Dublin Society, kindly undertook to make experiments to ascertain from the position of the lines if they were due to the cylinder of lime, or to impurities contained in it.

Upon my return to town I made the following experiments; shortly after commencing them I received from Dr. Reynolds the account of his experiments, which, with his permission, I have added to this note.

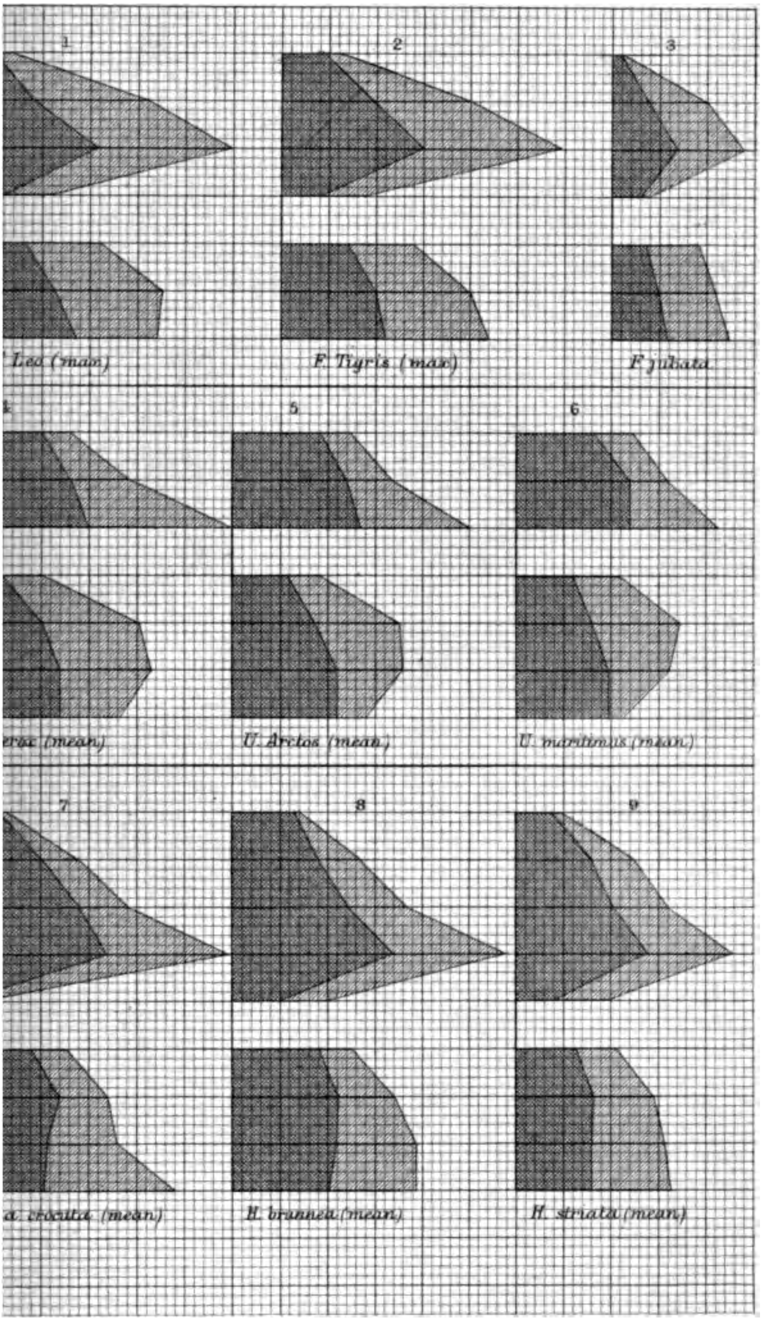
Erbia.—A few months since I received, through the kindness of Dr. Roscoe, F.R.S., a few grains of nitrate of erbia, which he had procured from a trustworthy source. I followed Bunsen's method of placing it with syrupy phosphoric acid upon a platinum wire. The erbia, obtained by this method in a finely divided state, was then submitted to the heat of the oxyhydrogen blowpipe.

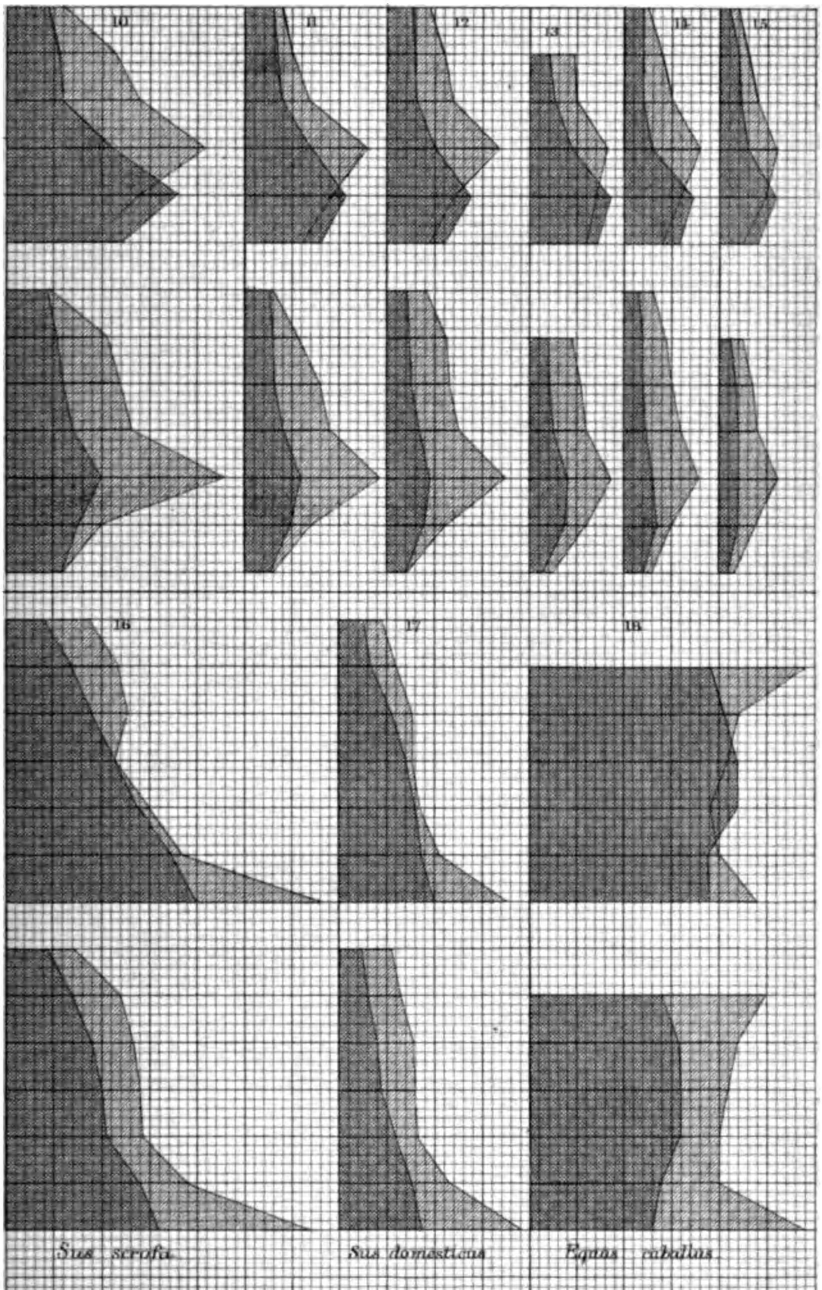
In all the experiments described in this paper hydrogen alone was first turned on, and the effect of the heat of the flame on the substance under examination observed with the spectroscope. Oxygen was then admitted slowly, and the effect of the increased heat carefully noted.

With the flame of hydrogen alone, the lines represented in the map

* Liebig's Annalen, Bd. lxi. (1866) S. 1.

† Dr. W. Allen Miller informs me that in 1845 he noticed a bright line in the spectrum of the diffused light of the oxyhydrogen jet reflected from a sheet of paper.


1
2
3
Leo (max)
F. Tigris (max)
F. jubata
5
6
rax (mean)
U. Arctos (mean)
U. maritimus (mean)
7
8
9
a. crocuta (mean)
H. brunnea (mean)
H. striata (mean)


10
11
12
13
14
15
16
17
18
Sus scrofa
Sus domesticus
Equus caballus

which accompanies Bahr and Bunsen's paper were seen, but the lines were more distinct when a small proportion of oxygen was admitted. With the full proportion of oxygen, the light from the glowing erbia was more intense, but the lines were not so well seen. Even with the intense heat of the oxyhydrogen flame I was unable to trace the lines beyond the limits of the solid erbia, though the line of sodium could be seen for some distance from the erbia. I found, however, that the lines appeared more distinct, in consequence, probably, of their being brighter relatively to the parts of the continuous spectrum where they occur, when the slit was directed from the side upon the gas immediately in front of the glowing part of the erbia.

The spectrum of bright lines obtained by means of the oxyhydrogen flame agreed more completely with the absorption spectrum represented by Bahr and Bunsen (No. 2 in their diagram) than the spectrum of bright lines figured by those observers (No. 3). The most important differences occurred in the band in the red, which showed two points of greatest brightness, thus forming a double line with a little outstanding light, and the line in the green at 65 of the scale, which was double, precisely as the corresponding absorption-line is represented in spectrum No. 2 of the diagram.

Lime.—The experiments were made with the cylinders of lime prepared for use with the oxyhydrogen blowpipe, and also with pieces of pure caustic lime, but there was no sensible difference presented in the spectroscope.

The bright lines consisted of a double line in the green, and several bands in the orange and red, which were found to form a spectrum identical with that which is produced when chloride of calcium is heated in the flame of a Bunsen's burner.

When the spectroscope was directed to a point in the flame a little above the incandescent portion of the lime, the lines appeared beyond the bright continuous spectrum, showing that they are not produced by the white-hot solid lime, but by the luminous vapour into which a portion of the lime has been converted by the heat of the flame.

Magnesia.—The commercial heavy oxide of magnesium was made into a paste with distilled water, and formed into a small pellet upon the end of a platinum wire. The pellet of magnesia was slowly dried, and then placed in the oxyhydrogen flame. I was surprised to see a spectrum of bright lines precisely similar to that which is produced by lime. Chloride of magnesium, when introduced into the Bunsen flame, gave a similar spectrum. I record these results as the oxide and chloride were those sold as pure. I found afterwards that a very small trace of lime may be detected in magnesia by means of the oxyhydrogen flame.

I then took metallic magnesium, which I had found by the spectroscope to be nearly pure, and formed from it magnesia and chloride of magnesium. When this magnesia, formed into a small ball upon a wire, was sub-

jected to the oxyhydrogen flame, two bright bands were seen in the green. One of these was found to be coincident with the triple line of Fraunhofer's *b*, which distinguishes magnesium, and the other with a group of bright lines which is seen between *b* and F, nearly in the position of the brightest double line of nitrogen, when metallic magnesium is burnt in air.

The chloride formed from magnesium, when introduced into the Bunsen flame, gave the same bands, but the more refrangible band was exceedingly faint.

When an induction-spark was taken from a wire covered with cotton-wool soaked with a solution of the chloride, the lines at *b* and the more refrangible group were seen. If the heating-power of the spark be increased by the introduction of a Leyden jar, the band between *b* and F becomes scarcely distinguishable, while the lines peculiar to metallic magnesium are much more intense. When a spark is taken between electrodes of the same specimen of magnesium from which the chloride was formed, no trace of this band was detected.

Baryta.—When pure caustic baryta is subjected to the heat of the oxyhydrogen flame, a brilliant spectrum is seen identical with the well-known spectrum which presents itself when chloride of barium is heated in the Bunsen flame. Baryta furnishes a larger quantity of vapour than lime and magnesia, and therefore the lines could be traced to a greater distance from the solid baryta.

Strontia.—Pure strontia was fused into a large bead upon a platinum wire. When this bead was heated by the oxyhydrogen flame, the same spectrum of bright lines presented itself as is seen when chloride of strontium is placed in the flame of a Bunsen's burner.

Zirconia.—One of the small pellets of zirconia prepared in France for use with the oxyhydrogen blowpipe was found to give no trace of bright lines. This great fixity of zirconia as compared with lime is in agreement with the inalterability of the substance under the action of the oxyhydrogen flame.

Alumina.—Pure alumina treated in the same way as the magnesia gave a continuous spectrum only, without any trace of bright lines.

Glucina.—Glucina gave a bright line in the red, which I found to be due to potassium. Glucina, therefore, appears not to form vapour of any kind under the heat of the oxyhydrogen blowpipe.

Titanic acid gave a continuous spectrum without lines.

Oxide of uranium a continuous spectrum without lines.

Tungstic acid a continuous spectrum without bright lines.

Molybdic acid a continuous spectrum without bright lines.

Silica (precipitated) a continuous spectrum without bright lines.

Oxide of cerium a continuous spectrum without bright lines.

The question presents itself as to the nature of the vapour to which the bright lines are due in the case of the earths, lime, magnesia, strontia, and baryta. Is it the oxide volatilized? or is it the vapour of the metal reduced by the heat in the presence of the hydrogen of the flame? The experi-

ments show that the luminous vapour is the same as that produced by the exposure of the chlorides of the metals to the heat of the Bunsen gas-flame. The character common to these spectra of bands of some width, in most cases gradually shading off at the sides, is different from that which distinguishes the spectra of these metals when used as electrodes in the metallic state*.

Roscoe and Clifton have investigated the different spectra presented by calcium, strontium, and barium, and they "suggest that at the lower temperature of the flame or weak spark, the spectrum observed is produced by the glowing vapour of some compound, probably the oxide, of the difficultly reducible metal; whereas at the enormously high temperature of the intense electric spark these compounds are split up, and thus the true spectrum of the metal is obtained. In none of the spectra of the more reducible alkaline metals (potassium, sodium, lithium) can any deviation or disappearance of the maxima of light be noticed on change of temperature"†.

As the experiments recorded in this paper show that the same spectra are produced by the exposure of the oxides to the oxyhydrogen flame, Roscoe and Clifton's suggestion that these spectra are due to the volatilization of the compound of the metal with oxygen is doubtless correct.

The similar character of the spectrum of the bright lines seen when erbia is rendered incandescent would seem to suggest whether this earth may not be volatile in a small degree, as is the case with lime, magnesia, and some other earths. The peculiarity, however, of the bright lines of erbia, observed by Bahr and Bunsen, that they could not be seen in the flame beyond the limits of the solid erbia, deserves attention. My own experiments to detect the lines in the Bunsen gas-flame, even when a very thin wire was used, so as to allow the erbia to attain nearly the heat of the flame, were unsuccessful. The bright line in the green appears, indeed, to rise to a very small extent beyond the continuous spectrum, but I was unable to assure myself whether this appearance might not be an effect of irradiation.

It is perhaps worthy of remark that the chlorides of sodium, potassium, lithium, cæsium, and rubidium give spectra of defined lines which are not altered in character by the introduction of a Leyden jar, and which, in the case of sodium, potassium, and lithium, we know to resemble the spectra obtained when electrodes of the metals are used. Now all these metals belong to the monad group; it appeared therefore interesting to observe the behaviour of the other metal belonging to this group.

Chloride of silver when introduced into the Bunsen flame gave no lines. The chloride was then mixed with alumina, which had been found to give a continuous spectrum only, and exposed to the oxyhydrogen

* For the spectra of metallic strontium, barium, and calcium, see Phil. Trans. 1864, p. 148, and Plates I. and II. Both forms of the spectra of these substances are represented by Thalen in his 'Spektralanalys.'

† Roscoe's Spectrum Analysis, p. 175, and Proc. Lit. & Phil. Soc. Manchester, April 1, 1862. See also A. Mitscherlich, 'Ueber die Spectren der Verbindungen,' S. 10.

flame, but no lines were visible. When, however, the moistened chloride was placed on cotton and subjected to the induction-spark without a jar, the true metallic spectrum was seen, as when silver electrodes are used.

The behaviour of silver, therefore, is similar to that of the other metals of the monad group. Now the difference in basic relations which is known to exist between the oxides of the monatomic and polyatomic metals would be in accordance with the distinction which the spectroscope shows to exist in the behaviour of the chlorides; the chlorides of the polyatomic metals would be more likely to split up in the presence of water into oxides and hydrochloric acid.

In the case of some of the oxides and chlorides, one or more of the lines appeared to agree with corresponding lines in the metallic spectra; it may be, therefore, that under some circumstances, as in the case of magnesium burning in air, the metallic vapour and the volatilized oxide may be simultaneously present.

Dr. Reynolds's Experiments.

"After you observed the occurrence of two bright lines in the spectrum of the light emitted by incandescent lime, you recollect we identified these as belonging to calcium. At the time we supposed that these lines were produced by the ignition of the vapour of some volatile calcium compound probably present as an impurity in the sample of limes used in the experiments. If this explanation was found to be true for lime, the bright lines seen in the spectrum of erbia might possibly be accounted for in a similar manner. In order to examine the matter fully, I arranged the experiments described below.

"I selected two oxides for comparison with erbia, viz. lime and magnesia. As it seemed desirable to prepare these oxides in precisely the same manner as the erbia, some calcium and magnesium nitrates were made chemically pure to ordinary tests, and then used in the preparation of the respective oxides.

"The oxyhydrogen flame was employed as the chief source of heat. The hydrogen was made from zinc and sulphuric acid in the usual way, and the oxygen from potassium chlorate. As both gases are certain to be contaminated with traces of acids, I took the precaution of passing each gas through a long tube filled with fragments of solid potassium hydrate. If this plan were not adopted, the traces of acid which would find their way into the hydrogen or oxyhydrogen flame might produce volatile compounds with the earths, and so lead to mistakes.

"1. *Experiments with Magnesia.*—A loop of stout platinum wire was moistened with syrupy phosphoric acid, and some magnesium nitrate made to adhere. The nitrate was then heated in the hydrogen flame, and a residue of magnesia obtained. No lines were observed in the spectrum of the light emitted by the incandescent earth, and when the latter was in-

tensely heated in the oxyhydrogen jet only a continuous spectrum was seen*.

"2. *Experiments with Lime.*—A platinum wire of the same thickness as the last was moistened with the phosphoric acid, some calcium nitrate was then taken up in the loop, and heated in the hydrogen flame until a residue of lime was obtained. At the outset the calcium-spectrum was observed, but the light speedily gave only a continuous spectrum. The lime and loop of wire were kept well enveloped in the hydrogen flame for nearly half an hour in order to ensure the complete decomposition of the nitrate. During this time no lines could be detected on the background of the continuous spectrum, or in the spectrum of the flame surrounding the lime. More hydrogen was now turned on and oxygen slowly admitted, the light being examined with the spectroscope during the time. When the proportion of oxygen had reached a certain point, faint traces of the two brightest Ca lines appeared on the bright background, and the intensity of these lines increased with the amount of oxygen admitted up to a definite extent. When a certain proportion of oxygen was exceeded, the lines became less distinct. The best results were obtained when the hydrogen was decidedly in excess of the oxygen in the flame, that is to say, more than in the proportion of 2 : 1.

"When the slit of the spectroscope was pointed in such a way that only the light from the flame surrounding the incandescent lime entered the instrument, all the Ca lines and bands were observed with great ease without a continuous spectrum. On looking at the mantle of flame with the naked eye it was easy to perceive a reddish tinge. I next maintained the small fragment of lime at the highest temperature its supporting wire was capable of resisting for *three hours*; at the end of this time the Ca lines were as strongly marked as before, and the lime on the wire had very appreciably diminished in amount. The same results were obtained when no phosphoric acid was employed to attach the calcium nitrate to the wire in the first instance.

"Again, a piece of well-burned quicklime, of very small size, was heated alone on a platinum wire for more than an hour, and the bright Ca lines were seen during the whole time.

"From the results of these experiments, we must draw the conclusions (1) that when lime is sufficiently heated the light which it emits is derived in part from the incandescent solid, and partly from ignited vapour; (2) that lime is either volatile *as such*, or that in the first instance it suffers reduction by the excess of hydrogen in the flame, the luminous vapour of calcium then giving its own peculiar spectrum.

"3. *Experiments with Erbia.*—The specimen of erbium nitrate which

* "Since writing the above, I have succeeded in observing the bright lines described by Mr. Huggins as occurring in the spectrum of the flame surrounding the incandescent magnesia. In the earlier experiments I probably admitted too much oxygen to the mixed gas-flame in the first instance."

you kindly gave me was attached to a platinum loop with syrupy phosphoric acid as usual, and decomposition of the salt effected in the plain hydrogen flame. After heating for a short time in this way, the chief green line of erbia became visible, but seen upon the continuous spectrum. Oxygen was now turned slowly into the flame. As the temperature rose, two of the other bright lines of the earth were seen. The best observations were made when the oxyhydrogen flame had hydrogen in excess, and the erbia was kept in such a position that it was very strongly ignited. The erbia lines were most distinctly seen when the slit of the spectroscope took in the light from the extreme edge of the incandescent solid. When the bright lines were best observed, the continuous spectrum was relatively faint. Again, when the slit was made to cut the edge of the ignited bead of the earth, the strong green line of erbia was seen to extend to a very small but appreciable distance above or below (as the case might be) the continuous spectrum. I could only observe this for the strong line. I failed to get any trace of lines in the spectrum of the flame beyond the incandescent erbia.

"The erbia was next heated in the oxyhydrogen flame to the maximum temperature that the wire would bear for *three and a half hours*, but the green line was seen to be just as strongly marked at the end as at the beginning of the experiment. The bulk of the erbia was so much reduced by this treatment, that I have now scarcely a trace left.

"From the results of these experiments, I think we must conclude (1) that the light emitted by incandescent erbia is derived chiefly from the ignited solid, but that the bright lines observed in its spectrum have as their source a luminous vapour of extremely low tension at even the highest temperature of the oxyhydrogen flame; (2) that this interrupted spectrum belongs either to erbium or to its oxide.

"If these conclusions are true, it follows that erbia is not an exception to the ordinary law.

"It would appear that in these experiments three substances have been employed, varying in their degree of volatility. At the temperature of the oxyhydrogen flame magnesia appears to be less volatile than lime; but I am in doubt what relative volatility to assign to erbia, since its spectrum of bright lines can be seen when the earth is heated in the plain hydrogen flame, and yet at the much higher temperature of the oxyhydrogen jet the volume of luminous vapour does not appear to materially increase.

"Finally, we have yet to learn whether or not in all these cases reduction of the oxide precedes volatilization; if reduction takes place, the luminous vapour must be that of the metal. The settlement of this question would no doubt be very difficult. But I rather incline to the view that the vapour whose spectrum is obtained on igniting these earths is that of the metal; for I find that the bright lines are most easily observed when hydrogen is present in excess in the oxyhydrogen flame. Moreover, the actual amount of matter volatilized on very prolonged heating is really very

small, and this circumstance appears to favour the view that a slow surface-reduction is in progress."

XII. "On the Values of the Integrals $\int_0^1 Q_n, Q_{n'}, d\mu$, Q_n, $Q_{n'}$ being Laplace's Coefficients of the Orders n, n', with an application to the Theory of Radiation." By the Hon J. W. STRUTT, Fellow of Trinity College, Cambridge. Communicated by W. SPOTTISWOODE, F.R.S. Received May 17, 1870.

(Abstract.)

These integrals present themselves in calculations dealing with arbitrary functions on the surface of a sphere which vary discontinuously in passing from one hemisphere to the other. When n, n' are both even or both odd, the values of the integrals may be immediately inferred from known theorems in which the integration extends from -1 to $+1$, or over the whole sphere; otherwise a special method is necessary. In the present paper a function of two variables is investigated, which, when expanded, has for coefficients the quantities in question. As an example of the method, the problem is taken of a uniform conducting sphere exposed to the heat proceeding from a radiant point. It will appear at once that the heat received by any element of the surface is expressed by different analytical functions on the two hemispheres—a source of discontinuity which renders necessary a special treatment of the problem. The solution is afterwards generalized to meet the case of a sphere exposed to any kind of radiation from a distance.

One remarkable result not confined to the sphere is, that the effect of a radiation which is expressed by one or more harmonic terms of odd order is altogether nil, with the single exception of the term of the first order.

XIII. "Note on the Construction of Thermopiles." By the EARL OF ROSSE, F.R.S. Received June 14, 1870.

Although in the measurement of small quantities of radiant heat by means of the thermopile much may be done towards increasing the sensibility of the apparatus by carefully adjusting the galvanometer and rendering the needle as nearly astatic as possible, there must necessarily be some limit to this, and it therefore appears desirable that the principles on which thermopiles of great sensibility can be constructed should also be carefully attended to.

With the view of obtaining a pair of thermopiles of greater sensibility and of more equal power than I had been able to procure ready made, I made a few experiments with various forms of that instrument, and I was led to the conclusion (one which might have been foreseen) that the

sensibility of the thermopile is much increased by reduction of its mass, and more especially by a diminution of the cross section of the elements.

To obtain a clear idea of the problem before us, which is, how to construct the thermopile so that, with a given amount of radiant heat falling on its face, the greatest current may be sent through the galvanometer, let us consider the thermopile under two different conditions :—

1. With the circuit open.
2. With the circuit complete.

In the first case, when radiant heat falls on the face of the pile, the whole mass of metal rises in temperature, the rise being greatest at the anterior face, and less and less as you approach the other end. This rise of temperature will increase till the heat radiated from the anterior face, together with that which traverses the depth of the pile and is radiated from the posterior face, is just equal to that radiated to the anterior face at that moment, or when

$$k(t+t')=kt+\frac{sc}{l}(t-t')=Q,$$

where (t, t') are respectively the temperatures of the anterior and posterior face, s, l the cross-section and depth of the pile, c proportional to the mean conductibility of the material of the pile, (Q) the quantity of heat falling on the pile in a unit of time, and (k) a constant.

Let us now suppose the circuit completed, and we shall have, in addition to the above, two causes operating to reduce the temperature of the anterior face,—the abstraction of heat *by* the electric current, and *proportional* to that current $=LI$, where I is the intensity of the current and L a constant, then there will be equilibrium when

$$k(t+t')+LI=kt+\frac{sc}{l}(t-t')+LI=Q.$$

It is quite clear therefore that if Q be constant, I will become the larger the smaller the other two terms become ; and therefore as long as the first term continues small compared with the remaining terms, and the resistance in the pile is very small compared with that in the rest of the circuit, we shall increase the intensity of the current by every reduction of the cross-section of the elements of the thermopile.

There is another point which, though less important, cannot be entirely lost sight of, namely, that the more we reduce the mass of the anterior face and adjacent parts of the pile, the more rapidly will the temperature rise to its state of equilibrium, and therefore the more convenient will it be for use where the needle is liable to disturbances from various causes, and where consequently the more speedily the needle can be brought to rest, the more accurately will its observed motion be a measure of the radiant heat falling at that moment on the face of the pile.

Let us now compare the case of a single pair of small cross-section with a metal disk soldered to the junction of the two bars, and of suffi-

cient size to catch all the radiant heat required to be measured, with that of a pile of (n) pairs, each of equal dimensions with those of the single pair, the area of face being the same in the two cases.

By increasing the number of elements from one to n, we increase the number of solderings in that proportion; consequently the average amount of heat reaching any soldering is $\frac{1}{n}$ as great as that reaching the soldering of the single pair; therefore, if the same percentage of the total heat be lost by conduction, the total electromotive force is the same in the two cases. But inasmuch as the total cross-section of metal to conduct the heat away from the anterior face is n times as great in the pile as in the pair, and the resistance of the pile is n times as great as that of the pair, the pile will be inferior in power to the pair, unless these two causes of inferiority are counterbalanced by the loss due to the greater average distance to the soldering from the points where the heat reaches the face, in the case of the pair, than that of the pile of n pairs.

The experiments already referred to were made with three different thermoelectric pairs. These consisted each of a pair of bars of bismuth and an alloy of twelve parts of bismuth and one part of tin of different thicknesses, of about equal lengths in each case, and soldered about ¼ inch apart upright, on disks of sheet copper of ½ inch diameter. A slip of wood was placed between the two bars, to protect them from injury, and to which they were fixed with thread. The three piles were compared with a pile of four elements, made by Messrs. Elliott, and the deviation due to the latter being taken equal to unity, the following deviations were obtained for the three thermo-pairs:—

	Weight of disk face.	Weight of two bars.	Deviation.	Metals employed.
I.	8 grains	42 grains	·676	Bismuth, antimony.
II.	4½ "	6 "	1·35	Bismuth { Bismuth, tin $\frac{1}{12}$. }
III.	½ grain	3 "	3·23	Bismuth { Bismuth, tin $\frac{1}{12}$. }

A heavy and a light pile were also compared, taking the interval between raising and depressing the screen, first $=\frac{1}{2}$ minute, and then $=2$ minutes; and it was found that, in the first case,

$$\frac{\text{Deviation due to light pair}}{\text{Deviation due to heavy pair}}=2{\cdot}6;$$

and, in the second case,

$$\frac{\text{Deviation due to light pair}}{\text{Deviation due to heavy pair}}=2{\cdot}9;$$

so that the light pair arrived rather more rapidly at the condition of equilibrium than the heavier pair.

Although the above experiments are far less complete than I could have wished, they are sufficient to show that the sensibility of thermopiles may be considerably increased by diminution of the section of the bars composing them; whether they may be with advantage reduced to a greater extent than I have already done I cannot say, but I am inclined to think that they may. I have ascertained from Messrs. Elliott that the alloys used by them in the construction of thermopiles, at the time when I received mine from them, were 32 parts of bismuth +1 part of antimony, and 14½ of bismuth +1 part of tin. If allowance be made for the substitution of the first of these two alloys for pure bismuth, the difference between Elliott's pile and the pairs II. & III. will be rather greater. The pile by Messrs. Elliott, if made of the same metals as I employed, would have been reduced in power from 1 to 0·9.

The construction of thermo-couples, on the plan I have described, is comparatively easy. In about two hours I was able to make one, and in more experienced hands their construction would be still easier.

An experiment was made with one of the piles to ascertain whether, when the heat was not directed centrally on the pile, much diminution of power would take place. There *was* less deviation in consequence of the increase of the mean distance which the heat had to travel before it reached the soldering; but I believe that this defect might be remedied, probably without diminution of the power of the pile, by increasing the thickness of the face, and leaving the dimensions of the bars the same.

INDEX TO VOL. XVIII.

END OF THE EIGHTEENTH VOLUME.

PRINTED BY TAYLOR AND FRANCIS,
RED LION COURT, FLEET STREET.

OBITUARY NOTICES OF FELLOWS DECEASED.

Jean Victor Poncelet, Foreign Member of the Royal Society, was born at Metz on the 1st of July, 1788. After having studied Mathematics for two years at the Lycée Imperial of Metz, he was admitted to the École Polytechnique, where he remained till 1810, his studies in the meanwhile having been interrupted by a serious illness. He then entered the École d'Application of Metz as Sublieutenant of Engineers, and left it in March 1812, in order to assist in constructing the defensive works of Ramekens in the island of Walcheren. His first engineering work here was the erection of a casemated fort in a very limited time, on a peat soil, without having at his command proper materials for a foundation. In the month of June 1812 he was called away to take part in the Russian campaign, and joined the invading army at Vitepsk. On the 18th of August he reconnoitred Smolensk, exposed to the fire of the garrison, and afterwards took an active share in the battle fought the same day. On the 19th he was employed in throwing bridges over the Dnieper below Smolensko, under the fire of the Russian batteries on the opposite bank of the river. Deceiving the enemy by an ostentatious display of preparations for crossing at a particular spot, he succeeded in constructing bridges at other points better protected from the Russian fire. During the retreat from Moscow, at Krasnoi, not far from Smolensko, seven thousand French soldiers under the command of Ney, without artillery, encountered twenty-five thousand Russians with forty-five pieces of artillery, under Prince Miloradowich, on the 18th of November, 1812. In this battle Poncelet charged the Russian batteries at the head of a column of sappers and miners; his horse was killed under him, and he was taken prisoner. After a painful four months' march through snow, half naked and ill fed, in a season when mercury was repeatedly frozen, he arrived at Saratoff on the Volga. In April 1813, on recovering from an illness brought on by the hardships he had endured, he resolved to occupy his unwelcome leisure with the study of descriptive geometry. But his recollections of the teaching of Monge, Carnot, and Brianchon had been totally effaced by the privations and sufferings he had undergone. Without books to aid him he was obliged, with much labour, to construct bit by bit the elementary propositions required for the line of research he was desirous of following. The results of his labours at this time were afterwards published in Gergonne's 'Annales,' from 1817 to 1821; and the original manuscripts, written at Saratoff, were published in 1862. On the conclusion of peace in June 1814, he quitted Saratoff for France, where he arrived in September of the same year. From 1815 to 1825, as Captain of Engineers, he superintended the construction of machinery in the arsenal of Metz. From 1825 to 1835 he was Professor of Mechanics; and while he imparted to the young officers clear ideas of mechanical science, capable of immediate practical application, he delivered, at the suggestion of Baron Dupin, gratuitous evening

Committee for constructing the Fortifications (
appointed Professor of Mechanics at the Sorl
France; became Lieutenant-Colonel in 1841, (
of Brigade on the 19th of April, 1848. A
pointed Governor of the École Polytechnique
1850. During the troubles of June 1848, pl
the pupils of the École Polytechnique, he led
to the Luxembourg, where they formed a gı
tection of the Provisional Government. For
ral Cavaignac appointed him to the comman
the Department of the Seine. He was also el
stituent Assembly. As President of the Sc
the English Exhibition of 1851, he drew up a
the Arts involving the application of Scienc
tury. Besides the works already mentione
of Memoirs and Scientific Reports in the M
du Comité des Fortifications, and the Compt
Officier of the Legion of Honour, Chevalier of
Mérite," Corresponding Member of the Academ
Turin, and of many other learned Societie
Member of the Royal Society took place in
painful illness, he died in Paris on the 23rd of

NATHANIEL BAGSHAW WARD, F.R.S., F.L
1868 in his 77th year, was a sonnd practical
known as the inventor of the closely glazed ca
which bear his name. When quite young he

vation of plants; and his house in Wellclose Square was conspicuous for the vegetation which surrounded it. But the deleterious atmospheric influences to which it was exposed subjected him to continual vexation and disappointment; and the only way in which he could maintain a fluctuating appearance of freshness and verdure was by bringing back a renewed supply of plants on the occasion of any visit to nursery grounds or the country. In the summer of 1829, a solution of his difficulties presented itself. He had placed the chrysalis of a moth in some mould in a glass bottle covered with a lid, in order to obtain a perfect specimen of the insect; after a time a speck or two of vegetation appeared on the surface of the mould, and turned out to be a fern and a grass. His interest was excited; he placed the bottle in a favourable situation and found that the plants continued to grow and to maintain a healthy appearance. On reflecting upon the matter, he found that the conditions necessary to the life of the plants were maintained, and deleterious agents, as soot, noxious gases, drying winds, &c., were excluded. The first Wardian case gave rise to numerous others; and in two or three years the success of the plan was satisfactorily demonstrated. In 1836 Mr. Ward wrote upon the subject to the late Sir W. Hooker; and the letter was published in the Companion to the Botanical Magazine for May of that year. In 1838 Mr. Faraday lectured upon the "cases" at the Royal Institution; and subsequently Mr. Ward himself explained his plan at various Societies and at Meetings of the British Association. In 1842 the first edition of Mr. Ward's work on "The Growth of Plants in Closely-glazed Cases" was published; and a second edition appeared a few years later.

It was soon recognized that Mr. Ward's method was susceptible of various valuable applications, of which the following may be noticed:—1. The growth of plants in the dwellings of all classes, in town as well as country. 2. The transport of plants to and from different countries: the tea-plant and the cinchona-tree have by means of the Wardian cases become established in India. 3. For purposes of philosophical investigation. 4. To the study and conservation of animals: the Vivaria were first established as a modification of the cases by Mr. Ward himself.

When residing at Wellclose Square, Mr. Ward gave frequent microscopical soirées. Out of these sprang the Microscopical Society in 1840, Dr. Bowerbank and the late Messrs. Quekett and Jackson having also taken part in its foundation. He was elected a Fellow of the Royal Society in 1852. In 1856, a large number of friends combined to recognize Mr. Ward's services by having his portrait painted by J. P. Knight, R.A., and placed in the meeting-room of the Linnean Society.

The estimation in which the subject of this notice and his scientific services were held by those best capable of forming an opinion, will be shown by the following extracts from a letter written by Dr. Hooker to the editors of a scientific journal:—

"During the whole period that I knew Mr. Ward, and, I believe, for

many years before, his hospitable house, first in Wellclose Square, and afterwards at Clapham Rise, was the most frequented metropolitan resort of naturalists from all quarters of the globe of any since Sir Joseph Banks's day. His unpretending entertainments were frequent, for many years periodic, and often weekly. On these occasions his many scientific friends flocked to see himself, his live plants, and the many specimens, instruments, and preparations he had collected to instruct and entertain them; and on such occasions it was that many a country and colonial naturalist was introduced for the first, and too often for the last time in his life, to some of the most eminent naturalists in Europe.

"Of the value of that contrivance which justly bears his name, the Ward's case, it is impossible to speak too strongly; and I feel safe in saying that without its aid a large proportion of the most valuable economic and other tropical plants, now cultivated in England, would not yet have been introduced." "Of even more consequence was the application of these cases to town gardening, whereby he has afforded to the denizens of this metropolis far greater and purer pleasures than all artists, house-decorators, &c. have contributed; for a primrose placed in flower under a bell-glass at Christmas in a London drawing-room will charm when a Raphael does not, and will charm none the less when a Raphael charms also." "In the memory of all who knew him, Mr. Ward will live as a type of a genial, upright, and most amiable man, an accomplished practitioner, and an enthuiastic lover of nature in all its aspects."

Mr. Robert Porrett was born on the 22nd September 1783. His father held the office of Ordnance Storekeeper in the Tower, and resided there; and the son, having early shown an aptitude for such a situation, was employed as his father's assistant, and, succeeding him in his appointment, eventually rose to be chief of the department. His official work, not being of an engrossing nature, left him leisure to apply his intelligent and inquiring mind to scientific pursuits, especially to chemistry; and inasmuch as he received a medal from the Society of Arts for a chemical discovery in 1809, he was probably at the time of his death the oldest representative of experimental chemistry in this country. In fact he was a worker in chemistry before the introduction of the atomic theory, and was among the first to apply the new doctrine to the verification of chemical analysis. Mr. Porrett's earliest researches were on hydro-ferrocyanic and hydrosulphocyanic acids, of which he was the discoverer. The investigation of the constitution of these acids (which he named *ferruretted* and *sulphuretted chyazic acids*) and of their salts forms the subject of various papers which he contributed between 1809 and 1819 to the 'Philosophical Transactions' and other scientific publications: they are as follows:—"On Prussic and Prussous Acid," Trans. of the Soc. of Arts, vol. xxvii. 1809, p. 89; "On the Nature of the Salts termed triple Prussiates," &c., Phil. Trans. 1814, p. 527; "Further Analytical Experiments relative to the Constitution

of the Prussic, of the Ferruretted Chyazic, and of the Sulphuretted Chyazic Acids, and on that of their Salts; together with the application of the Atomic theory to the analysis of these Bodies," Phil. Trans., 1815, p. 220; "On the Anthrazothion of Von Grotthus, and on Sulphuretted Chyazic Acid," Thomson's Annals of Philosophy, vol. xiii. (1819) p. 356; "On the Triple Prussiate of Potash," Ann. Phil. vol. xii. p. 214, which contains a discussion of his own analyses of "ferruretted chyazic acid," and that of Dr. Thomson, published in a previous part of the same volume; "On Ferrochyazate of Potash, and on the Atomic weight of Iron," Ann. Phil. vol. xiv. 1820, p. 205.

In 1813 Mr. Porrett was engaged with Messrs. Wilson and Rupert Kirk in an investigation of chloride of nitrogen, with a view chiefly to the examination of the physical properties and chemical composition of that dangerously explosive compound, and the discovery of safe and suitable processes for preparing it.

In 1816 he communicated to Thomson's Annals of Philosophy, vol. viii. p. 74, an account of "Two Curious Galvanic Experiments," in which he showed that a fluid is made to pass against gravity by the electric current through a membrane from the positive to the negative pole when the conducting wires of a battery are connected with water placed at different levels on each side of the membrane. The fact so discovered by him is by German writers generally associated with his name as "das Porrettsche Phänomen." He also described the increase of action which is produced in an exhausted voltaic battery by removing a portion of the fluid, whereby the still moist plates are exposed to the action of the air. In 1817 he made some "Observations on the Flame of a Candle," which were published in the 'Annals of Philosophy,' vol. ix. p. 337.

After an interval of twenty-six years, he again, in 1846, at the age of sixty-three, took up chemical investigation, and contributed, in conjunction with the late E. F. Teschemacher, a paper "On the Chemical Composition of Gun Cotton" (Memoirs of the Chemical Society, vol viii. 1845–1848, p. 258). His last paper "On the existence of a new Vegeto-Alkali in Gun Cotton," for which he proposed the name of Lignia, was read before the Chemical Society on December 21st of the same year, and is printed in the Memoirs, vol. iii. p. 287.

While devoting his leisure time principally to chemistry, Mr. Porrett also occupied himself with antiquarian pursuits, especially the study of ancient arms and armour, for which his residence in the Tower afforded favourable opportunity. He retired from official duty in 1850, after a service of 55 years. On that occasion his long and useful service was honourably recognized by his superiors, and he received most gratifying expressions of regard and attachment from his subordinate officers. He was a Fellow of the Astronomical and Antiquarian Societies, and one of the original members of the Chemical Society; his election into the Royal Society was in 1848. He died on the 25th November 1868, at the age of 85.

Carl Friedrich Philipp von Martius, Foreign Member of the Royal Society, was born on the 17th of April, 1794, at Erlangen, where his father, Ernst Wilhelm Martius, was Court Apothecary, and Honorary Professor of Pharmacy in the University. The family is said to have come from Italy, but had for several generations been settled in Germany. After a careful and judicious training at home, for which he was indebted chiefly to an intelligent and accomplished mother, young Martius received his general education in the school and the gymnasium of his native town. From his father he had inherited a taste for Natural History; and under the tuition of Professors Richter and Besenbeck, of the Gymnasium, he acquired a well-grounded knowledge of classical literature; so that a good foundation was laid for that well-balanced general mental culture of which the fruits are conspicuous in his writings. When not quite sixteen years of age, he entered the University of Erlangen. His main object was the study of medicine, but he also followed his early bent towards Natural History, and especially Botany. The Botanical Professor of that day was Schreber, who had himself studied under Linnæus; but Martius's attachment to the science was greatly fostered and promoted by the friendship of the brothers Nees von Esenbeck, then his fellow students, who afterwards rose to eminence as botanists. From the elder of the brothers Martius also received a tincture of the then prevalent "Natur-Philosophie," which may be perceived to colour his earlier writings; but its influence seems to have been but transient.

In March 1814 he was promoted with distinction to the degree of Doctor of Medicine, and published his inaugural dissertation under the title "Plantarum Horti Academici Erlangensis Enumeratio," a critical catalogue of plants arranged according to the Linnean system.

An event had happened some time before which decided Martius's future career. The Academy of Sciences of Munich, on the death of Schreber, sent to Erlangen two of its members, Schrank and Spix, to acquire his botanical collections for the Academy; and these naturalists, having seen the promise of future excellence evinced by the young man, invited Martius to apply for admission into the "Institution of Elèves," then existing in the Academy, in which the pupils had the advantage of following out the higher study of selected branches of science under the auspices of the Academy and the immediate guidance of certain of its members. Through the prospect thus set before him, the wish which Martius had already entertained of devoting himself entirely to botany, became a settled resolution. After going through the prescribed trials, he was in May 1814 received among the Elèves of the Academy, and appointed, under the direction of Schrank, now advanced in years, to be assistant in the management of the Botanic Garden at Munich, with an annual salary of 500 florins. Two years later he was advanced to the rank of "Adjunct of the Academy," an order which no longer exists, having been abolished, together with the Institute of Elèves, by King Louis in 1827.

Martius not only laboured zealously in the superintendence of the Garden, but made frequent excursions through Bavaria and the adjacent regions for the study of the indigenous flora ; and it was on one of these occasions that he made the friendship of Hoppe, the Director of the Botanic Garden of Ratisbon, and began with him a scientific correspondence which was long-enduring. At this time he published the 'Flora Cryptogamica Erlangensis' (Norimbergi, 1817), which contained the results of his first independent researches, and met with high approval from his fellow workers in the science. His earnest devotion to study, his conspicuous talents, and his untiring activity, could scarcely fail to earn for him the regard of his older academical colleagues, such as Schrank, Schlichtigroll, Sœmmerring, and the Conservator General von Moll—all of them men fitted to produce a beneficial influence on his mental development. In like manner he attracted the kindly notice and favourable consideration of the King Maximilian Joseph I., who, being a great lover of plants, paid frequent visits to the Garden under the welcome guidance of the young superintendent ; and this had an important effect on his future fortune.

This enlightened prince had for some time entertained the project of sending a scientific expedition to America ; and as the Emperor of Austria was about to send out scientific explorers to Brazil in the retinue of the Archduchess Leopoldina, who was about to sail for that country as the bride of the Crown Prince of Portugal, afterwards the Emperor Don Pedro I. of Brazil, King Max. Joseph availed himself of the opportunity offered to him of sending out two Bavarian naturalists on that occasion. The choice fell on Spix as Zoologist, and Martius as Botanist, who was selected by the king himself. After but a brief time allowed for equipment, the two travellers sailed from Trieste on the 2nd of April, 1817, with the imperial cortége, and, after touching at Malta, Gibraltar, and Madeira, arrived at Rio Janeiro on the 15th of July. There they parted from the Austrian savans, and set out on their own journey.

It is unnecessary here to trace the course of their travels ; suffice it to say that, after traversing the vast territory of Brazil in various directions, and ascending the River Amazons and its tributary the Hyapurá as far as the confines of Peru and New Granada, they arrived at Pará on their return journey on the 16th of April, 1820, three years after they had sailed from Europe. From Pará they were conveyed to Lisbon in a Portuguese ship of war, and reached Munich on the 8th of December, 1820.

This expedition, irrespective of the sea voyage, extended over nearly 1400 geographical miles, and for months led through the most inhospitable and dangerous regions of the New World. Both explorers, however, escaped without any important disaster on the road, and they had the rare good fortune to preserve and bring home their collections complete and un injured.

The material fruits of the expedition consisted of about 6500 species of plants, the majority dried ; but several living species, as well as seeds, were

also brought home. The zoological collections (to which Martius contributed also on his solitary voyage up the Hyapurá) numbered 85 species of Mammals, 130 of Amphibia, 350 of Birds, 116 of Fishes, 2700 of Insects, 80 of Arachnoids, and 80 of Crustacea. The species, especially the plants, are represented, many by numerous, and all by well-preserved specimens.

On their return home, the king nominated the travellers Knights of the Order for Civil Merit; and Martius received the appointment of ordinary member of the Academy of Sciences, and second conservator of the Botanic Garden.

In consequence of this expedition, the direction of Martius's future scientific activity was decided. Brazil was thenceforward the country to which he devoted the greater part of it. Before everything else his energy was centred on the flora of Brazil.

The first work made public relative to the Brazilian expedition was the Narrative of the Journey. It appeared in 1823–31, in three quarto volumes, accompanied by an atlas. The compilation of this work was originally intrusted conjointly to both travellers by Maximilian Joseph I.; but Spix did not long survive the completion of the first volume, and so it happened that by far the greater portion of the work proceeded from Martius's unaided pen. Of course in the 'Narrative of Travels' natural products are treated of more or less in detail; but it could not be occupied with the special discussion and elaboration of scientific matter. This was reserved for a separate work, which appeared contemporaneously in a magnificent series of volumes. In the first place Martius undertook only the botanical section, and Spix the zoological; but, on account of the death of the latter in 1826, when he had only worked up the mammals, the birds, and a part of the amphibia, the continuation of this part of the work also fell upon Martius. He acquitted himself of the task in the most satisfactory manner, having secured the assistance of Agassiz, Andreas Wagner, and Pertz, for the actual work, whilst he acted as editor. The publication of the botanical treasures took the form of a selection of the most interesting novelties. The Phanerogamia, or flowering plants, were illustrated in the 'Nova Genera et Species Plantarum Brasiliensium' (3 vols. fol., Munich, 1823–32), and the Cryptogamia in the 'Icones Selectæ Plantarum Cryptogamicarum Brasiliensium,' 1 vol., 1827). The first volume of the former work was prepared by Martius's colleague, Zuccarini, the remainder entirely by Martius, except the chapter in the 'Icones Selectæ' on the internal structure of Tree-Fern stems, from the pen of Hugo von Mohl—a chapter that served to enhance the value of the work in the highest degree. In these publications not only were many new and highly remarkable plants made known (more than 400 species and more than 70 genera), but they were also so fully and lucidly described that botany received an essential enrichment. A practised and quick sight for natural affinities, a happy gift of combination—in short, an essentially "systematic tact," placed Martius in the rank of the first botanists of his time.

A third work was taken in hand by Martius in 1823, and, indeed, the one with which his name will be most closely and enduringly connected. This was the Monograph of Palms, 'Historia Naturalis Palmarum' (3 vols. imp. fol., Munich, 1823–50). The peculiar richness of Brazil in Palms, the beauty of Brazilian forms, and the honour likely to accrue from a new and comprehensive work on this group of plants induced Martius to concentrate his attention upon them immediately after his arrival in Brazil. The fulfilment of this great undertaking cost twenty-eight years of labour and research.

For the matters with which he was less conversant, Martius obtained the cooperation of distinguished colleagues. The chapter on the anatomy of palms was written by H. von Mohl; the fossil palms fell to the share of F. Unger; and Sendtner and A. Braun contributed to the morphology. But by far the greater part came from the pen of Martius himself, notably:—the chapter on the geographical distribution of palms, in which Martius enunciated his views on phyto-geography in general; and the whole of the third volume, containing descriptions of all known palms, systematically arranged, and forming in itself an almost complete monograph of the family. The scientific merit of this work was universally acknowledged. Not only was the special knowledge of palms thereby greatly extended, but the science of botany in general was signally promoted; and it may be said, in the words of a great naturalist, that, "so long as palms are known and palms are grown, the name of Martius will not be forgotten."

The last great work by Martius to which we can refer on this occasion is the 'Flora Brasiliensis.' He had made an attempt, in conjunction with Nees von Esenbeck, to publish such a work on a small scale, but soon abandoned the idea; but in 1839, encouraged by Prince Metternich, he planned a far more ambitious publication, in conjunction with the celebrated Viennese botanist Endlicher. The groundwork of it was to consist of an entirely new and scientific elaboration of all the accessible materials brought together from Brazil, accompanied by numerous plates, thus forming a splendid systematic whole. To comprehend in some degree the magnitude of such an undertaking, it must be remembered that the flora of Brazil numbers almost five times as many species as that of the entire area of Central Europe. It was plain that the carrying out such a work could be accomplished only by the joint labours of many scientific men; and Martius was fortunate enough to obtain the services of the most eminent German and foreign botanists for this purpose. The Emperor Ferdinand I. of Austria, and the Emperor Don Pedro II. of Brazil, and also King Louis of Bavaria took the work under their special patronage. After Endlicher's death in 1849, Fenzl, his successor in office, supplied his place, as co-editor with Martius. At first the work proceeded slowly, on account of the novelty and costliness of the undertaking; but since the year 1850, in consequence of the increased interest taken in it by the Bra-

of all the monographs published, two only we
supplemented nearly all the others by valuab
graphical distribution and the medicinal, te
portance of the several plants. He also cor
teristic plates representing the vegetation (
accompanied by masterly definitions, in elegant
maps of the floral districts, routes of travel,
graphs in the 'Flora Brasiliensis' are esteen
many cases the men who wrote them had
study to the respective groups. The mere en
writings would fill a long space, for there a
works. Among these may be specially menti
nographie und Sprachkunde Braziliens' as evi
in the narrative of his travels, that he devote
Brazil besides the study of its natural history.

Reverting to the main facts of Martius's li
after his return from Brazil, when he was no
the Academy, and second conservator of the
years his position remained unchanged. Wh
Ludwig I. ascended the throne, and the Univ
moved to Munich, he was appointed Professo
tion; and six years later, upon the retireme
received the post of first conservator. With
journeys to England, France, Holland, &c.
interruptedly the duties of both appointme
he was elected Secretary to the Mathemati
the Munich Academy, and continued in the
death.

interest himself in the garden; and his principal occupation thereafter was the publication of the 'Flora Brasiliensis.'

Whatever the world could offer in acknowledgment of his merits Martius received. He was elected member of nearly all the academies and learned bodies in Europe, and kings and emperors honoured him with the most distinguishing marks of favour. His election as Foreign Member of the Royal Society was in 1838. He rejoiced in the esteem and friendship of his most distinguished contemporaries; and plants and animals, and even a mountain (Mount Martius in New Zealand), were named in his honour. But the most gratifying expression of homage and veneration was presented to him on the 30th of March, 1864, the 50th and jubilee anniversary of the day on which he was invested with the degree of Doctor. His friends caused a medal to be struck, with the inscription, "Palmarum patri dant lustra decem tibi palmam. In Palmis resurges." And on the 15th of December, 1868, the remains of the departed were lowered into their last resting-place bedecked with Palm-leaves.

General Thomas Perronet Thompson was born at Hull, on the 15th of March, 1783, the eldest of three sons of Thomas Thompson, Esq., a merchant and banker of that town, and for several years M.P. for Midhurst. His mother was the grand-daughter of the Rev. Vincent Perronet, vicar of Shoreham in Kent, a Swiss Protestant by descent, and one of the few clergymen of the Church of England who joined John Wesley at the commencement of his mission. The youth's early education was received at the Hull grammar school, under the Rev. Joseph Milner, author of the "Ecclesiastical History;" and in October 1798 he entered Queen's College, Cambridge, where in due time he took his B.A. degree with the honour of Seventh Wrangler—no bad start in life for a boy under nineteen.

In 1803 he sailed as a midshipman in the 'Isis' of 50 guns, the flagship of Vice-Admiral (afterwards Lord) Gambier, on the Newfoundland station, and was shortly after put in charge of a West-Indiaman recaptured in the mouth of the Channel, and ordered with other prizes to Newfoundland, where she arrived, the only one that had stuck by her convoy through those foggy latitudes. In the following year he received information of having been elected to a Fellowship at Queen's, "a sort of promotion," he remarks, "which has not often gone along with the rank and dignity of a midshipman." Trafalgar, for which he saw Nelson embark on board the 'Victory' at Portsmouth in September 1805, closed the prospect of active service in the navy, and in 1806 he joined the "old 95th Rifles" as a second Lieutenant, and was among the prisoners captured, together with General Crawford, by the Spaniards in the Convent of San Domingo, in Whitelock's attack on Buenos Ayres, on the 5th July, 1807.

After his liberation and return to England he was sent in the spring of 1808, at the age of twenty-five, as Governor, to Sierra Leone, through the influence of Mr. Wilberforce, an early friend of his father's. Here his

efforts to put down the Slave Trade, which secretly existed under the name of "apprenticeship," marked the man who ever after stuck "closer than a brother" to the dark-skinned races of the earth. "There was no time for hesitation" (he wrote long afterwards). "Of two things he must do one, either withdraw under the pressure of the acknowledged danger of meddling with a dishonest system, or push forward for the present abatement of the mischief, with the almost certainty of being abandoned by the government at home." He chose the latter, and was recalled. When the official documents connected with his proceedings were subsequently required for discussion in Parliament, reply was made that they *could not be found.*

After marrying, in 1811, Anne Elizabeth, daughter of the Rev. Thomas Barker, of York, he joined the 14th Light Dragoons in Spain as Lieutenant, and was present at the actions of Nivelle, Nive, Orthes, and Toulouse, for which he received the Peninsular War-medal with four clasps. During the campaign of 1814 he was taken off regimental duty and attached to the staff of General (afterwards Sir Henry) Fane, of whose kindness and ability be preserved a grateful recollection. "Some old dragoons, discharged on eightpence a day," he writes of himself, "may remember that he was a careful leader of a patrol, a good look-out on picquet, could feel a retiring enemy, and carry off a sentry for proof, as well as another, a great hater of punishment, and a man of very small baggage, consisting of something like a spare shirt and an Arabic grammar."

His youngest brother, Charles, B.A. and Travelling Bachelor of Queen's, and Lieutenant and Captain in the 1st Foot Guards, was killed in action at Biarritz, in the South of France, on the 12th December, 1813; and the survivor, in the irresistible desire of seeing his face once more, had him taken up a few days after and reinterred in the garden of the Mayor of Biarritz, where he rests among the strawberry beds with two other officers of the same regiment, over whose graves the gallant Frenchman has placed a stone with an appropriate French inscription. This striking incident was commemorated by the muse of Amelia Opie, who on this occasion felt as a friend, a relative, and a poet.

Promoted at the peace of 1814, Captain Thompson exchanged into the 17th Light Dragoons, serving in India, where he improved his knowledge of Arabic, which he had begun to study as a subaltern of dragoons in Spain. Arriving at Bombay in 1815, he soon after served in the Pindarry campaign, and had charge of the outposts of the force under Sir William Grant Keir, whom he accompanied in 1819 as Arabic interpreter to the expedition against the Wahabees of the Persian Gulf. In this capacity he assisted at the reduction of Râs al Khyma and other places on the coast, and had a prominent part in negotiating the treaty with the defeated tribes, the most remarkable article in which was the declaring the Slave Trade to be piracy; the earliest declaration to that effect in point of time, though the American one reached England first (see "Exercises," vol. iv. p. 29).

When the main body of the expedition returned to Bombay, he was left in charge of Râs al Khyma with 1100 men, Sepoys with a detachment of European artillery, and was eventually ordered to demolish the town, and withdraw the troops to the island of Kishme on the Persian coast. A misunderstanding having arisen between the Bombay Government and the Arabs of Al Ashkerch on the coast of Omân, who had plundered certain boats, the former sent an order to Captain Thompson to act against them from Kishme in the event of their clearly appearing to be piratical, but to address a letter to them previously to any attack being made. This attempt at negotiation failing through the murder by the hostile tribe of the messenger bearing the letter, the injunction to communicate appeared to be fulfilled and answered. Few will see any alternative but to execute the orders to act; and military men will comprehend the duty of acting with decision under the circumstances which had arisen. Landing at Soor, on the Arabian coast, forty-six English miles from the town of the hostile tribe of Beni Bou Ali, Captain Thompson's small force of 320 Sepoys and four guns was joined by the Imâm of Maskat with 2000 men of his own. The force of the enemy was reported to be 900 bearing arms. On the 9th November, 1820, as the column was toiling through the sand, the hostile sheik, Mohammed Ben Ali, advanced to the attack, sword in hand. What followed is best described in Captain Thompson's own words, written in a private letter the next day:—"The Arabs made the guns the point of attack, and advanced upon them. The instant I heard a shot from the light troops, which showed the Arabs to be in motion, I ordered the Sepoys to charge with the bayonet. Not a man moved forward. I then ordered them to fire. They began a straggling and ineffectual fire, aided by the artillery, the Arabs all the while advancing, brandishing their swords. The Sepoys stood till the Arabs were within fifteen yards, when they turned and ran. I immediately galloped to the point where the Sepoys were least confused, and endeavoured to make them stand; but they fired their musquets in the air and went off. The Imâm's army began a fire of matchlocks, and went off as soon as the Arabs approached. I rode to the Imâm and found him wounded. The people just ran like sheep. I saw some of the European artillerymen, and ran to endeavour to make them stand; but they were too few to do anything."

In the midst of the *mêlée* the writer was struck on the shoulder by a matchlock ball, which passed through coat and shirt, grazing the skin, as he used to say, "like the cut of a whip." The loss of the force in men and guns was most severe, "as must always be the case," he observes, "when troops wait to be attacked with the sword and then give way." The remnants were at length rallied at the town of Beni Bou Hassan, about three miles from the scene of action, and after repulsing a night attack, were led back overland to Maskat by Captain Thompson in person, eight days after the fight.

Another expedition was quickly sent from Bombay. The town was

taken, and the defenders were conveyed as prisoners to Bombay, where, at the meeting between the captive sheik and his original assailant, they agreed heartily on one point, that it would have been a happy thing for both if the letter, lost by the murder of the messenger, had reached its destination.

A court-martial followed, as usually happens in cases of disaster, however undeserved. He was "honourably acquitted" of the two graver charges affecting his personal conduct, and only "found guilty" of so much of the remainder as, in the opinion of the Court, warranted a reprimand for "rashly undertaking the expedition with so small a detachment," and for "having addressed an Official Report to Government, in which, from erroneous conclusions, he unjustly and without foundation ascribed his defeat to the misbehaviour before the enemy of the officers and men under his command." The Report alluded to is in the Supplement to the 'London Gazette' of the 15th and 18th May, 1821 (copied in the 'Times' of the 19th), and may be usefully compared with the finding of the Court.

Their position no doubt was painful, as standing between the incensed Bombay Government and its unsuccessful officer; and it was difficult to reconcile the logic of facts with a natural regard for the wounded feelings of the Company's service. The wars of Affghanistan, Sind, the Punjab, and the Mutiny had not taken place to prove the inferiority of Sepoys to a hardier race; and Indian public opinion was slow to believe anything to their disadvantage. Under these circumstances, and viewed by the light of subsequent experience, the result of the trial was alike honourable to the Court and to the accused; but it nearly broke his heart at the time, and left traces for life on his mind and spirits. Yet it is characteristic of his generous disposition that he retained no prejudice against the Sepoys as a body; and when they were punished, as he thought, with undue severity after the mutiny, his voice and pen were vigorously exerted in their behalf.

In 1822, his regiment being ordered home, Captain Thompson returned with his wife and child by the Red Sea, Cosseir, Thebes, the Nile, Cairo, and Alexandria, through Italy and France. The "overland route" of that day was a very different undertaking from what it is now; and the voyage, performed in country vessels, was protracted by contrary winds, so that more than a year was consumed in reaching England. In 1827 he was promoted to a Majority in the 65th Regiment, then in Ireland, and in 1829 to an unattached Lieutenant-Colonelcy of Infantry. His subsequent promotions bore date, Colonel 1846, Major-General 1854, Lieutenant-General 1860, and General 1868.

And now, after his return to England, commenced the literary and political portion of his life. To the first number of the 'Westminster Review' he furnished the article on the "Instrument of Exchange," the result of eleven years' continuous study. In 1829 he became virtually the sole proprietor; and beginning with the article in support of Catholic Emancipation, of which 40,000 copies were dispersed under the title of the

"Catholic State Waggon," he continued to write at the rate of three or four articles per number, making upwards of a hundred in all, till the Review was transferred in 1836. In 1825 he wrote, to serve the Greek cause, two pamphlets in modern Greek and French on the service of outposts, and on a system of telegraphing for field service. In the following year he published the 'True Theory of Rent,' in support of Adam Smith against Ricardo and others; in which view he was borne out by Say. And in 1827, eleven years before the Anti-Corn Law League was formed, and when he was only a Major in a marching regiment, he published his celebrated 'Catechism on the Corn Laws,' a work which went through many editions, and to which Mr. Cobden always acknowledged the obligations of the Free Trade cause. "For breadth of principle," says a generous political opponent, "humorous and telling illustration, a strong racy Saxon style, there is nothing in Cobbett superior to this little pamphlet."

He was elected a Fellow of the Royal Society in 1828. The following year he wrote "Instructions to my Daughter for Playing on the Enharmonic Guitar; being an attempt to effect the execution of correct harmony, on principles analogous to those of the ancient Enharmonic." He followed this up by the construction on the same principle of an Enharmonic Organ, which was shown at the Great Exhibition of 1851, and "honourably mentioned" in the Reports of the Juries. In 1830 he published 'Geometry without Axioms,' being an endeavour to get rid of Axioms, and particularly to establish the Theory of Parallel Lines without recourse to any principle not founded on previous demonstration. The work went through several editions, with successive amendments, but attracted more attention in France than here; and an accurate translation was published by M. Van Tenac, Professor of Mathematics at the Royal Establishment at Rochefort, and subsequently attached to the Ministry of Marine at Paris. In 1830 he also published a pamphlet on the 'Adjustment of the House of Peers,' which obtained the remarkable compliment of being republished in Cobbett's Register. The same year, at the invitation of Jeremy Bentham, he edited the Tenth Chapter (on military establishments) of his "Constitutional Code," and wrote the notes and "Subsidiary Observations" at the end. In 1834 he published at Paris, in answer to the *Enquête*, or Commercial Inquiry then carried on by the French Government, the "Contre-Enquête; par l'Homme aux Quarante Ecus;" in which the principles of commercial freedom were developed under a familiar form. In 1842 he collected all his writings in six closely printed volumes, under the title of "Exercises, Political, and others,"—a mine of literary, political, military, mathematical, and musical information. This was followed, in 1848, by his 'Catechism on the Currency,' the object of which is to show that the best currency is one of paper, inconvertible, but limited. The views set forth in this publication are embodied in a motion of which Colonel Thompson gave notice in the House

of Commons on the 17th July, 1850, and in a series of twenty-one Resolutions which he moved in the House on the 17th June, 1852, and which were negatived. (3 Hansard, cxxii. 899). His 'Fallacies against the Ballot,' afterwards reprinted as a "Catechism," first appeared in 1855.

At the general election in January 1835, he polled 1386 votes at Preston without being present. In June following he was elected, after a sharp contest, by a majority of five, for Hull, his native place, and was, as he expressed it, "laid down and robbed at the door of the House of Commons" to the amount of £4000 by a petition of which none of the charges were proved before the Committee. While in Parliament, both at that time and afterwards, he maintained a constant correspondence with his constituents, addressing them generally twice a week through local newspapers in short and pithy reports, which were republished under the title of "Letters of a Representative," and "Audi Alteram Partem," this last consisting chiefly of an indignant commentary on the measures taken to suppress the Indian Mutiny. He was also an active promoter of the abolition of corporal punishment in the army, and an opponent of the restriction of marriage with a deceased wife's sister.

Defeated at Maidstone by Mr. Disraeli in 1837, and subsequently at Marylebone, Manchester, and Sunderland, he was elected in 1847 for Bradford, again defeated there in 1852 by six votes, and finally, in 1857, returned without a contest. The dissolution of 1859 closed his career in Parliament, for which he never stood again, although he continued to write in various periodicals on public matters under the signature of "An Old Reformer," and latterly as "A Quondam M.P.," in strenuous defence of the Irish Church. As one of the leaders of the Anti-Corn Law League, the pioneer and fellow-labourer of Cobden and Bright, he will live in the grateful remembrance of many whose cheap loaf is due to the Father of Free Trade, "the literary soldier who wrote the 'Corn Law Catechism.'"

In person he was short, active, and well made, and in middle age might be, as he described himself, "stouter than would become a Light Dragoon;" but he was capable of much fatigue, and insensible to irregularities of hours and seasons. Of his acquirements and ability the foregoing sketch may give some idea; but only those who knew and loved him in private life can tell the depth of his learning, of his goodness, benevolence, and kindness of heart. After a life so long, so varied, and at times so stormy, his end came peacefully at Blackheath, early on the 6th of September, 1869, in the 87th year of his age. He had written letters on various subjects, including his favourite Enharmonic Organ, up to the middle of the day before, in full possession of his mental and bodily faculties, and he may be said to have died, as he lived, pen in hand—"*Qualis ab incepto.*" He was followed by his children and grandchildren to Kensal Green, where he rests not far from an old friend and fellow reformer, Joseph Hume.

In the list of Fellows lost to the Royal Society this year, the name THOMAS GRAHAM stands out with great prominence. Much as he was known, and widely, he was little seen in what may be called the social circles of scientific life; and although we shall miss him and his work in a field which he alone seemed to cultivate and understand, and although his work must greatly influence science, and through it civilization, the public will not observe that any name of importance is absent on great occasions or in large meetings.

He was born in Glasgow, in 1805, Dec. 21st. His father was a merchant of that city, and gave him every opportunity of learning. Having attended the primary school, he went at nine years of age to the Grammar School for Latin and Greek under Dymock, for the usual term of four years, and under the rector, Dr. Chrystal, for the finishing year. Then he went to the University, at an early age certainly, but such is the custom of the place. We hear of no great feats of scholarship in the Grammar School. Graham was too quiet to be brilliant. We hear of diligence, and that he occupied a seat in the first form, and got prizes for lessons as well as a prize every year for not having been absent for one day. The education at this school was sound, and it was not easy for a boy to leave it without some useful knowledge of the languages taught, as well as a very clear idea of the history and progress of the world. In college he remained for seven years before taking his degree of M.A. in 1826. At that time the university was too much of a high school, but it was of course obliged to suit itself to the young who attended. It is clear that Graham had his whole time occupied at the best schools of learning around him, and many must still remember his teacher in chemistry, Dr. Thomas Thomson, and in physics, Professor Meikleham.

His attention seems to have at this time been devoted for the most part to physics and mathematics. When he had taken his degree, he was expected to enter on distinct professional studies. His father had designed him for the church, but his mind was bent on the study of science. A struggle took place between two strong wills, and caused him much misery for many years. Neither was accustomed to speak his mind, otherwise the great respect which each had for the other would have been discovered sooner for the good of both.

This sorrow was softened to Graham by the great tenderness of his mother, to whom he was most devoted, and to whom he told, in a long series of confiding letters from Edinburgh, where he now went to study, all his doings and feelings. In these letters we are led to hope for a very full picture of the early manhood of Graham. It was at this time that he learnt isolation, and satisfied his love of sympathy by writing, so that he acquired a habit which never left him. It is in these notes, reaching up to his last illness, that we must look for all that he thought on scientific and other subjects; and they, with his published papers, will constitute his true autobiography. Few stirring events happened to him; his life, externally

at least, was calm; and equally calm was his mode of thinking, as evinced in his numerous scientific memoirs. He stayed in Edinburgh, studying with Dr. Hope, the well-known Professor of Chemistry, for two years, and there made the acquaintance and enjoyed the friendship of Leslie. Returning to Glasgow, he began to teach mathematics; he then took a room for a chemical laboratory in Portland Street, and in this he gave lectures, for a very short time only, as he was Lecturer in the Mechanics' Institute for the winter 1829–30, having succeeded Dr. Thomas Clark, afterwards Professor in Aberdeen. In the latter year he was transferred to the Andersonian Institution, succeeding Dr. Ure, who went to London.

Graham began in Glasgow with the good wishes of all who knew how laboriously he had studied. He was then 24 years old, but he had been known to scientific men for three years previously; his earliest memoir bearing date 1826, and one on diffusion of gases 1829. He appeared extremely young, and like a boy beginning to teach. He was not fluent in speech; there was a hesitation as if it were difficult to find the proper word, and a quietness of demeanour which (except for the little perceptible nervousness) completely covered the great enthusiasm which kept him at constant work for forty years after that period. He remained in Glasgow lecturing and teaching in the laboratory till 1837, and sending out diligent workers who have since shown themselves vigorous in the regions of science and its application to the Arts. In that year he went to London as Professor at the London University, now University College. He had his residence near it in Torrington Square, which he afterwards left for a house a few doors distant, at 4 Gordon Square, where he ended his days on the 16th of September, 1869. In the College he was held in high regard by his pupils and colleagues. It is true that, as a lecturer, he had to contend with a want of natural fluency and with a feeble utterance; but as he had always the clearest conception of the matter he was treating of, his manner of exposition, even of intricate subjects, was singularly clear and perspicuous, and the instruction imparted was well grounded and thorough, and was pervaded by the same philosophical spirit which guided him in his original investigations.

In 1855 he ceased to be connected with the College, having succeeded Sir John Herschel as Master of the Mint.

An occasional visit to Scotland to see his relations, sometimes to Ballewin at Strathblane, a property which his father had left him, made up his chief journeys, and in later years he was afraid to go except in June or July. His chest, as indeed his whole constitution, was tender, and the accidental exposure to an open window in a warm August day brought on his final attack.

Were it possible to write at present a correct account of Graham's intellectual life, the space required would be too long for this occasion, and a short notice will be given of his principal papers only.

His earliest memoir indicated in the 'Royal Society's Catalogue' is in

Thomson's 'Annals of Philosophy,' 1826, "On the Absorption of Gases by Liquids." He there reasons out the idea that gases are converted into liquids by mixing with or being absorbed by liquids, and that the phenomenon becomes simply that of two liquids mixed together. He concludes that gases may owe their absorption by liquids to their capability of being liquefied, and to the affinities of liquids to which they become in this way exposed. These two properties are considered to be the immediate or proximate causes of the absorbability of gases. It follows "that solutions of gases in liquids are mixtures of a more volatile with a less volatile liquid." He says also that it is a coincidence more than accidental that the gases which yielded to condensation in Mr. Faraday's hands are, generally speaking, of easy absorbability. He objects therefore to Henry's law, that the quantity of gas which water absorbs is directly proportionate to the pressure, because it is not likely that this law would have been spoken of had such gases as muriatic acid been employed, that being very readily absorbed, although there might be an approximation to such a law when the quantity of gas absorbed was inconsiderable.

Graham illustrated the condensation and solution of a vapour in a liquid by supposing steam of the heat of 600° F. to be passed through sulphuric acid of 600° F., when he doubted not immediate absorption and actual solution would take place, as if water and sulphuric acid were mixed at lower temperatures; and yet the steam would be brought into the condition of a liquid which by ordinary cooling would have taken place only after nearly 400° diminution of temperature.

So late as 1866, speaking of the dialytic separation of gases through colloid septa, he is desirous of showing that the flow is not that of diffusion or of effusion, or of transpiration, but that of a liquid absorbed by one side and passed to the other; and in 1868 he illustrates the passage of hydrogen through palladium by saying that it is analogous to liquid diffusion through a colloid. There are forty years between the beginning and end of this train of thought.

This is a fair specimen of Graham's habit of mind and of his perseverance. He seems to have begun life with an intense desire to know the inner structure of matter, stimulated to understand more than the atomic theory could give him, but nevertheless a true student of Dalton. Born in 1805, when Dalton was preparing for the press the ideas he had already given in lectures, he seems to have been destined to begin a new line of work closely allied to that which Dalton had done.

It is almost painful to think of the attempts of mankind to understand why bodies should have a definite composition, and Dalton's simple idea of adding atom to atom not only made it appear possible, but showed why the contrary should be most improbable. Now it seems so simple that some men believe it was scarcely a discovery, whilst every chemist is slavishly bound to it in some form or other, unable either in practice or theory to escape; and this point seems now to be true for all time. Still the simple

axiom, so to speak, was not a science; and as one proposition after another arises out of it, the first idea itself appears grander and grander. Dalton, like Newton, used the term atom as meaning a particle which could not be divided by any force. But there seems no need to say that it could not be divided by the imagination or even by new forces, in which case it becomes the practical as opposed to the theoretical atom. Under the present chemistry there probably exists another which shall deal with the broken atom of our present science; we do not know how many layers there may be under it. It is strange that Dalton's idea was so purely mechanical, although illustrating a purely chemical act: there is no talk of obscure forces; it is a movement like a carpenter's, a fitting of pieces in the manner of a workman. Graham took up the subject in the same spirit, and seems to have during his whole life sought for nothing beyond the knowledge of the constitution of matter, and the mode in which the atoms or molecules move. His favourite word is molecule, not atom; indeed he seems too guarded to use the latter in any case of measurable movement. His destiny was to follow the progress of the molecule, and to show that there were movements in bodies which depended on that aggregration of atoms, whether ultimate atoms or not. Whilst Dalton showed the relative weights of the combining quantities, Graham showed the relative magnitude of groups into which they resolved themselves.

Having discovered that solid bodies could be divided into two classes, colloid and crystalloid, and that the first consist of substances existing in great varieties of conditions, and apt to undergo long and remarkable progressive changes, he seems to have taught us the way to obtain many substances practically new, although nominally such as we have seen. Whilst one, the colloid, has power of motion in itself to a considerable extent, the other, the crystalloid, has power of motion in solutions, so that we are introduced to a series of new forces, the end of which is not in the faintest way foreseen. The door by which we enter these strange regions is found by a series of the most uninviting trials; it seems to have been hidden under the most homely brushwood, and few would think of toiling so long in such a field.

It may be well to go over some of his principal papers, and to observe how constantly he kept to these ideas whilst penetrating further into the subjects.

In 1827 he observed that phosphate of magnesia effloresced very readily; this, he argued, proved a weak affinity for water; if weak, heat ought to destroy it, and so he found that it was thrown down anhydrous on boiling. He argues that it is only the hydrate that is soluble, properly speaking, in other cases also.

This led him, in 1835, to examine the hydrate, when he found that the tendency of phosphate of soda to combine with an additional dose of soda was connected with the existence of closely combined water. This induced him to separate the water of salts into two parts, crystalline and basic, the

first being easily removed, the second requiring more than the boiling-point of water to remove it. This atom of water may be replaced by a salt, forming a class of double salts. Amongst the salts with basic water he puts sulphuric acid as sulphate of water, and an additional atom as equal to the atom of crystallization.

In the same year he speaks of ammonia performing the function of water in compounds of copper. In 1836 he says that his "researches make it probable that the correspondence between water and the magnesian class of oxides extends beyond their character as bases, and that in certain subsalts of the magnesian class of oxides the metallic oxide replaces the water of crystallization of the neutral salt and discharges a function which was thought peculiar to water." The inquiry was extended to the constitution of the phosphoric acids, and the amount of base taken up by them shown to be equivalent to the amount of water in the acid; from this he passed to the arseniates. It is quite evident that he treated water as he treated metallic oxides; indeed he speaks of metallic oxides performing the functions or taking the place of water. It was a distinct recognition of hydrogen as a metal in its place in salts, whilst his latest paper in 1869 endeavours to establish its specific gravity when combined with palladium as an alloy.

This was one of the chains of discovery which, at an earlier period, led to the doctrine of substitution. It is still sound; and although the water does not now hold the same place of honour in the phosphorus acids, the place is held firmly by the hydrogen, which takes its position as a metal. This idea cannot be regarded as originating with Graham; Davy seems to have hinted it, and Dulong made it distinct; but it was Graham whose careful experiments and cautious reasoning gave it consistency and force, although he himself did not actually adopt it in general teaching. Probably nothing tended so much to give hydrogen its present place as the inquiry into the constitution of the phosphates, and his explanation of the monobasic, bibasic, and tribasic acids.

We have the first results of his experiments on diffusion in the Philosophical Magazine for 1829.

After giving various details of experiments he says, "It is evident that the diffusiveness of gases is inversely as some function of their density, apparently the square root of their density." This is the conclusion he arrived at finally.

The separation of gases by simple diffusion is shown to be practicable, and is there illustrated; he mentions it as conceivable "that imperceptible pores and orifices of excessive minuteness may be altogether impassable (by diffusion) by gases of low diffusive power, that is, by dense gases, and passable only by gases of a certain diffusive energy." Here we observe his wonderful caution: he will not say that the atoms or molecules may be too large, he will not say that the gas will not pass, but he says "impassable (by diffusion)."

In his paper read before the Royal Society of Edinburgh, Dec. 19th, 1831, he goes more fully into his favourite subject, beginning with firmness. "It is the object of this paper to establish with numerical exactness the following law of the diffusion of gases." "The diffusion or spontaneous intermixture of two gases in contact is effected by an interchange in the position of independent minute volumes of the gases, which volumes are not necessarily of equal magnitude, being in the case of each gas inversely proportioned to the square root of the density of that gas."

In this paper (1831) he also tried the speed with which gas passed through stucco under pressure.

In 1846 Graham read to this Society a memoir "On the Motion of Gases." There he showed what he called the effusion of gases into a vacuum through a thin plate ($\frac{1}{328}$ of an inch thick), "leaving no doubt of the truth of the general law, that different gases pass through minute apertures into a vacuum in times which are as the square roots of their respective specific gravities, or with velocities which are inversely as the square roots of their specific gravities," and that "the effusion-time of air of different temperatures is proportional to the square root of its density at each temperature." The remarkable results of transpiration are fully developed in his second paper (June 21st, 1849) "On the Motion of Gases."

If a tube of a certain length be used to allow the escape of the gas, the velocities of the gases attain a particular ratio which remains constant with greater lengths and resistances. This ratio depends on a new and peculiar property of gases, which he called Transpiration. He considered that solids have many modes of showing their character, the varieties of structure being endless; but gases could only show theirs in a few directions, and he believed that the ratios of transpirability would have a simplicity comparable to that of the specific gravities, or even the still more simple relations of the combining volumes. As gases, compared with solids, are capable of small variation in physical properties, those characters which do show themselves may well be supposed to be the most deep-seated and fundamental with which matter is endowed. He adds, "It was under this impression that I devoted an amount of time and attention to the determination of this class of numerical constants which might otherwise appear disproportionate to their value and the importance of the subject. As the results, too, were entirely novel, and wholly unprovided for in the received view of the gaseous constitution, of which, indeed, they prove the incompleteness, it was the more necessary to verify each fact with the greatest care." As examples, the density of nitrogen is 14 when hydrogen is taken as 1; but the transpiration velocity of hydrogen is exactly double that of nitrogen. The transpiration time of carbonic acid is inversely proportional to its density, when compared with oxygen. These results he believed to show "the important chemical bearing of gaseous transpirability, and that it emulates a place in science with the doctrines of gaseous densities and combining volumes."

This remarkable property of gases was viewed by Graham as a result of one of the initial endowments of matter, and in this search he showed his usual desire of approaching nearer than we had ever done to the actual constitution of the primitive molecule and practical atom. These inquiries on the motion of gaseous molecules led Graham to look at the motion of bodies in solution or "liquid diffusion." The first paper was read in 1849. He naturally connected this with his earlier experiments on the phosphates, and the amount of water held by them and phosphoric acid; and he termed solubilities of substances weak and strong, as well as great or small. He supposed that this varying strength of solubility might arise from a greater or less diffusive power.

Graham believed liquid diffusion to have an analogy to evaporation; and as the squares of the times of equal diffusion of gases are in the ratio of their densities, so by analogy it might be inferred that the molecules of the several salts, as they exist in solution, possess densities which are to one another as the squares of the times of equal diffusion. He attributed the diffusion of substances in solution, like the transpiration of gases, to a fundamental property of bodies. The pith of the inquiry is thus stated:—"The fact that the relations in diffusion of different substances refer to equal weights of these substances, and not to their atomic weights or equivalents, is one which reaches to the very basis of molecular chemistry. *In liquid diffusion we deal no longer with chemical equivalents or the Daltonian atoms, but with masses even more simply related to each other in weight.* Founding still upon the chemical atoms, we may suppose that they can group together in such numbers as to form new and larger molecules of equal weights for different substances; or, if not of equal weight, of weights which appear to have a simple relation to each other. It is this new class of molecules which appears to play a part in solution and liquid diffusion, and not the atoms of chemical combination." He seems glad to obtain the densities of a new kind of molecules, although knowing no more respecting them. One result is the formation of classes of equidiffusive substances; these, again, led to a new mode of analysis in 1861, and a new division of soluble bodies. It was observed that the power of diffusion of a solution of albumen was very small, 1000 times less than that of common salt; and this fact led to an examination of numerous substances, when it was found that they divide themselves into two classes without respect to organic nature; one is colloid, and includes gelatine and gelatinous silica, alumina, albumen, gums, sugar, starch, and extractive matter. The plastic elements of the animal body are found in this class; and here Graham uses the word (as, indeed, he uses all words) in a very exact sense, that which has the power of forming.

Continually seeking the origin of chemical action, he ascribes to bodies possessing this colloidal condition a dynamical character. They are slow in changing, but seem in a continual change. They possess *energia*, and may be the primary source of the force appearing in the phenomena of

vitality; and to these gradual colloidal changes may be referred the characteristic protraction of chemico-organic changes. These colloid bodies are very easily penetrated by the soluble crystalloid bodies to which they are opposed, and they form a medium also of separation or dialysis.

This process of dialysis has a high value in the explanations it affords, and promises to afford, of many physiological phenomena. But these hitherto unknown and still obscure properties of molecules seem destined to lead us still further on; and by presenting to us a nearer view of the fundamental phenomena, they give us an idea of the enormous magnitude of the structure under which they seem to lie. Graham believed that the rate of diffusion held a place in vital science not unlike the time of the falling of heavy bodies in the physics of gravitation.

In a paper "On the Molecular Mobility of Gases," June 18th, 1863, he further compares several substances as to their facilities for diffusion, and defines clearly the effusion-rate and the transpiration-rate as distinct. A substance suiting the purpose of diffusion is graphite.

He obtained by the graphite diffusiometer a separation of oxygen from the air, making a mixture with 2 per cent. additional of that gas. This led him to try another mode; and by lengthening the surface and tube, he obtained 3½ per cent. more oxygen than in the atmosphere. This process he calls atmolysis. Trying diffusion without an intervening septum, he found that carbonic acid had proceeded half a metre length in seven minutes.

The separation of gases was carried out much further, and described in a paper read June 21st, 1866. Here he begins the use of caoutchouc, having been led to it by the experiments of Dr. Mitchell, of Philadelphia. He found that air drawn through sheet rubber contained as much as 41·8 oxygen, the theoretical speed being 40·46, deduced from the passage of the separate gas. He is desirous of showing that in this case the flow is different from diffusion; it is caused by an absorption of the gases, which are taken into the caoutchouc in a liquid state, and are then given out on the opposite side.

This inquiry naturally led to an examination of the absorption by metals. Deville and Troost had discovered that platinum and iron absorbed gases when hot. Graham found hydrogen to pass through heated platinum 1·1 millim. thick at the rate of 489·2 cub. centimetres per minute on a square metre. Oxygen scarcely passed, and other gases tried did not pass. Wrought platinum took up 5·53 vols. of hydrogen, which, on cooling, were shut up or occluded in the mass. Fused platinum took only 0·171 vol.; hammered platinum 2·28–3·79. Palladium, however, was most remarkable, as it took up 643 vols. of hydrogen; in a later paper the quantity is stated to be 935 vols. These gases were pumped out from the reheated, but could not be removed from the cold metal. Palladium cold, however, was found to take up hydrogen when it was used as the negative pole of a galvanic battery, and spongy palladium, which had absorbed hydrogen when heated, deoxidized some salts in the cold.

Iron manufactured, or from telluric sources, was found to contain carbonic oxide.

Silver, gold, copper, osmium-iridium took up little gas, and antimony no hydrogen. He divides the metals into crystalline and colloid.

His paper of May 16th, 1867, enters more fully into the subject, and shows that meteoric iron contains hydrogen, with the probability, if not certainty, that it was cooled in an atmosphere of that gas. Messrs. Huggins and Miller, as well as Father Secchi, had concluded that hydrogen is one of the gases shown on the spectrum of the fixed stars, and especially mentioned it as found with the unusual increased light of T Coronæ in November 1866. The actual handling of the gas brought from distant space was a strange experimental proof, and was remarkably characteristic of Graham's peculiar inclination to place his work before his thought. He seemed to feel his way by his work. He was not able to impregnate iron with above one volume of hydrogen, whereas the meteoric iron contains at least three. He thinks this shows that it may have been absorbed under pressure.

On May 22nd, 1868, he showed that palladium took up 0·723 per cent. by weight of hydrogen; and he inclines to believe that the passage of the gas through palladium is analogous to liquid diffusion through a colloid.

As a private man, Graham led an uneventful life; but no man has passed through the world more uniformly respected. Too retired, too quiet, his life appears to have a deep tinge of melancholy in it, notwithstanding its eminent success. Very intimate friends he had few out of the circle of the family of brothers and sisters, who were strongly attached to him, and to whom he was much devoted, being himself unmarried.

As a scientific man, his claims were never disputed; he was not called to assert his position, and he remained the undisputed head of his department. He received in early life (1834) the Keith Medal of the Royal Society of Edinburgh, and the Royal Medal of this Society in 1838, and in 1862 the Copley Medal. He was made a Doctor of Civil Law of Oxford, Honorary Member of the Royal Society of Edinburgh, Corresponding Member of the French Institute and of the Academies of Berlin and Munich, and of the National Institute of Washington. His election into the Royal Society was in 1836.

On his appointment to the Mint, Mr. Graham laboured assiduously and successfully in acquiring a thorough knowledge of the technical work and financial relations of his office, and discharged his duties with much energy and judgment. It is known that he brought about various reforms and economies in the working of the establishment; but the service for which he will be chiefly remembered was the introduction of the new bronze coinage, which, besides substituting a more convenient medium of circulation than that in previous use, was attended with a pecuniary profit to the state of very large amount.

An old and valued friend of Graham, Dr. A. W. Hofmann, then intimately associated with him, thus speaks of his administration of the Mint*: —"It would be difficult, within these narrow limits, to convey an adequate notion of the great and manifold activity exercised by Graham in the high office entrusted to him. The new chief of the Mint soon showed a vigilance, a knowledge of the work, an amount of industry and energy, and, when called for, an unsparing severity which astonished all, and especially some of the officials of the establishment. Such requirements had not heretofore been exacted, nor such control exercised. The new Master's love of innovation, and his disturbance of settled arrangements (for in such light was his action viewed), had to be resisted with every effort. The author of this sketch at that time held an office in connexion with the English Mint, and was therefore witness, though from without, of the struggle which Graham had to go through in his new position. It was years before he finally overcame these difficulties, and was enabled to return to his favourite study."

Graham, besides the memoirs mentioned and omitted here, wrote a system of chemistry. The second edition is still valuable; it explained at a very early stage theories which are now general, although it did not actually adopt novel arrangements. The book is a masterpiece of clearness in arrangement and style, but it was written so slowly that the publisher said that to press him was like drawing his blood. The anxiety to be correct was painful. It gives a calmness to all his writing, but really goes too far, as it rather represses the enthusiasm of the reader, and diminishes the force of the words. He may be said never to speculate till he has made experiments; he seems to feel the forms with his fingers before he ventures to describe them; but he reaches to utmost space in this manner more surely than others have done by the boldest imagination. He is, however, capable of the widest generalizations, and these he makes at times with surprising speed.

When speaking of liquids, Graham has been quoted as saying that the rate of diffusion held a place in vital science not unlike the time of falling bodies in the physics of gravitation. We judge of the value of discoveries by the fruit they produce; when we do so, it requires some time to judge fairly. Although there seems to us a boundless region opened up, it is not yet traversed; perhaps he who opened it was best able to see its extent. By his experimental examinations of the motion of molecules, he has made a step which before was left to reason only; and unless he can be shown to have made a mistake, we do right (whilst associating him with other illustrious men of former times) to connect him more closely with his most direct predecessor, Dalton. With such distinguished names, therefore, it seems just that we should, until the world shall teach us better, leave that of Thomas Graham.—R. A. S.

* In a masterly discourse on Graham's life and scientific work. delivered before the German Chemical Society in Berlin, Dec. 11, 1869.

Marie-Jean Pierre Flourens, elected Foreign Member of the Royal Society in 1835, was born at Maureilhan, near Béziers, Department of Hérault, in April 1794. He studied Medicine at Montpellier, where he took his Doctor's degree at the age of nineteen, and in the year following went to Paris. There he made the friendship of various eminent men, —of whom are noted especially Chaptal and Frederick Cuvier,—and devoted himself to the pursuit of Biological science, in which he soon attained reputation as a writer and original inquirer. His earliest and most important labours were directed towards the investigation of the functions of the nervous system, and on his experimental researches and writings on this department of Physiology, which continued afterwards to be his favourite pursuit, his scientific reputation may be said mainly to rest. The first fruits of these researches were made known in three Memoirs presented to the Academy of Sciences of Paris in 1822 and 1823; and subsequently published in an independent work entitled "Recherches Expérimentales sur les propriétés et les fonctions du système nerveux dans les animaux vertèbrés." Paris 1824. Of this a second and greatly extended edition appeared in 1842, containing the substance of Memoirs presented to the Academy since the publication of the first edition, with applications of the author's doctrines to pathology and surgery, researches on the reunion of divided nerves, on the movements of the brain, on the pulsation of arteries, and on the effects of section of the semicircular canals of the ear—also an extension of his previous inquiries to reptiles and fish.

Following in the line of Haller, Zinn, Lorry, Saucerotte, Magendie, and others, Flourens endeavoured, by inflicting injuries experimentally on the encephalon and spinal cord, but especially by studying the effect of removal of definite portions of these organs, to assign the specific offices of the several parts of the cerebrospinal centre; and whatever difference of opinion may prevail as to some of the physiological conclusions at which he arrived, it must be admitted that his experiments, which have for the most part been confirmed by later inquirers, have served in large measure as a basis of subsequent reasoning on the subject.

On his first coming to Paris Flourens became a writer in the 'Revue Encyclopédique,' and contributed articles to the 'Dictionnaire classique d'Histoire Naturelle,' and in the course of his life published numerous papers on different anatomical and physiological subjects, besides that with which he was more enduringly occupied. The titles of these papers (up to 1863) form a goodly array in the Royal Society's Catalogue, to which we refer for details. The more notable of them are on the nutrition and growth of bone, on the structure of the skin and mucous membranes and on the epidermis and its appendages in man and animals, on the mechanism of Rumination, on vomiting in ruminants and its non-occurrence in the Horse, on the vascular connexion of mother and fœtus, &c., while some are on questions of anthropology, comparative psychology, and natural history;

but although these writings are for the most part founded on actual observation and real work, it can scarcely be said that they rise above mediocrity.

Flourens's first and most important memoir on the nervous system became the subject of a commendatory and most instructive report by Baron Cuvier in 1822, whose friendship and favour he thenceforth enjoyed. Cuvier a few years after (in 1828) intrusted Flourens (as his deputy) with the delivery of the lectures on Natural History at the College of France, and two years later appointed him in like manner to give the lectures on Human Anatomy at the 'Jardin du Roi,' in which appointment he was confirmed as Professor in 1832. In 1835 he became Professor in the College of France.

Though rising in fame, it was probably through Cuvier's influence, more immediately, that Flourens was in 1828 elected a Member of the Institute, in succession to Bosc. In 1833 he was appointed one of the perpetual Secretaries, on the retirement of Dulong. In this latter capacity he furnished from time to time eloges of various distinguished members of the Academy deceased during his tenure of office. These productions of his pen, as well as his official reports and his writings generally, were highly esteemed for their literary merit, and no doubt led to the much-coveted distinction he received of being elected into the Académie Française in 1840.

While recognizing M. Flourens's undoubted merits, we are nevertheless constrained to remark that, measured by them, his career as regards both social and scientific distinction, was singularly prosperous. Besides holding a highly influential position in affairs of science, he was elected a Member of the Chamber of Deputies for the Arrondissement of Béziers in 1837, and in 1846 he was created a Peer of France. He still, however, retained his professorship, and suffered neither honours nor revolutions to interrupt his scientific work. In his latter years he was affected with softening of the brain, ending in general paralysis, to which he succumbed, at his country seat Mont Geron, in the Department of Seine at Oise, on the 6th of December 1867. He has left three sons.

Peter Mark Roget, M.D., died on the 12th of September, 1869, in his 91st year. For the last 54 years he had been a Fellow of the Society, and during 21 of these had filled the office of Secretary. The earlier events of his life belong to a former page of the world's history, and to a generation that has passed away. He was born in London, in Broad Street, Soho, on the 18th of January, 1779.

His father, the Rev. John Roget, was a native of Geneva. When about 25 years old he came to reside in London, as Minister of the French Church in Threadneedle Street, founded by Edward VI., and was, two years afterwards, united in marriage with Catherine, only surviving sister of the illustrious Sir Samuel Romilly, then a young man of about 20, between whom

and Mr. Roget a warm friendship had arisen, together with sentiments of the highest mutual esteem. The subject of the present memoir was the only son of this marriage.

He had the misfortune to lose his excellent father very early in life. Not five months after his birth his parents were compelled to leave him in England and hasten to Geneva, on account of Mr. Roget's declining health. Two years afterwards the child was brought to them by his uncle, Mr. Romilly, who was then studying the law; and in two years more, under the same escort, the widowed mother returned to England with her son, and a daughter that had been born a few weeks only before the father's death, which event happened in May 1783.

In the following year the Rogets resided in Kensington Square, in the family of Mr. Chauvet, of Geneva, who kept a private school, where much of the character of parental intimacy was infused into the ordinary relations of teacher and pupil. Here the boy received the rudiments of education; but he was no doubt mainly indebted for his early training to the devoted care of his mother, who was admirably qualified for the task, not only by her mental acquirements, but by a systematic habit of mind, which was inherited by her son in a marked degree. At a very early age, moreover, he began the practice of self-instruction; and having conceived a strong taste for mathematical studies, which he pursued without aid or even encouragement from others, he soon made considerable progress in the elements of science.

Although from time to time returning to Kensington, Mrs. Roget and her two children spent the greater part of the ten years next after her husband's death in short sojourns in the provinces. This was an eventful period of history; and, late in life, Dr. Roget remembered how, during a summer spent at Malvern, the news arrived of the taking of the Bastille, and how while at Dover they used to see the emigrants landing from France and thanking God for their deliverance. In the year 1793, the mother with her two children took up their residence in Edinburgh, where Roget, then 14 years old, was entered at the University, which was then at the height of its fame. During the first two years of his residence there he attended the classes of Humanity (Dr. Hill), Greek (Mr. Dalzell), Chemistry (Dr. Black), Natural Philosophy (Greenfield), and Botany (Dr. Rutherford).

In the summer of 1795 his studies were agreeably varied by a tour in the Highlands, in company with his uncle Romilly and their attached friend Mr. Dumont, well known in connexion with the writings of Bentham, and as author of the 'Souvenirs sur Mirabeau.' To the early guidance of the last-mentioned companion, who took a warm interest in his welfare, and was at especial pains to aid the cultivation of his intellect, Dr. Roget was wont to attribute the enlightened principles which governed his conduct throughout life. He entered the medical school in the ensuing winter, and attended during that and the two following years, the lectures

of Dr. Monro on Anatomy, Drs. Black and Hope on Chemistry, Mr. John Allen on the Animal Economy, Drs. Wilson and Gregory on the Practice of Medicine, Dr. Hamilton on Midwifery, Dr. Home on Materia Medica, Dr. Duncan and Mr. James Russell's Clinical Lectures, and, to his especial interest and delight, those on Moral Philosophy, by Professor Dugald Stewart, from whom he received much kindness, and for whom he always expressed a peculiar regard. While thus diligently engaged, he was in the summer of 1797 prostrated for a time by a severe attack of typhus fever, which he caught in the wards of the Infirmary, and which nearly proved fatal. On the 25th of June, 1798, he took his degree of M.D., being then only 19 years of age. The subject of his thesis, which he dedicated to his uncle Romilly, was "De Chemicæ Affinitatis Legibus." In the same year he wrote a letter to Dr. Beddoes on the non-prevalence of consumption among Butchers, Fishermen, &c., which is published in that writer's 'Essay on the Causes &c. of Pulmonary Consumption,' London, 1799.

After a summer and autumn spent in a trip to the Falls of the Clyde and the English Lakes, and a succession of visits to Dr. Darwin at Derby, Mr. Keir (the Chemist) near Birmingham, Dr. Beddoes at Clifton, and the Marquis of Lansdowne at Bowood, Dr. Roget came to London and continued his professional studies, first at Dr. Willan's Dispensary in Carey Street, and shortly after as a pupil of St. George's Hospital, where he attended Dr. Baillie's lectures in the early part of 1799. In that year he wrote a letter to Davy on the effects of the respiration of the then new gas (oxide of azote, or nitrous oxide), which communication appears in Sir Humphry Davy's 'Researches,' published in 1800.

In October 1800, Dr. Roget spent six weeks with Mr. Jeremy Bentham, who it is understood consulted him at that time upon a scheme which he was concocting for the utilization of the sewage of the metropolis. It may easily be imagined with how great an interest that most remarkable man was regarded by the young physician. In November he began to attend Abernethy's lectures at St. Bartholomew's Hospital.

At the end of the following year he went to Manchester on an engagement to travel with the two sons of Mr. John Philips of that town; and it was while thus employed that he met with an adventure which he ever after regarded as forming the great crisis of his life. The peace of Amiens having thrown open the continent to English tourists, Dr. Roget and his two pupils spent about three months in Paris in the early part of 1802, and thence proceeded in the summer to Geneva, having for their travelling companion thither Mr. Lovell Edgeworth, brother of the authoress Maria Edgeworth. There Dr. Roget found his old friend and preceptor Chauvet, and stayed for some time at his house. The succeeding winter was spent amidst the congenial society of Geneva, and in forming plans for a summer tour in Switzerland. These prospects were, however, suddenly dispelled by the news of the rupture of peaceful relations between England and France, of which country Geneva then formed a part.

This was soon followed by Bonaparte's celebrated order to arrest all the English then in France, and above eighteen years of age. On the first rumour which reached Geneva of this measure on the part of the First Consul, Dr. Roget determined to retire at once with his pupils into Switzerland; but on attempting to do so discovered, to his dismay, that the most active measures had been taken to prevent their escape. Their only course was to submit. The two Philipses were passed as under 18, but Roget was detained prisoner on *parole*.

This state of things lasted for about six weeks; and in the mean time fresh rumours reached Geneva of a contemplated deportation of the English prisoners into the interior.

While Dr. Roget was considering what steps he should take under these circumstances, he suddenly received the startling intelligence that in about a week's time all the English in Geneva were to be sent to Verdun, and that those in Switzerland were already arrested.

Dr. Roget then, as a last resource, applied to the authorities for exemption from arrest, on the ground that he was entitled to the rights of a citizen of Geneva by virtue of his descent from Genevese ancestors. This claim was fortunately admitted; and two days afterwards he saw the rest of the English, with poor Edgeworth among them, set out for Verdun.

Dr. Roget and his young companions now lost no time in leaving the country, but a long detour had still to be made ere they could reach England. The French were rapidly extending their boundaries westward, and the travellers found it necessary to proceed by way of Stuttgart, Frankfort, Leipsick, Potsdam, Berlin, Lubeck, and Husum, whence they sailed for England, reaching Harwich on the 22nd of November. On the way the elder Philips fell ill of a fever, which detained them for two months at Frankfort. Dr. Roget thereby made acquaintance with the celebrated anatomist Soemmering, whom he called to his aid in attending the patient.

In the spring of 1804 he repaired to Edinburgh with the intention of pursuing his studies, but was called from thence to Bath to attend upon the Marquis of Lansdowne, whom he accompanied to Harrogate, and afterwards to Bowood, as his private physician, remaining with him till the 11th of October.

Being then in his 26th year, and desirous of establishing himself in practice, he took up his residence in Manchester, where, on the death of Dr. Percival, there appeared to be an opening in his profession. He was in the same month appointed one of the physicians to the Infirmary, an institution comprising a large Hospital and Dispensary, a Fever House, and a Lunatic Asylum. Dr. Roget is regarded as having, in conjunction with his colleagues, Mr. Gibson and Mr. Hutchinson, laid the foundation of the Medical School in Manchester. In the winter of 1805–6 he gave with them, a joint course of lectures to the pupils of the hospital on Anatomy and Physiology, himself taking the latter subject and delivering eighteen lectures from 29th January to 31st March, 1806.

In the midst of this apparent devotion to pursuits for which he had shown so much natural taste, and which seemed to promise him success in life, he, strange as it may appear, accepted in November 1806 the appointment of private secretary to Lord Howick, then Secretary of State to the Foreign Department, and afterwards Earl Grey. He very soon, however, became conscious of a dislike for the service, and quitted it in a month, returning to Manchester, where he busied himself again in an occupation in which he was destined to rise to eminence. In the lectures he had already delivered he had introduced, in addition to the subject of Human Physiology, as already taught in the school, a comparative survey of the functions of animals, with a view to its forming a useful branch of general knowledge. Encouraged by this first attempt, he commenced, in January 1807, a more popular course on the Physiology of the Animal Kingdom, at the rooms of the Philosophical and Literary Society. This Society numbered among its then members men of high distinction in science and general attainments. In its proceedings Dr. Roget took an active part, and he was one of its Vice-presidents. His lectures, fifteen in number, were delivered in the evenings twice a week, and were well attended and highly esteemed.

Dr. Roget resigned his post at the Infirmary in October 1808, and transferred the scene of his labours to London, where he established himself in the following January in a house in Bernard Street, Russell Square, and on the 3rd of March was admitted Licentiate of the Royal College of Physicians. He lost no time in commencing on a wider field, the career which had been indicated to him by his success in Lancashire. An opportunity soon offered itself. The Russell Literary and Scientific Institution had been opened in the preceding year under the management of a number of distinguished residents in the neighbourhood, including his uncle, then Sir Samuel Romilly, Mr. James Scarlett (afterwards Lord Abinger), Mr. Francis Horner, &c.; and Dr. Roget and Mr. Pond (the Astronomer Royal) were chosen to inaugurate the first lecture season, in the spring of 1809, by the delivery of two courses of twelve afternoon lectures, the one on Animal Physiology, the other on Astronomy. Dr. Roget's course, repeated in the following year, proved to be the first of a long series on his favourite subject, which established for him a high reputation, in a career of more than thirty years' duration as a public lecturer. It will be convenient here to give a list of these courses.

Besides his lectures at Manchester in 1806 and 1807, and at the Russell Institution in 1809 and 1810, he lectured on the same subject at the Royal Institution in the spring of 1812, 1813, 1814, 1822, and 1823; at the London Institution in the spring of 1824; at the two last-named places concurrently in the spring of 1825; in 1826, at the London Institution in the spring, and at the new Medical School in Aldersgate Street in the autumn; and finally at the Royal Institution in the spring of 1835, 1836, and 1837, as the first Fullerian Professor, to which chair he was nomi-

nated by the founder, Mr. John Fuller. In these courses, which numbered from ten to eighteen lectures each, his favourite arrangement of the subject was that which he had adopted in 1807. After a general survey of Cuvier's classification, he would treat, first, of the mechanical functions; secondly, the chemical functions, circulation, respiration, and nutrition; and thirdly those of the nervous system, and the intellectual faculties. At Aldersgate Street he also dealt with the function of reproduction and evolution. Sometimes, however, he divided his subject zoologically, and dealt separately with each class of animals. In his earlier lectures he made Human Physiology the basis of his comparison, which plan he appears to have gradually exchanged for that in which the interest rises as the scheme of nature is reviewed in successive stages from the lower to the higher orders of the animal kingdom. At the Royal Institution in 1825 and 1836, and at the London Institution in 1826, he confined himself to one department, namely, that of the External Senses. The introductory lecture at the Aldersgate School was published by Longman and Co. in 1826, and of many of his lectures he furnished the abstracts published in the Literary Gazette. In all these discourses Dr. Roget kept in view what he had announced at Manchester as his leading object, namely, "to point out, on the plan pursued by Dr. Paley, those proofs of infinite wisdom and benevolence which are displayed in every part of the universe, but which are nowhere so eminently conspicuous as in the structure and economy of the animal creation."

In October 1809 he projected the foundation of the Northern Dispensary, which, with the cooperation of many influential neighbours, was opened in the following June, with Dr. Roget as its physician. The active duties of this office he performed gratuitously for the next eighteen years. In 1825 he was presented with a handsome piece of plate by the patron and governors. In 1810 he began to lecture on the Theory and Practice of Physic at the Theatre of Anatomy, Great Windmill Street, in conjunction with Dr. John Cooke, who two years afterwards resigned him his share of the undertaking. Dr. Roget then delivered two courses a year until 1815. Among his colleagues there, were Sir Benjamin Brodie, Sir Charles Bell, Mr. Brande, and other leading men of science.

In 1811 he was chosen one of the secretaries of the Medical and Chirurgical Society of London, of which he had been one of the earliest promotors in conjunction with his friends Drs. Marcet and Yelloly. In the same year he published a paper in the Medico-Chirurgical Transactions, vol. ii. p. 136, on "A Case of Recovery from the effects of Arsenic, with remarks on a new mode of detecting the presence of this Metal," to which he afterwards added a note in vol. iii. p. 342. In 1812 he wrote an article in the Edinburgh Review, vol. xx. p. 416, on P. Huber's 'Recherches sur les Mœurs des Fourmis Indigènes.' He was also the writer of the Review in vol. xxv. p. 363, of the same author's 'Nouvelles Observations sur les Abeilles.'

While engaged in these avocations, as well as in professional practice, which about the year 1813 began to be considerable, Dr. Roget was not unmindful of his early passion for the exact sciences. Of Mathematics and Natural Philosophy he made a practical study; and in the year 1814 he contrived a sliding-rule so graduated as to be a measure of the powers of numbers, in the same manner as the scale of Gunter, then in common use, was a measure of their ratios. It is a *logo-logarithmic* rule, the slide of which is the common logarithmic scale, while the fixed line is graduated upon the logarithms of logarithms. The consequence is that powers are read as easily as products are on the common rule, and the arrangement is such that high powers of quantities little exceeding unity, so much wanted in compound interest, statistics, &c., are read off on a single setting. His paper thereon, which also describes other ingenious forms of the instrument, was communicated by Dr. Wollaston to the Royal Society, and read on the 17th of November, 1814. It appears in the Philosophical Transactions for 1815, p. 9. It was through this communication that he gained admission to the Society. He was elected Fellow on the 16th of March, and admitted on the 6th of April, 1815. The date of this epoch in his life is noteworthy in relation to the importance which he attached to his deliverance in 1803 from the clutches of Bonaparte. The year in which his paper was read was that of his young friend Edgeworth's release. The manner in which the interval had been employed affords a measure of the loss which he and others would have incurred had he been destined to the like exile.

The next decade in Dr. Roget's life was a period of active industry, passed in the society of many of the most distinguished men of his time. Besides his occupations above specified, he employed his pen in the production of various published writings. In 1815 he contributed a paper to the Medico-Chirurgical Transactions, vol. vii. p. 290, "On a Change in the Colour of the Skin produced by the internal use of Nitrate of Silver." At various periods between 1815 and 1822 he wrote the following treatises and articles in the Supplement to the sixth edition of the 'Encyclopædia Britannica;' viz. Ant, Apiary, Barthez, Beddoes, Bee, Bichat, Brocklesby, Broussonet, Camper, Cranioscopy, Currie, Deaf and Dumb, Kaleidoscope, and Physiology. In 1818 he wrote a letter "On the Kaleidoscope" to the Editors of the 'Annals of Philosophy,' which was published in vol. xi. p. 375. That year was saddened by the melancholy death of his uncle, Sir Samuel Romilly. In July 1820 he was appointed Physician to the Spanish Embassy, which office he retained for many years. In the same year he wrote a letter to Mr. Travers on a voluntary action of the Iris, which was published by Mr. Travers in his work 'On the Diseases of the Eye;' and an Appendix to Larkin's 'Introduction to Solid Geometry and to the Study of Crystallography,' in which Dr. Roget demonstrates the ratios subsisting between the volumes of solids composing the artificial series, together with the various inclinations of their faces. In 1821 he wrote "Observations on

Mr. Perkins's Account of the Compressibility of Water," in the 'Annals of Philosophy,' N. S. vol. i. p. 135; and in 1822, a Biographical Memoir of his valued friend and frequent fellow-worker Dr. Alexander Marcet, in the 'Annals of Biography and Obituary' for 1823. In 1823 he is quoted by Dr. Cooke in his work on Epilepsy, pp. 147, 151, & 215. On the 1st of May in the same year he was appointed Physician to the General Penitentiary, Milbank, in conjunction with Dr. P. M. Latham, on the occasion of an epidemic dysentery which prevailed among the prisoners. His labours there occupied him for fifteen months; and in 1824 appeared the joint report of himself and his colleague to the House of Commons. In the autumn of that year was another great epoch of his life, namely that of his marriage.

Dr. Roget married Miss Hobson, only daughter of Mr. Jonathan Hobson, a merchant of Liverpool. The union was one of unclouded happiness, but of short duration. Mrs. Roget, after giving birth to a daughter and a son, died in the spring of 1833, of a lingering disease.

On the 9th of December, 1824, another mathematical paper of Dr. Roget's was read at the Royal Society; this is entitled "An Explanation of an Optical Deception in the appearance of the Spokes of a Wheel seen through vertical apertures" (Phil. Trans. for 1825, p. 131); and in 1825 he wrote another in the 'Scientific Gazette,' Nov. 5 and 12, "On an apparent violation of the Law of Continuity." In 1826, besides his "Introductory Lecture," there appeared an article by him on Electro-Magnetism in the 'Quarterly Review,' being a review of Ampère's 'Recueil d'Observations Electro-Dynamiques,' and Barlow's 'Essay on Magnetic Attractions;' and an article "On the Quarantine Laws," in the Parliamentary Review, p. 785.

In 1827 he received a commission, with Mr. Telford and Mr. Brande, under the Great Seal, to inquire into the supply of water to the Metropolis, which resulted in the publication of their report in 1828.

He began at this time the composition of the series of treatises in the 'Library of Useful Knowledge' on "Electricity, Galvanism, Magnetism and Electro-Magnetism." They were issued in parts in the years 1827 1829, and 1831, and were afterwards published together in one volume. These treatises were held in considerable repute at the time they were published, and that on Electricity reached a second edition. He also wrote the article Galvanism in the 'Encyclopædia Metropolitana.' His connexion with the Society for the Diffusion of Useful Knowledge, for which the above treatises were written, is thus referred to by Mr. Charles Knight, in his 'Passages of a Working Life':—"Amongst the founders of this Society, Dr. Roget was, from his accepted high reputation, the most eminent of its men of science. He was a vigilant attendant on its committees; a vigilant corrector of its proofs. Of most winning manners, he was as beloved as he was respected. . . . . Upon all questions of Physiology, Peter

Mark Roget and Charles Bell are the great authorities in the Useful Knowledge Society."

On the 30th of November, 1827, Dr. Roget was elected Secretary of the Royal Society, on the retirement of Mr. (afterwards Sir John) Herschel.

In company with his friend Dr. Bostock in 1828, and again with Mrs. Roget in 1830, shortly after the "three days" revolution of that year, he revisited Paris, with what recollections it is easy to imagine. In the former year a distinguished friend of Dr. Roget's died, for whom he had a peculiar veneration, namely Dr. Wollaston, and in 1829 he lost his early adviser, Dumont. In 1829 and 1830 he occupied the chair as President of the Medical and Chirurgical Society, of which he had ceased to be secretary three years before. In 1828 he wrote an article in the 'Parliamentary Review' on "Pauper Lunatics;" and in 1831 he contributed to the 'Journal of the Royal Institution of Great Britain,' vol. i. p. 311, a paper "On the Geometric Properties of the Magnetic Curve, with an account of an Instrument for its mechanical description." In June in the same year he was elected, *speciali gratia*, Fellow of the Royal College of Physicians, and in the following May he read the 'Gulstonian Lectures,' for which he selected as his subject "The Laws of Sensation and Perception." An abstract of them, written by him, appeared in the 'London Medical Gazette' for that month. In 1832 he furnished the articles AGE and ASPHYXIA to the 'Cyclopædia of Practical Medicine,' published under the superintendence of his friend Dr. Tweedie. Before this time, but at what precise date has not been ascertained, he had written the following articles in 'Rees's Cyclopædia; viz. SWEATING SICKNESS, SYMPTOM, SYNOCHA, SYNOCHUS, TABES, and TETANUS.

The year 1833 was one of great trial. The absorbing grief which he suffered on the death of his wife made other sorrows seem light; but several family afflictions occurred at the same time. Dr. Roget sought to divert his mind in the society of his scientific friends, and in the interest he could still take in scientific pursuits. He attended the Meeting at Cambridge of the British Association, which had been founded two years before at York. These gatherings were always a source of great delight and interest to him, and he was a frequent attendant at them for the next thirty years. At one or more of the earlier Meetings he filled the chair of the Physiological Section.

Fortunately also he was at this time engaged in an undertaking with which his memory will ever be associated, namely, the production of one of the 'Bridgewater Treatises.' The most important department of that celebrated series, executed under the will of the Earl of Bridgewater, to illustrate "the Power, Wisdom, and Goodness of God, as manifested in the Creation," had been assigned to Dr. Roget by the late President of the Royal Society, Mr. Davies Gilbert, to whom the selection was *ex officio* intrusted. His treatise, which forms the fifth of the series and is in two

volumes, has for its title "Animal and Vegetable Physiology considered with reference to Natural Theology." As the testator had specified "the effect of digestion, and thereby of conversion," and "the construction of the hand of man," as instances of the "reasonable arguments" whereby the collective work was to be illustrated, were departments assigned to other writers, to be dealt with in separate treatises; but with these exceptions, Dr. Roget's province was to embrace nearly the whole of the physiology of the two kingdoms of nature. Of the manner in which he performed the task it is needless to speak at length here. As the prescribed purpose of the work was the very object which he had set before him and retained in view ever since his early efforts at Manchester, he naturally adopted the arrangement which he had found best in his lectures, and he endeavoured to embody in the form of a compendium so much of the argument and such of the illustrations as were adapted to every class of readers, and might form a useful introduction to the study of Natural History. Since the time of Roget the science of Comparative Anatomy has entered upon new phases, then but dimly foreshadowed; but still his Bridgewater Treatise may be read with profit and delight by all, on account of the deeply interesting nature of the subject, the lucidity of the argument, the variety of illustration, the pure religious tone which pervades it, and the admirable style in which it is composed. Of the work in its original form three editions were published—the first and second in 1834, and the third in 1840; and two years before his death, the author superintended the passing through the press of a fourth edition, published by Messrs. Bell and Daldy. Dr. Roget was the last survivor of the authors of the Bridgewater Treatises.

In the years 1834 and 1835 he held the office of Censor to the Royal College of Physicians. In 1837 and subsequent years he took an active part in the establishment of the University of London, of the Senate of which he remained a member until his death; and in June 1839 he was appointed Examiner in Physiology and Comparative Anatomy, which office he held for some years. In 1838 his pen was again employed by the editors of the 'Encyclopædia Britannica,' to the seventh edition of which he contributed the articles BANKS (Sir Joseph), PHRENOLOGY, and PHYSIOLOGY. The last two were published separately in two volumes. That on Phrenology was, with some additions, a reprint of the article "Cranioscopy" belonging to the former edition. In the original article he had expressed his strong dissent from the conclusions of the phrenologists, and this had given rise to answers on their part, particularly by Mr. George Combe, in "Essays on Phrenology," Edinb. 1819, and by Dr. Andrew Combe in the 'Phrenological Journal.' To these criticisms, which were at least a tribute to the ability with which he had argued his case, Dr. Roget took this opportunity to reply. The article "Physiology" was an entirely new and comprehensive treatise, describing the various functions of the animal economy. That which he had before written under the same title was confined to the philosophical department of the subject, containing an analytical investigation

of the several classes of vital powers and their mutual relations, and pointing out the necessity of distinguishing, more carefully than had been done by early inquirers, between physical and final causes. In 1844 Dr. Roget again travelled abroad, revisited Geneva, and took the opportunity of attending the Meeting of the Italian Scientific Association held that year at Milan.

In other respects his public life during his long term of office as Secretary was intimately associated with the annals of the Royal Society. In the course of that time changes had been introduced which rendered the duties of the Senior Secretary exceedingly laborious. Not only did the task of editing the Proceedings both of the Society and of the Council fall to his share, but also that of making and preparing for publication the Abstracts of Papers read. This labour was performed by Dr. Roget from November 1827 until his retirement from office in 1848. Ever devoted to the interests of the Society and to his own important duties, he at times found his position one of great delicacy, and his name had occasionally to appear in the front rank of polemical warfare. On these occasions he maintained his position by firmness and forbearance, while sometimes smarting under undeserved attacks. On the 7th of November, 1836, a vote of thanks was accorded to him by the Society.

On retiring from office, although in his seventieth year, he at once embarked in a laborious undertaking which he had projected many years before. As long ago as the year 1805 he had formed, for his own use in literary composition, a small classed catalogue of words, which vocabulary had often proved of great service to him in his writings. This he determined to expand into a work of general utility, and after three or four years of labour he published, as its result, the now well-known 'Thesaurus of English Words and Phrases, classified and arranged so as to facilitate the expression of ideas, and assist in Literary Composition.' The appreciation which the work has received may be inferred from the fact that it has reached a twenty-eighth edition. It first appeared in 1852, and after running through two editions, was reduced to a more portable form, and stereotyped. Not the least remarkable part of the work is the arrangement, at once philosophical and practical, of the Ideas, which forms the basis of the classification. The book may be shortly described as the converse of an ordinary dictionary. A dictionary sets forth the idea belonging to a given expression; the 'Thesaurus' supplies the expression to a given idea. A French 'Thesaurus,' in which the author, Mr. T. Robertson, adopted in all its details Dr. Roget's arrangement, was published in Paris in 1859, with the title "Dictionnaire Idéologique. Recueil des Mots, des Phrases, des Idiotismes, et des Proverbes de la Langue Française classés selon l'ordre des Idées." An imitation of the original work, but omitting all the phrases from the classification, was also produced in America.

With the publication of the 'Thesaurus,' Dr. Roget's public career may be said to have closed. He had for many years retired from practice, and

now an increasing deafness excluded him to a great extent from the pleasures of social intercourse. This infirmity, which was almost the only sign of his great age, he bore with patience and resignation. He had survived all the friends of his youth and most of those of his manhood; but he was happy in the possession of mental resources, which enabled him to indulge, even to his last day, the habits of constant industry which he had acquired when a boy. As with advancing age he became less inclined for, and at last less capable of, deep study or long-sustained thought, his employments partook more of the nature of pastimes; but both in his selection and pursuit of these there might still be traced the scientific turn of thought and philosophical love of method which had characterized the main achievements of his life. The engines he had forged to store his mind were now employed to entertain his leisure. One example of this was very remarkable. At an early period (May 1811) he had attended a course of lectures by the celebrated Feinaigle, of whose system of Mnemonics he made constant use throughout life. This system comprises two main devices for a *memoria technica*. The one is designed to record chronological facts, or indeed any facts connected with the ordinary succession of numbers, the other to impress separate figures upon the memory. The first object is accomplished by a methodical arrangement in well-known portions of space, such as the sides of a room; the second by means of words which can be easily remembered, and of which the letters are made to represent figures under a conventional rule of interpretation. Of both these sources Dr. Roget had availed himself largely. He had applied the former to a great variety of subjects. For him familiar places had thus an additional interest. The houses he had lived in, and those of friends whom he had visited, the old rooms of the Royal Society at Somerset House, and of various Institutions which he frequented, were pictured to his mind's eye as peopled with an infinitude of facts, and teeming with varied information. The chronicle of universal history, the measurement of earth and sky, the epochs of his life and of those of his contemporaries, the sources of his income, the categories of his 'Thesaurus,' the general arrangement of human knowledge, were all recorded in this manner on the tablets of his memory. Of the second device, he had also made extensive application. Logarithms, approximations to surds, and various ratios in common use in computation were set by him to doggrel phrases, which it was an amusement to repeat to himself as he walked; and he would sometimes astonish his acquaintance by accurately stating the value of π to forty or fifty places of decimals.

He was always fond of mechanical contrivances, and at one period spent much time and labour in attempts to construct a calculating machine. This design he abandoned on seeing the beautiful engine of Professor Scheutz, of which he at once admitted the superiority. He also made some progress towards the invention of a delicate balance, in which, to lessen the effect of friction, the fulcrum was to be within a small barrel floating on water.

Scientific toys were a source of great delight to him, as has been already seen in his study of the kaleidoscope. Late in life he amused himself much with conversions of plane rectilinear figures of equal areas—cutting out pieces of card so that they could be differently put together to prove the equality, and thereby forming a series of geometrical recreations. He was also fond of exercising his ingenuity in the construction as well as the solution of chess problems, of which he formed a large collection. Some of those figured in the 'Illustrated London News' were of his invention. To assist persons interested in the same pursuit, he contrived and published (in 1845) a pocket chess-board, in which small men of card, lying flat on the board, were kept in place by the insertion of their bases into folds or pockets in the chequered paper which composed it. In the 'London and Edinburgh Philosophical Magazine' for April 1840, there is a "Description of a Method," which he invented, "of moving the Knight over every square of the chess-board without going twice over any one; commencing at a given square and ending at any other given square of a different colour." The complete solution of this problem, which had engaged the attention of some of the most eminent mathematicians, including Euler and De Moivre, had never been effected before.

During his latest years, which were passed in complete retirement, he derived great amusement from light epigrammatic literature, still collecting and classifying according to his wont; but his chief resource was in the pages of his 'Thesaurus,' to which he continued to make additions until the last day of his life. His constant spirit of cheerfulness as his end drew nigh, and the kindness and benevolence which endeared him to all around, befitted a life spent in accordance with his belief that the purpose of our existence here on earth is that of doing good to our fellow creatures in furtherance of God's everlasting glory. After spending last summer at Malvern in the enjoyment of his usual health, his strength failed him during the great heat of August, and on the 12th of September he expired, peacefully and without suffering, from the natural decay of that vital power the mysterious working of which he had so laboured to illustrate.

Dr. Roget was also Consulting Physician to Queen Charlotte's Lying-in Hospital; Hon. Member of the College of Physicians in Ireland; Fellow of the Astronomical, Entomological, Geographical, Geological, and Zoological Societies, and the Society of Arts; Member of the Royal Institution; Hon. Member of the Institute of Civil Engineers, of the College of Physicians in Ireland, and of the Literary and Philosophical Societies of Liverpool, Bristol, Quebec, New York, Haarlem, Turin, Stockholm, and Athens. He was also a member of a variety of social scientific clubs, among others, an Honorary Member of the Smeatonian Society of Civil Engineers; and he was at the time of his death the "father" of the Royal Society Club, of which he had been a member since 1827.